



A NEW METHOD OF TREATING THE DYSPEPSIA

BY DR. J. C. WATSON, M.D., OF THE UNIVERSITY OF PENNSYLVANIA
AND OF THE HOSPITAL FOR THE INSANE, PHILADELPHIA
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H. T. BROWN, M. D.
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S. FOSTER HAVEN, M. D., *of Boston, Massachusetts.*
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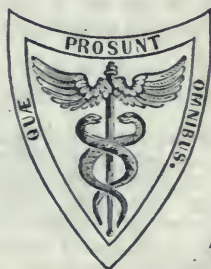
EDITED BY

ISAAC HAYS, M.D.,

FELLOW OF THE PHILADELPHIA COLLEGE OF PHYSICIANS; MEMBER OF THE
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The following works have been received :—

Lettsomian Lectures on Insanity. By FORBES WINSLOW, M. D., D. C. L., &c. &c. London: John Churchill, 1854. (From the Author.)

The Diagnosis of Surgical Cancer (the Liston Prize Essay for 1854). By JOHN ZACHARIAH LAURENCE, Surgeon to the Northern and Farringdon Dispensaries, &c. London: John Churchill, 1855. (From the Author.)

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Surgical Reports and Miscellaneous Papers on Medical Subjects. By GEO. HAYWARD, M. D., President of the Massachusetts Medical Society, Fellow of the American Academy of Arts and Sciences, etc. Boston, 1855. (From the Author.)

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Experimental and Clinical Researches on the Physiology and Pathology of the Spinal Cord, and some other parts of the Nervous Centres. By E. BROWN-SEQUARD, M. D., of Paris, Professor of the Institutes of Medicine and of Medical Jurisprudence in the Medical College of Virginia, etc. Richmond, 1855. (From the Author.)

Case of Penetrating Gunshot Wound of the Heart: Life Protracted for Eleven Days. Bullet found embedded and encysted in the substance of the heart. By J. M. CARNOCHAN, M. D., Surgeon in Chief to the State Hospital, etc. New York, 1855. (From the Author.)

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Improved Forceps for Hare-Lip Operation, and Fatal Result of an Operation for the Removal of a Tumour from the Neck. By ALDEN MARCH, M. D. (From the Author.)

On Adipocire and its Formation. By CHARLES M. WETHERILL, Ph. D., M. D. Read January 19, 1855. Extracted from the Transactions of the American Philosophical Society, Vol. XI. Philadelphia, 1855. (From the Author.)

Portrait of Austin Flint, M. D., editor of the Buffalo Medical Journal from 1845 to 1855. Published by his friends of the city of Buffalo. (From the Publishers.)

A Lecture on the Effects of Alcoholic Drinks on the Human System, and the Duties of Medical Men in relation thereto. Delivered in the Lecture-room of the Rush Medical College on Christmas day, 1854. By N. S. DAVIS, M. D., Professor of Pathology, Practice of Medicine, and Clinical Medicine. Chicago, 1855. (From the Author.)

An Address, delivered at the First Medical Commencement of the Massachusetts Medical College, March 7, 1855. By D. HUMPHREYS STORER, M. D., Professor of Obstetrics and Medical Jurisprudence. Boston, 1855. (From the Author.)

Introductory Lecture to the Class of 1855 of the Medical Department of the University of Missouri. By JOHN R. ALLEN, M. D., Professor of Obstetrics and Diseases of Women and Children. And the Valedictory Address to the Graduates. By PAYTON SPENCE, M. D., Professor of Physiology and Comparative Anatomy. Published by the Class. St. Louis, 1855.

Needs, Duties, and Privileges of the Medical Profession. By JOHN M'CALL, M. D., Utica, N. Y. Read before the New York State Medical Society at its Annual Meeting in the city of Albany, February, 1855.

Statistics of Injuries of the Heart. Observations on Wounds of the Heart and their Relations to Forensic Medicine, with a table of forty-two recorded cases. By SAMUEL S. PURPLE, M. D. New York: S. S. & W. Wood, 1855. (From the Author.)

Annual Announcement and Catalogue of the Medical Department of the University of Nashville. Sessions 1855-'56. Nashville, 1855.

University of New York, Medical Department. Annual Announcement of Lectures. Session 1855-'56. With catalogues of the class of 1854-'55, and of the Graduates of the College. New York, 1855.

Annual Reports of the Officers of the New Jersey State Lunatic Asylum at Trenton for the Years 1852 and 1854. Trenton, 1853 and 1855. (From H. A. Buttolph, M. D.)

Thirty-Eighth Annual Report of the State of the Asylum for the Relief of

Persons Deprived of the Use of their Reason. Philadelphia, 1855. (From Dr. Worthington.)

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Annual Report of the Trustees of the State Library of the State of New York. Transmitted to the Legislature March 1, 1855. Albany, 1855. (From Dr. T. R. Beck.)

Eighth Annual Report of the Regents of the University of the State of New York on the condition of the State Cabinet of Natural History, and of the Historical and Antiquarian Collection annexed thereto. Made to the Senate Jan. 15, 1855. Albany, 1855. (From Dr. T. R. Beck.)

The following Journals have been received in exchange:—

Archives d'Ophthalmologie. Par M. A. JAMAIN. November, December, 1854.

Revue de Thérapie Médico-Chirurgicale. Par A. MARTIN-LAUZER. Jan., Feb., March, April, 1855.

Annales Médico-Psychologiques. Par MM. les Docteurs BAILLARGER, CERISE, et MOREAU. Jan., 1855.

Le Moniteur des Hôpitaux. Par M. H. DE CASTELNAU. March, April, 1855.

Gazette Médicale de Paris. March, 1855.

Medical Times and Gazette. March, April, 1855.

Dublin Medical Press. March, April, 1855.

The Dublin Hospital Gazette. March, April, 1855.

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. April, 1855.

The Glasgow Medical Journal. April, 1855.

Monthly Journal of Medicine. March, April, May, 1855.

The Edinburgh Medical and Surgical Journal. April, 1855.

The British and Foreign Medico-Chirurgical Review. April, 1855.

Association Medical Journal. Edited by JOHN ROSE CORMACK. March, April, May, 1855.

The Dublin Quarterly Journal of Medical Science. May, 1855.

The Medical Chronicle or Montreal Monthly Journal of Medicine and Surgery. Edited by WILLIAM WRIGHT, M. D., and D. C. MCCALLUM, M. D. April, May, June, 1855.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D., and HENRY ROSSIGNOL, M. D. February, April, May, June, 1855.

New York Dental Recorder. Edited by CHARLES W. BAILLARD, D. D. S. March, April, 1855.

Memphis Medical Recorder. Edited by A. P. MERRILL, M. D. March, May, 1855.

Iowa Medical Journal. Conducted by the Faculty of the Iowa University. February, March, April, May, 1855.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. April, May, June, 1855.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. April, 1855.

The American Medical Gazette. Edited by D. M. REESE, M. D. April, May, June, 1855.

The Western Journal of Medicine and Surgery. Edited by LUNS福德 P. YANDELL, M. D. March, April, 1855.

The Boston Medical and Surgical Journal. Edited by Drs. SMITH, MORLAND, and MINOR. April, May, June, 1855.

The American Medical Monthly. Edited by EDWARD H. PARKER, M. D. April, May, June, 1855.

Buffalo Medical Journal. Edited by AUSTIN FLINT, M. D., and SANFORD B. HUNT, M. D. April, May, June, 1855.

The Stethoscope. Edited by Drs. ATKINSON, HAXALL, BOLTIN, LEWIS, MERRITT, and WILSON. April, May, June, 1855.

The Western Lancet. Edited by T. WOOD, M.D. April, May, 1855.

New York Medical Times. Edited by H. D. BULKLEY, M.D., and J. G. ADAMS, M.D. April, May, June, 1855.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M.D., and E. ANDREWS, M.D. April, May, June, 1855.

The Virginia Medical and Surgical Journal. Edited by Drs. McCaw, PEEBLES, and OTIS. April, May, June, 1855.

The New Jersey Medical Reporter. Edited by S. W. BUTLER, M.D. April, May, June, 1855.

The New Hampshire Journal of Medicine. Edited by GEORGE H. HUBBARD, M.D. April, May, June, 1855.

The American Journal of Dental Science. Edited by CHAPIN A. HARRIS, M.D., D. D. S., and A. SNOWDEN PIGGOT, M.D. April, 1855.

The North Western Medical and Surgical Journal. Edited by N. S. DAVIS, M.D., and H. A. JOHNSON, M.D. April, May, June, 1855.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M.D., and PAUL F. EVE, M.D. April, May, June, 1855.

The Medical Counsellor. Edited by R. HILLS, M.D. April, May, June, 1855.

New Orleans Medical News and Hospital Gazette. Edited by Drs. CHOPPIN, BEARD, and VANCE. April, May, June, 1855.

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American Journal of Pharmacy. Edited by WM. PROCTER, Jr. May, 1855.

The New York Journal of Medicine and the Collateral Sciences. Edited by SAMUEL PURPLE, M.D., and STEPHEN SMITH, M.D. May, 1855.

Charleston Medical Journal. Edited by C. HAPPOLDT, M.D., assisted by D. J. CAIN, M.D., and F. PEYRE PORCHER, M.D. May, 1855.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M.D. May, 1855.

The American Journal of Science and Arts. Conducted by Professors B. SILLIMAN, B. SILLIMAN, Jr., and J. D. DANA, in connection with Professors GRAY and AGASSIZ, and Dr. GIBBS. May, 1855.


The New Orleans Medical and Surgical Journal. Edited by BENNET DOWLER, M.D. May, 1855.

St. Louis Medical and Surgical Journal. Edited by M. L. LINTON, M.D., W. M. MCPHEETERS, M.D., JOHN S. MOORE, M.D., and J. R. ALLEN, M.D. May, 1855.

The Southern Journal of Medical and Physical Sciences. Edited by Drs. W. P. JONES, B. WOOD, J. W. KING, and R. O. CURREY. January, March, 1855.

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2. The Proceedings and Reports of the Medical and Chirurgical Faculty of Maryland, Incorporated in 1799, at its Annual Session, in June, and Special Session in September, 1854, in the City of Baltimore. 8vo. p. 82.	
3. The Transactions of the New York Academy of Medicine. Vol. i. Part II. Containing Hospital Hygiene Illustrated, by John H. Griscom, M.D. 8vo. pp. 12.	
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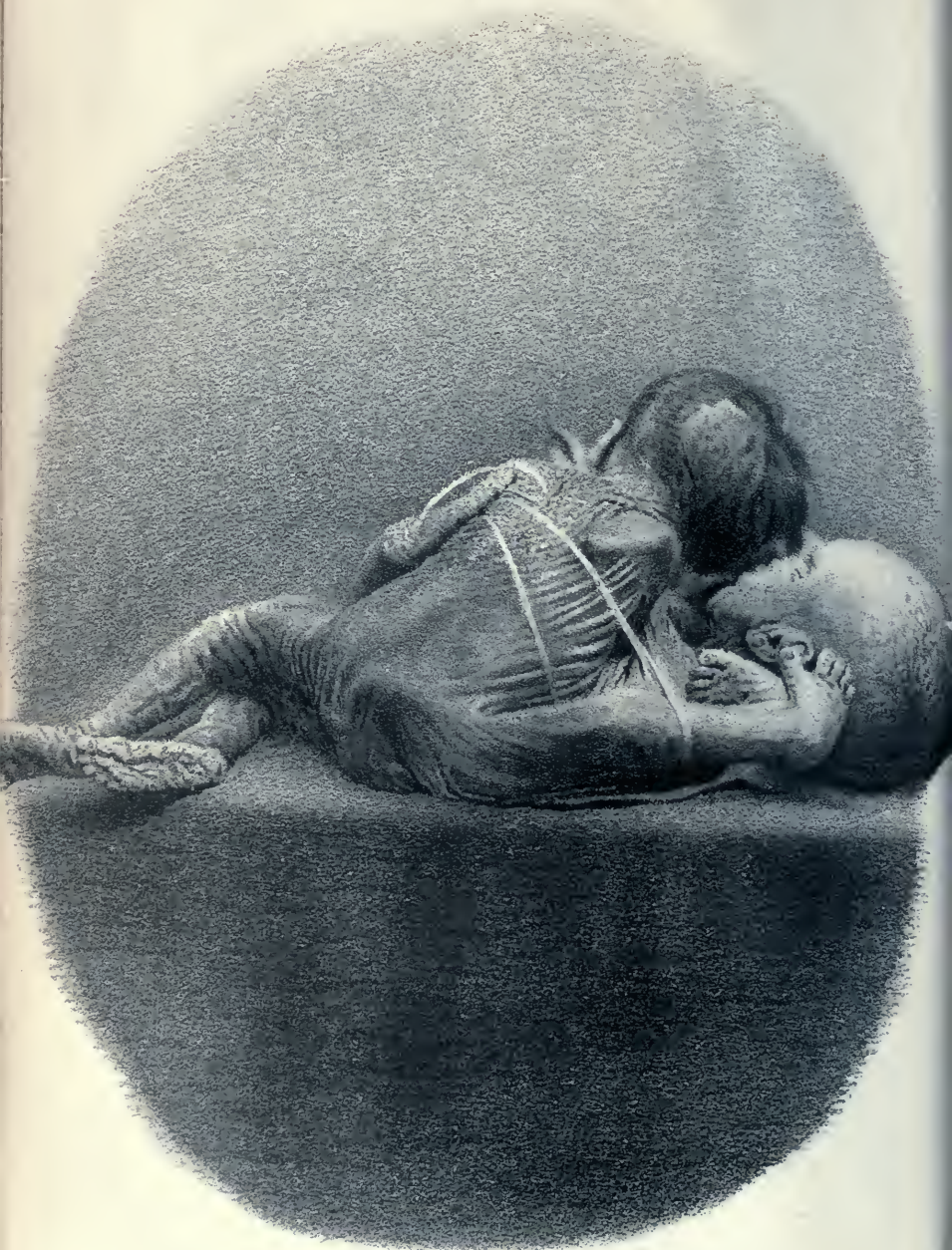
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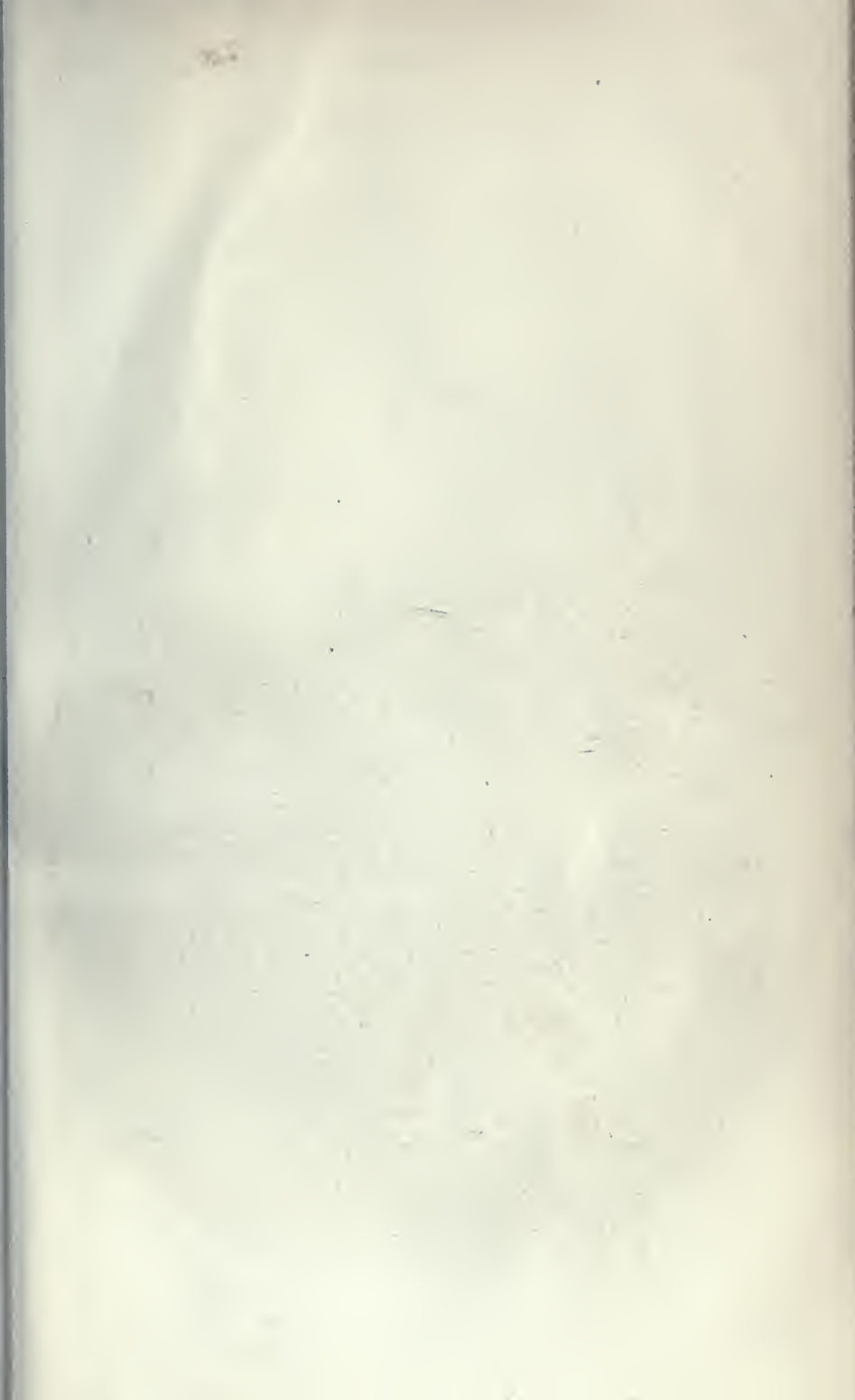
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T Sinclair's lith Phil

BÆRSTLER'S CASE OF COMPLEX HEPATODERM MONSTER.





T Sinclair's lith. Phil

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FOR JULY 1855.

ART. I.—*Case of a Monstrous Birth—described in a Letter from Dr. G. W. BÖRSTLER, of Lancaster, Ohio, to Dr. MEIGS, of Philadelphia—with remarks on the Case.* By C. D. MEIGS, M. D., Professor of Midwifery, &c., in Jefferson Medical College. (With two lithograph illustrations from Daguerreotypes by Root.)

CASE—*Dear Sir:* In compliance with my promise, I now give you a history of the highly interesting, and, as far as I have had the means of reference, altogether *unique* monstrosity.

On Tuesday evening, 16th January, I visited the wife of one of our most respectable citizens, who was in her ninth labour; the uterine contractions being strong, and at fifteen minutes' intervals. At 7 o'clock I touched, and found the os high up the sacrum, and dilated to a dime's size. 7½ o'clock, touched again—os dilating rapidly; 8 o'clock revealed it fully dilated, and the membranes slightly protruding.

This examination gave the impression that a knee or elbow presented, and I at once placed the mother in the usual position for turning. I could not satisfy my mind as to the presenting part, and therefore ruptured the membranes, and then felt what I believed a groin, the hip resting in the left iliac region, the body oblique across the maternal pelvis. Into this supposed groin I hooked two fingers, and, with some little traction, brought the breech into the proper axis of the pelvis, congratulating myself on finding only an ordinary breech presentation, which would give me little trouble.

Waiting to see whether the foetus would now advance, and finding this not so, after some very vigorous throes, I again introduced the left hand, for the purpose of bringing down the inferior extremities; and, feeling the supposed groin, I slipped up my hand to the knee, and in attempting to flex it I was foiled. Continuing the examination of this limb, I felt the spinal column along, and parallel to it. Reflection could bring to my mind no such presentation, and, partly withdrawing the hand to search for the

foetal pelvis, I found the two lower extremities flexed on its abdomen, both of which I brought down, unfortunately separating the left femur from its epiphysis.

Having now three legs to deal with, the idea of twins was naturally fixed in my mind. The uterine contractions being vigorous, and the tractile force applied greater than in ordinary breech cases, I believed the leg of the other child to be the impeding obstacle, and at once introduced the hand to correct the entangled posture of the *twins*; carefully feeling this third leg, I found it firmly united to the body of the child whose two legs I had brought down, and arising from the dorsum of the ilium. A malformation was now evident; its character unknown. Hooking two fingers above the attachment of this back leg, and grasping the two other thighs with the right hand, I acted with considerable force on these two points, and found the pelvis and body of the child gradually advancing through the inferior strait, until it reached the junction of two bodies united by an intergrowth of the ensiform cartilages. At this point I met the greatest resistance during the labour. This being happily overcome, I found two bodies diverging from each other, which, however, passed the strait without much trouble; and, by passing the hand up, I felt two apparently well-formed heads, each as large as that of an ordinary child. To my mind it seemed impossible that those two heads could pass the strait at once, and I looked to the probable necessity of lessening one or both; but, to my great gratification, they passed without any extraordinary efforts being called for. This could not have resulted, but for the *good fortune* of the head of the smaller child resting upon the neck and cheek, up to the malar bone, of the larger. Thus was completed a delivery of a most extraordinary *lusus naturæ*, to the well-being of the mother and children. The mother's recovery was rapid; the children are living—one vigorous, the other feeble; both take food, and urinate and defecate. There was one ordinary placenta, and one cord.

Description of the children.—The heads, faces, arms and hands, and the chest down to the ensiform cartilages, are well developed and in proper proportions. From the junction downwards, one body, its anterior surface from side to side broader than in an ordinary child; the abdominal muscles well developed; the umbilicus *in situ*; two spinal columns, perfect; the coccyx of each terminating on each side of the anus, and about half an inch from it; two pelves *united*, the left one belonging to the larger child, encroaching upon and lessening that of the right side. The sex female; one vulva and one anus, the opening of the latter not larger than an ordinary rye straw—the two lower extremities are of proper size and proportions. Upon the back, and from the dorsum of the two ossa ilia arises a leg running up between the two spines and the two inner shoulders, and terminating in a right and left foot joined at the heel. This is, in truth, a double leg, enveloped in one common integument, having two femurs, two tibias, and two fibulas; the leg admits of partially moving it directly backwards to a distance of several

inches from the back of the child. I have no doubt that nature designed one of these legs for each child—for, when the right child is awake, it moves its lower extremity, as also the left foot of the double leg; when asleep, the limb is quiet; and so with the other child. Tickling the sole of either foot, movement follows in the limb; but I have not perceived motion in the opposite leg to the one tickled. The two bodies have each its heart and lungs; puerile respiration distinct; the action of each heart easily felt; though the first and second sounds cannot be distinguished, owing to the rapid systolic and diastolic movements. So far as I have been able to detect, I believe the respiratory and cardiac action of both children to be synchronous, though the harmony of the former is interrupted by the crying of either child. I have frequently observed that, when one of the children is nursing and the other crying, the latter falls asleep; so frequent is this occurrence, that I hold it to be the rule, and it shows the strong sympathetic relations between these *two distinct human beings joined in one*. What the union of organs may be in the abdominal cavity we, of course, have no means of knowing; all reasoning thereon must be hypothetical. That the viscera are duplicated, we think probable. In the feeble child there existed the anomaly of the *frænum lingue* arising from the dorsum of the tongue, about half an inch from the tip, and inserted into the palatine arch, of course rendering the tongue useless in sucking.

The following admeasurements were made, in the presence, and by the aid of Drs. Effinger, Davis, and Wagenhals. Owing to the want of calipers, the *circumference* was taken :—

Occipito-frontal, $13\frac{1}{4}$ inches in the larger, $12\frac{1}{2}$ inches in the smaller; biparietal, 6 inches— $5\frac{1}{2}$ inches. This is half the circumference, having measured from one parietal protuberance to the opposite, across the vault. Mental, $4\frac{3}{4}$ inches— $3\frac{3}{4}$ inches; occip. bregmatic, $13\frac{1}{2}$ inches— $13\frac{1}{4}$ inches; shoulders, 10 inches—9 inches; junction of bodies, $16\frac{1}{4}$ inches; pelvic, $13\frac{1}{2}$; length from head to foot, $17\frac{1}{2}$ inches—17 inches; weight, 10 pounds.

These are the facts in this, to me, exceedingly interesting case, and I very much regret the absence of men fully capable to examine it in all the lights which our science demands from its votaries. In the development of embryos in utero, certain starting points are always *essential* to the production of particular organs or limbs; this law is clearly manifested in the production of the double leg—for in the fusion of the two pelves, the *acetabular portion* is preserved, and hence the development of this extra limb.

A letter from Prof. Meigs refers me to the great work of M. Serres, on Monstrosities; to it I have no access, but from the Professor's letter I learn that a specimen, approaching mine, was born in Sardinia a few years ago, and brought to Paris for public exhibition. The union in this case was in the two pelves, and called, by M. Serres, a *hepatodym*. In mine, the union is from the xiphoid cartilages down; and, hence, I ask if it is not properly called an *hepato-pelvidym*?

Death of the children.—On Tuesday morning, 20th February, the mother observed the larger twin gasp a few times, and at fifteen minutes before 6 o'clock breathing ceased. At 8½ o'clock I saw it, and could not detect any respiratory act, nor pulsations of heart or arteries. Drs. Effinger and Wagenhals pointed out to me the apparent movements of the carotids. We, however, all became satisfied that *these movements depended upon the circulation of the smaller child.* The asphyxiated condition continued for *four and a half hours; no respiration; no pulse; the capillaries of the skin filled with dark blood, giving a purple hue to its entire body, and beautifully showing the demarcation between the asphyxiated and living child.* This line from the junction down, extended half an inch to the right side of the umbilicus. A violent effort in coughing by the smaller child communicated a shock to the larger, convulsive movements of its limbs followed, and it uttered a few feeble cries, when it again relapsed into its condition of suspended animation, and so remained till 5 o'clock in the evening, when the smaller child died. One gasp in the larger, and in ten seconds it slept with its sister. Thus these children fortunately survived their unfortunate union only five weeks.

Accompanying this history you will have daguerreotypes of the front, back, and side views. These pictures are *fac similes* of the original, and executed by my fellow-townsmen, V. M. Griswold, in his usual style of accuracy and beautiful finish.

You have seen the children, and I ask you to add to the above any remarks and reflections you may think proper.

Believe me to be truly your obliged friend,

To Prof. C. D. MEIGS.

G. W. BÆRSTLER, M. D.

I cannot allow the present opportunity to pass without expressing my thanks to Dr. Bærstler for his liberal kindness in sending me this valuable specimen as an addition to my collection in the Museum of Jefferson College. I am indebted to his kindness also for the privilege of presenting the accompanying lithographs, taken from crayon-size daguerreotypes by Mr. Root, an inspection of which may show the reader the external peculiarities of the specimen. It is with Dr. B.'s consent that I have caused them to be preserved entire, instead of destroying them by a dissection, which appeared to me wholly unnecessary, since the children so closely resemble in every particular those that were dissected by the late Mr. Serres, and delineated by him in the Atlas to his work entitled *Anatomie Transcendente*.

The birth of so curious a specimen of humanity as that described above by Dr. Bærstler would hardly fail in any part of the world to excite feelings of astonishment among the people, because it would present an appearance of mystery not to be accounted for, except by persons duly instructed as to the causes of such strange combinations. No surprise, however, would be experienced were it understood that the development of such a monster is due to the regular operation of natural laws, which, by a mere accident, have been diverted from a normal way of action.

Most persons, unacquainted with the doctrines of embryology, would regard such a creature as a double child, and such an opinion would tend to increase the astonishment of the observer, since any excess in the production of parts or organs is known to be in absolute violation of the powerful and invariable law of species or specific forms. But there would be no surprise if it were everywhere understood that it is not a double child that has been thus unfortunately born, and that the specimen represents two distinct individuals, who have become more or less completely fused or welded together in the womb, each depriving its twin sister of certain parts of its organization.

In regard to the specimen now under consideration, it requires only a casual glance to see that the child on the right has contributed one-half of the abdomen, and the left child the other half. The two livers have become compounded into one mass, constituting what Mr. Serres, in his anatomy of *Ritta-Christina*, denominates a complex hepatodym; a word derived from *hepar* and *didymus*.

These complex hepatodyms always result from a lateral union of twins in the womb, according to Mr. Serres, because in every instance of such lateral union the livers must become compounded together into one mass composed of those two individual organs. In case, however, the union of the twins should take place in front, that is, face to face, or nearly so, then the result is the production of an acomplex hepatodym, a case in which the livers remain separate.

The complex hepatodym gives two heads, and, for the most part, two trunks with four arms; whereas the acomplex hepatodym gives one compound head, one thorax, two arms, four bellies, and four lower extremities. In either of these cases there must be combination or fusion of some portion of the bowels of the children, owing probably to changes or obstruction, at a very early stage, in the arrangement and distribution of the omphalo-mesenteric bloodvessels, and the excretory tubes of the two umbilical vesicles.

When the union takes place laterally, we therefore should find a pelvidym; and when it takes place in front, we ought to have a cephalodym, because the complex hepatodym implies that the two pelves are compounded more or less completely, and the heads separate, while the acomplex hepatodym implies that the two pelves shall remain separate, but the thoraces and heads compounded together under one form.

The lateral union is sometimes so complete as to present the appearance of a very perfect single child, having two well-formed heads growing from common and perfect shoulders. To show that this is the case sometimes, I annex a wood-cut taken from a life-size portrait by Mr. Jno. Neagle, an account of which I have already published in my work called *Obstetrics, the Science and the Art*, at p. 220.

An inspection of the wood-cut shows that while there was a single pelvis, abdomen, and thorax, there were also two sets of cervical vertebræ, and probably two sets of at least a large moiety of the dorsal range of vertebræ. It

is not known how far downwards in the ranges the duality of the rachis proceeds before it acquires its condition of unity. It is at least evident that the duality is lost in unity in the lumbar series, and that only a single sacrum could exist in the specimen. Indeed, I have sometimes thought that it might have been possible, in this very case, that the union should have gone even further, so as, in fact, to have con-founded the two heads into one, leaving the spectator in doubt and uncertainty as to whether he had before him one child, or two half children united into one. It is, however, to be believed that Mr. Serres's law as to the complex and the acomplex hepatodym is invariable, and that in every instance we can have only a pelvidym on the one hand or a cephalodym on the other.



To look at my wood cut, or to examine the lithograph, it becomes clear that the left child is an absolute identity, while the right one equally retains its unity or individuality. The left child, which has its head and left arm entire and perfect (*vide* wood-cut), has also preserved half of its thorax, abdomen, and pelvis; *i. e.*, it has the left half of its sacrum and the whole of its left os innominatum. The conservation of its left os innominatum implies also the conservation of its left acetabulum, which may be fancifully compared to a bud from which to develop its left leg and foot, which are accordingly seen to be in all respects complete. This left child lost its right os innominatum, and, having no right acetabulum, could by no means develop a right leg, which is a dependency of that acetabulum. The same observations apply to the child on the right, *mutatis mutandis*.

When the embryos came together in mutual contact they became fused or welded by a process not dissimilar, I suppose, from that called by gardeners *enarching*, or budding, or grafting. The dermal surfaces are not formed at a very embryonal age, and contact leads to fusion or blending as certainly as contact of freshly incised surfaces leads to surgical union by the first intention. And this is what has happened in Dr. Boerstler's specimen, in the case represented by the wood-cut, in *Ritta-Christina*, and probably in all the cephalodyms and pelvidyms that have appeared to astonish the world. Individuality or personal identity is not lost in such cases, for the specific or generic law operates invariably; and, though each of the children has lost something, it is a loss by default only, and is never a case of excess.

In Dr. Pfeiffer's case (*vide* wood-cut) there were, doubtless, two independent stomachs. The duodenum of each child was separate, as well as the jejunum

and part of the ilium, but one-half of the lower part of the ilium, the cæcum, colon, and rectum was lost by each embryo respectively, each of which contributes its half of the cæcum, colon, and rectum, one ovary, half the bladder of urine, half a clitoris, nymphæ, vagina, and womb, the remainder being mutually lost by default. As there is only one navel and umbilical cord, it is true to say that each child owns one-half, as well as half of the allantois, one umbilical artery, and probably one-half of the umbilical vein. The confounding together of some part of the small intestines, which must lose their duality, is an unavoidable result of the early confusion of the omphalomesenteric system, and the tubes of the vesicles or vitellary sacs.

To touch and tickle the left foot, while the child was sleeping, would be to waken it and cause it to smile, without exciting any reflex or other sensibility in its twin brother.

When Ritti's foot was tickled, it did not wake the sleeping twin Christina, and *vice versa*. Dr. Boerstler observed the same phenomenon in his cases.

There appears to me, upon the whole, to be less matter for surprise at witnessing the birth of such twins than there is in the reflection, how rare are the cases. Thus, Madame Lachapelle, who had the superintendence of more than thirty-seven thousand births while she was at the head of the Maternité at Paris, found only 444 cases of twins in that great number, which affords about one twin case to every one hundred births. I have no means of knowing how many cases of monstrous births like that under consideration may occur in the course of a year: I should suppose, however, that three or four such instances would hardly be met with, in every year, in the whole globe; though it is computed that 24,000,000 of children are born annually. And this leads to the inquiry, What is the cause of such abnormalities, or what is the reason why they do not oftener occur?

If a gardener, in performing the act of enarching, or budding, or grafting, should place a bit of membrane betwixt the surfaces to be united, he would wholly fail of success. In like manner, if two or more children should be carried in the same womb, there would be interposed betwixt their growing surfaces a membrane produced by each embryo, the amnios; the interposition of which would effectually debar the possibility of such unfortunate conjunction of the fruit of the womb. It may, therefore, be safely assumed that, wherever the amniotic sac is in a normal state, such unhappy duplicity as in the case before us is rendered impossible; and, consequently, that all such instances of conjunction are events that depend upon rupture or incomplete development of the amnion. I have purposely left out of question the state of the chorion, because I do well know that, although in twin pregnancies there are sometimes found two absolutely distinct chorions and placentas, there are very numerous instances in which both of the children, and sometimes three, are contained within a single chorion, on the outer surface of which is developed a single placenta. This observation I have made so carefully, and so many different times, that I rest assured of the correctness of the opinion. But in all these

examples of a single chorion covering twins or triplets, I have invariably found a distinct amniotic sac enveloping each child. Indeed, I cannot well conceive of an embryo in the womb as existing without its amnion, save in the cases where it is absent by default of arrest of development, or by accidental rupture.

I fully conclude, therefore, that the cause giving rise to the hepatodym-monster, whether it be complex or acomplex, and no matter how or where the union is effected, is a dependency on the accidental imperfection of the amniotic sacs of the twins; and that it is necessary that both of the amnions should fail, since the existence of a single one must prove an effectual bar against the union of the embryos.

It is well known that in the complex hepatodym or pelvidym, although there is never more than one navel and cord, there may or may not be developed, in addition to two perfect lower extremities, other rudimental limbs, more or less complete—a case that is strictly dependent upon the amount of injury done to or of interference with the development of the acetabulum.

Dr. Boerstler describes the singular leg that is seen in my lithograph lying upon the back of the conjoined twins. This member is inclosed in a common tegument, and rises up along the posterior surface of the hepatodym, and loses its apparent, but unreal unity, only at the tarsus, where the two imperfect organs acquire a visible duality. To show that they belong each to a distinct individual, Dr. Boerstler made the experiment of tickling the foot, which the reader has noticed in the doctor's interesting letter. To examine the specimen, or even to scan these lithographs, one may readily solve the problem as to the apparent unity of these two hind legs, as I may call them. In *Ritta-Christina*, and in Dr. Pfeiffer's case (*vide* wood-cut), the fusion of the pelves in their lateral union was so complete as to make it impossible to produce more than two legs. (See M. Serres, Plate XI. of the skeleton of *Ritta-Christina*.) That gentlemen gives us, in Plate XX., the skeletons of united twins, having a perfect right and left leg to each, an abortive left leg for the right child, and only a rudimental right femur for the left child.

Let the reader look again at my lithograph to see that, in the lateral or pelvidym union of these two children, there was a tendency of the embryos to approach each other face to face, and that the contact took place in such a way as to cause fusion and partial arrest of development of the horizontal or body-portions of the left and right pubes. Now, as the pubis contributes one-fifth of the acetabulum, and the ischium and ilium each two-fifths, it has happened that the pubal fifths of the two acetabula were arrested and lost, and that the ischial and iliac two-fifths of each party being left unruined, there sprung from them the curious back leg which thus lost, apparently, its duality, which has been unavoidably inclosed within a common integument, and was compelled to take its place upwards along the dorsal region.

If the turn on their axes had been a backward instead of a forward turn, it is probable that this compound and imperfect leg would have been developed in a direction upwards, and in front of the common abdomen.

In regard to the obstetric peculiarities of such cases, it may not be inapposite for me to remark that Dr. Boerstler ought to be esteemed fortunate in having so rare a case as this to offer itself in a pelvic presentation. It is, probably, to that circumstance that he owes the opportunity he enjoyed of seeing the twins survive their birth so long.

If they had offered to him a cephalic presentation, it is scarcely to be believed he could have extricated them alive. The wood-cut, so often referred to, may be examined with a view to discover what must be the effect of a head-presentation in such a case.

If we suppose the left head to present at the superior strait, and to be driven out in front of the genital fissure, we cannot conceive of the right one as immediately following it. On the contrary, the right one must remain above the brim, and the left one in front of the pubis; the two heads being separated by the symphysis pubis. They may be likened to two sleeve buttons, each buttoned in a wristband. The inner or upper head cannot escape, because the left shoulder occupies the opening, for the escape of the left head has converted the vertex case into one of what is called shoulder-presentation. It is impossible to perform the operation for turning here, because that operation consists in carrying the presenting part up towards the fundus, and bringing another part, as the feet or breech, to present. Clearly, the now presenting part, which is a left-shoulder, cannot be carried up towards the fundus, for it is chained to the strait by the head already born, which firmly holds the shoulder to its presentation.

Pfeiffer's case was of this kind, and the children could not possibly be born save by the act of what is technically denominated evolution of the foetus, usually spoken of as spontaneous evolution in shoulder-presentations.

I shall not presume here to explain what is meant by spontaneous evolution of the foetus, for it is to be supposed that every reader of this journal is perfectly acquainted with a subject so trite. It is highly proper for me, however, to say that, in the obstetric operation, the attending practitioner may always, however embarrassing the case, confide in his ability to deliver the woman, provided he will first reflect upon the mechanism of these spontaneous evolution cases; and, if compelled to do so, resort to a compulsory process of evolution, in which he shall cause the foetus to undergo the same changes as those that take place in unassisted deliveries of shoulder-presentations.

ART. II.—*A Sketch of the Medical Topography of Fort Reading, California.*

By J. F. HAMMOND, M. D., Assistant Surgeon U. S. Army. (Communicated by THOS. LAWSON, M. D., Surgeon General U. S. A.)

As it is the impression in this department generally that this post will be broken up next spring, I take advantage of a quiet time to write you a hasty sketch of its medical topography.

The meteorological tables, and the others relating to the sick, are condensed from the retained copies of the Meteorological Register and the Register and Quarterly Reports of the Sick and Wounded in the Post Hospital; for which observations up to the first of December last, credit is due to the medical officers who preceded me here.

This military post was established in May, 1852, and was named Fort Reading, after an old settler from one of the Eastern States, who still lives in the neighbourhood.

It is in latitude $40^{\circ} 28' 22''$ N.; longitude $122^{\circ} 7'$ W. from Greenwich. In altitude about 800 feet above the level of the sea. The country around is, in a general view, an irregular prairie, bounded on the east by a range of mountains—Lassen's Mountains—running north and south, sixty miles distant, and one-fourth of the way to the range of the Sierra Nevada; on the west by the Coast Range, twenty-five miles distant; on the north by Shasta Butte, ninety miles distant, which appears to spread out east and west, and connect with Lassen's and the Coast Range; and on the south it is continuous with a plain that follows the course of the Sacramento River. The average height of these mountains is five or six thousand feet above the sea. The highest point of Lassen's Range—Lassen's Butte—which is nearly due east from the fort, must be near 10,000 feet above the sea. Snow is seen on it at all seasons. Shasta Butte, which is immediately under the 122d parallel, is, according to the most reliable map of Northern California, 16,600 feet above the sea. It is covered perpetually with snow for some distance below its summit. Three-fourths of the horizon is, in winter, bounded by a line of snow.

This large basin is the northern part of the valley of the Sacramento River. Its inclination is to the south. It is furrowed everywhere by ravines and valleys, in which, from October to May, are flowing mountain torrents. The last vary in size from the smallest to a quarter of a mile broad, and have an average depth of many feet. The remainder of the year the ravines contain strings of pools of stagnant water, alternating with sandy or rocky beds, exposed to the rays of the sun. The prairie is studded here and there with *mots* of white oak; and white oak, the nut pine, and willows, with long grass and dense undergrowth, skirt the water-courses. Elsewhere the country is bare of everything that would intercept the winds. The population, which is composed of emigrants and a few small bands of roving Indians, is, except in the mines, very sparse. The cultivation of the soil is of such little extent that it cannot effect in any degree its healthfulness.

The post is a mile east of the Sacramento River, in the valley of a mountain stream called Cow Creek. It is on the brink of the bank, twenty feet high, which limits the bed of the creek on its western side, in a fine grove of large white oaks entirely free from undergrowth. The quarters for the officers and men are of adobes, one story high, of ample dimensions, well ventilated except beneath the floors, which are of planks laid directly on the ground; they are well lighted, and the distances between the buildings spacious, so as

to allow free access and circulation of fresh air. The whole is thoroughly policed. The creek, generally confined between its banks, sometimes overflows just above the fort, and forms a stream fifteen yards or more in width, that runs between the buildings occupied as quarters, and leaves a damp and fetid bed. It was found necessary to build a permanent bridge over the bed of this cut off, for such an emergency. During the winter and spring the creek has, for a mile or two above its mouth, an average depth of fifteen feet, and forty yards breadth. Its bed, however, will average one hundred yards wide. Just now the stream is, at the fort, 350 yards wide. In the summer and fall, the average depth is not more than four feet, its breadth is contracted two-thirds, and much of its bed, which consists of boulders, gravel and sand, or argillo-arenaceous layers containing a large amount of fragments and particles of pumice and vegetable remains, is exposed to the sun. When the stream is swollen by the rains, it has a yellowish-gray turbidness; but the deposit is slight, from the short duration of the floods and its rapid current; as it subsides, it has the opalescent hue of streams of limestone water; and when at its summer height it is limpid, and delicious to drink. Its course is S. S. W. It runs a little nearest the eastern border of its valley, and empties into the Sacramento River, a mile below the fort.

The valley of the creek is a mile wide. Its bottom is a plain of argillaceous soil mixed with gravel and vegetable remains from grasses and weeds. It produces a very full crop of grasses and weeds, and great variety of flowers. It is exceedingly retentive of moisture, and is, during the rainy season, so boggy that it is dangerous for cattle to get off the roads. The subsoil is a light yellow sand, more or less compact. The valley has the appearance of a valley from disruption, and presents, on both sides, abrupt escarpments fifty feet high. The continuity of the latter is much interrupted by ravines formed by the rains, and the angles have been rounded, and the subsoil concealed by various meteorological causes. From the tops of the escarpments on both sides of the valley, extend plains to the foot of other precipices, or are interrupted by ravines and valleys. The surface of these plains are red clay mixed with gravel and the remains of crops of grass, and they are covered with rolled flints. In some places, over an extensive tract, they are formed into numerous depressions, which contain puddles or ponds of water during the rainy season. For some time after that season, even a man cannot walk on them without miring. In summer they are dry and somewhat spongy, and water is found a short distance beneath the surface. The substrata are clay mixed with rolled flints, gravel, yellow sand, all of various degrees of compacture, but friable, extending to an indefinite depth. Cryptogamic plants are observed during most of the year. After a rain many mushrooms spring up; mould fixes on the ground in numerous places, and seizes upon all fresh deposits of animal or vegetable matter.

The following meteorological tables are from observations taken at this post, from June, 1852, to January, 1855, inclusive—a period of thirty-two

months. The thermometer, which is detached, is perfectly protected from reflection.

The first is the mean temperature monthly :—

The Monthly Mean of Four Observations per Day of the Temperature.

Month.	Thermometer detached.	Month.	Thermometer detached.
1852.		1853.	
June . . .	78.80°	November . .	53.12°
July . . .	80.37	December . .	46.20
August . . .	78.75		
September . .	72.33	1854.	
October . .	59.76	January . .	49.00
November . .	51.77	February . .	47.26
December . .	43.42	March . . .	53.14
		April . . .	60.11
1853.		May . . .	67.40
January . .	48.03	June . . .	72.11
February . .	49.37	July . . .	83.74
March . . .	52.84	August . . .	79.62
April . . .	58.77	September . .	72.67
May . . .	70.96	October . .	61.90
June . . .	78.50	November . .	53.60
July . . .	83.75	December . .	44.14
August . . .	78.76		
September . .	70.69	1855.	
October . .	65.06	January . .	44.20

The next is the monthly mean of all the observations taken daily at sunrise and 3 P. M. ; and the mean monthly range of the thermometer.

Mean Maximum and Minimum Temperature, and Mean Range of the Thermometer, Monthly.

Month.	Min.	Max.	Range.	Month.	Min.	Max.	Range.
1852.				1853.			
June . . .	62.26°	96.26°	34.00°	November . .	46.50°	61.73	15.23°
July . . .	62.35	97.16	34.81	December . .	39.41	54.90	15.50
August . . .	62.25	95.70	33.45				
September . .	55.53	90.73	35.20	1854.			
October . .	45.80	75.00	29.20	January . .	34.38	48.41	14.03
November . .	45.86	59.76	13.90	February . .	41.46	55.03	13.57
December . .	40.19	47.93	7.54	March . . .	45.02	61.17	16.15
				April . . .	51.22	69.17	15.96
1853.				May . . .	65.32	77.45	18.13
January . .	42.06	57.35	15.29	June . . .	60.08	83.08	23.00
February . .	39.89	60.35	20.46	July . . .	69.28	99.21	29.93
March . . .	42.64	63.93	21.29	August . . .	64.05	95.01	30.96
April . . .	49.20	69.00	19.80	September . .	56.19	87.26	31.07
May . . .	55.64	76.45	20.81	October . .	54.06	70.16	16.10
June . . .	64.06	93.20	29.14	November . .	42.21	68.11	25.90
July . . .	67.48	101.54	34.06	December . .	34.15	59.17	25.02
August . . .	62.51	94.83	32.32				
September . .	56.73	85.63	28.90	1855.			
October . .	50.48	81.22	30.74	January . .	37.24	55.18	17.94

The next is the maximum and minimum temperature observed at any time during the month.

The Highest and Lowest Temperature at any time During the Month.

Month.	Min.	Max.	Month.	Min.	Max.
1852.			1853.		
June	54°	106°	November . .	33°	75°
July	53	106	December . .	28	66
August	56	101			
September . .	40	108	1854.		
October . . .	40	86	January . . .	15	71
November . . .	31	87	February . . .	32	66
December . . .	28	57	March	33	76
			April	40	84
1853.			May	44	91
January	29	72	June	54	96
February	30	68	July	61	110
March	32	84	August	55	108
April	39	83	September . .	53	99
May	46	92	October	46	98
June	51	105	November . . .	37	84
July	56	109	December . . .	21	71
August	55	104			
September . . .	51	96	1855.		
October	40	95	January	32	72

The next is the greatest range of the thermometer in any one day, and the least range in any one day of each month.

The Greatest and Least Daily Range of the Thermometer, Monthly.

Month.	Max. range.	Min. range.	Month.	Max. range.	Min. range.
1852.			1853.		
June	47°	21°	November . . .	40°	1°
July	43	17	December . . .	24	1
August	42	11			
September . . .	53	15	1854.		
October	44	9	January	31	1
November	37	0	February	25	1
December	19	0	March	30	5
			April	33	8
1853.			May	25	4
January	37	1	June	32	8
February	33	6	July	43	17
March	42	2	August	41	14
April	36	1	September . . .	36	20
May	35	1	October	43	3
June	39	3	November . . .	33	14
July	43	27	December . . .	33	3
August	43	13			
September . . .	41	14	1855.		
October	40	11	January	38	33

The next is the temperature of the warmest and of the coldest of the thirty-two months.

The Temperature of the Warmest Month and of the Coldest Month.

1853.	THERMOMETER.					1854.	THERMOMETER.				
July.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.	Jan.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
1	70°	86°	100°	82°	84.50°	1	41°	48°	60°	40	47.25
2	69	90	103	83	86.25	2	40	43	52	40	43.75
3	70	89	104	89	88.	3	31	40	52	40	40.75
4	71	89	99	87	86.50	4	31	36	50	45	40.50
5	60	90	103	86	84.75	5	33	36	41	35	36.25
6	75	93	103	85	89.	6	32	40	51	33	38.
7	70	85	101	82	84.50	7	32	33	48	30	35.75
8	68	82	98	84	83.	8	26	31	51	43	37.75
9	65	84	99	82	82.50	9	39	49	47	36	38.25
10	61	81	97	83	80.50	10	31	35	54	48	42.
11	69	80	89	74	78.	11	42	46	55	47	47.50
12	65	78	95	72	77.50	12	47	48	53	51	46.75
13	56	81	98	77	78.	13	54	57	62	51	56.
14	58	85	101	79	80.75	14	41	42	47	36	41.50
15	63	84	101	82	82.50	15	31	32	36	31	32.50
16	63	86	100	82	82.75	16	37	37	38	36	37.
17	66	84	100	85	83.75	17	36	36	37	35	36.
18	68	86	96	84	83.50	18	33	37	40	33	35.75
19	66	81	102	83	83.	19	22	25	27	20	23.50
20	62	91	106	82	85.25	20	16	21	32	23	23.
21	65	84	105	87	85.25	21	15	21	34	25	23.75
22	69	80	96	82	81.75	22	24	26	41	35	31.50
23	68	86	102	82	84.50	23	35	38	41	38	38.
24	66	90	107	84	86.75	24	37	38	43	40	39.50
25	66	89	109	88	88.	25	39	41	54	40	43.50
26	69	90	105	89	88.	26	32	39	55	38	41.
27	75	89	105	86	88.75	27	35	36	50	43	41.
28	73	90	107	90	90.	28	40	44	57	40	45.25
29	74	91	105	89	89.75	29	40	46	71	50	51.75
30	73	92	107	88	87.50	30	36	45	66	49	49.
31	72	92	105	85	86.	31	38	49	66	52	51.25
Mean	64.03	86.38	101.87	83.64	84.51	Mean	34.29	38.29	48.74	38.80	39.51

The mean temperature for the two months is, for July, 83.75°; for January, 40°.

The next is the hourly observations by the thermometer for twenty-four successive hours at the solstices and equinoxes.

Hourly Observations of the Thermometer for Twenty-four successive Hours, at the Solstices and Equinoxes.

	September 21, 1852.	December 21, 1852.	March 21, 1853.	June 21, 1853.	September 21, 1853.	December 21, 1853.	March 21, 1854.	June 21, 1854.	September 21, 1854.	December 21, 1854.
6 A. M.	46°	39°	43°	76°	55°	34°	42°	64°	59°	23°
7	49	39	46	79	59	34	46	72	63	33
8	51	40	52	81	66	34	52	74	70	35
9	60	40	61	85	74	36	56	73	74	36
10	66	43	67	87	81	42	61	73	79	41
11	75	43	70	91	86	46	65	74	79	53
12	81	45	73	93	90	49	68	74	84	65
1 P. M.	83	46	76	96	92	52	70	74	86	68
2	84	46	77	98	94	53	70	74	87	71
3	85	45	77	97	94	54	71	75	86	71
4	83	45	76	96	93	52	70	77	86	71
5	80	44	75	95	91	51	68	78	85	68
6	76	42	68	93	88	48	64	77	82	63
7	70	44	62	90	80	45	60	74	80	59
8	65	38	59	85	75	44	58	71	76	55
9	64	38	56	81	73	44	55	67	72	53
10	63	40	54	80	66	44	53	65	67	52
11	61	38	53	78	63	43	51	64	61	48
12	65	39	51	76	62	42	50	60	61	48
1 A. M.	65	38	51	75	60	40	49	59	60	46
2	65	38	49	75	58	36	48	56	58	44
3	63	37	48	75	57	35	46	56	58	44
4	59	37	47	75	55	34	46	54	55	43
5	58	36	47	75	54	34	45	53	55	43
6	56	36	47	75	54	33	45	56	53	42
Mean.	66	40.64	59.44	84.52	77.20	42.36	52.36	67.80	71.04	50.60

The next is the monthly mean clearness of the sky. The figure 0 indicating no clear sky, and 10 an entire absence of clouds and haze; the numerals between those figures signify the degrees of cloudiness.

The Monthly Mean Clearness of the Sky.

Month.	Mean clearness.	Month.	Mean clearness.
1852.		1853.	
June	8.50	November	3.50
July	8.75	December	2.
August	8.75		
September	8.50	1854.	
October	7.50	January	1.50
November	4.50	February	4.25
December	3.	March	5.
		April	1.25
1853.		May	6.50
January	5.25	June	6.50
February	6.25	July	8.75
March	5.75	August	8.
April	5.75	September	8.75
May	5.75	October	4.75
June	8.25	November	6.50
July	9.	December	4.25
August	8.25		
September	7.50	1855.	
October	7.	January	4.50

The next indicates the points from which the wind is most prevalent during each month. It blows from every direction. There is very little strong wind at any season.

The Prevailing Winds.

Month.		Month.	
1852.		1853.	
June . .	N. W., N., N. E.	November	E., N. E., N., S. W.
July . .	N. N. W., W., S. W.	December	E., N. E., N.
August .	N. N. E., E., S. E., S., S. W.		
September	N. N. W., N. N. E.	1854.	
October .	E. N. E., N. E., N.	January .	N. W., N. N. W.
November	S., S. W.	February .	E., S. E., N. E., N. W.
December	S., S. S. W.	March . .	N. W., S., S. W.
		April . .	N., N. W., S. S. W., S. S. E.
1853.		May . . .	N. E., N., N. W.
January .	E., N. E., N. N. W., S. S. W.	June . . .	N. N. W., S. S. W., S.
February .	N., N. W.,	July . . .	N., N. W., S. S. W.
March . .	N. W., N. E., E. S. E.	August .	S. S. W., N. N. W.
April . .	N. N. W., S. W., S. E.	September	N. N. E., S. E., S., S. W.
May . . .	S. E., S., S. W.	October .	W. S. W., N. W., N. E.
June . . .	N., N. N. W.	November	N. W., N. E.
July . . .	N. E., N. W., S. W.	December	N. W.
August .	N., N. E., S. E., S. W.		
September	W., S., E., N. E.	1855.	
October .	E., N. E., N., N. W.	January .	N. E., N. W.

The next shows the quantity of water in the form of rain or snow that has fallen monthly.

The Quantity of Rain.

Month.	Inches.	Month.	Inches.
1852.		1853.	
June	1.05	November	2.52
July		December	2.19
August			
September		1854.	
October48	January	2.90
November	8.48	February21
December	11.18	March06
		April	3.07
1853.		May	2.40
January	4.66	June	
February	3.18	July	
March	7.11	August	
April	4.57	September	
May70	October	2.26
June		November87
July		December	1.45
August24		
September02	1855.	
October02	January	3.69
Total, 63.31			

Of the sources to which have been referred the causes of intermittent fever, several are well marked here. For example: an argillaceous soil converted into a swamp during one-half the year, and presenting during the other half a dry, spongy surface, with water at a little depth beneath it; a prolonged rainy season, followed by one of dryness and intense solar heat; numerous beds of streams exposing dry surface or pools of stagnant water to the sun, especially one immediately beside us; the free passage of the winds in various directions; the great diurnal range of the temperature; the general growth of cryptogamic plants; and the grove immediately around us, the exhalations from which may chill the atmosphere at night, or shed down poisonous emanations intercepted by the leaves.

The intermittent fever occurs here at all seasons. A violent attack of tertian-intermittent occurred in an officer late in December last, just after fifteen successive mornings of white frost. A light shower followed the last frost, which was in turn followed by four frosty mornings, and on the second day of the last he was attacked. He arrived here for the first time seventeen days before his illness, and there is no reason to believe that he had contracted the disease elsewhere. The disease is perfectly controllable by the sulphate of quinia.

The following table is from the Reports of the Sick and Wounded at this post, made to the Surgeon General:—

Report of the Sick and Wounded at Fort Reading, California, for

MONTH.	FEVERS.				DISEASES OF THE ORGANS CONNECTED WITH THE DIGESTIVE SYSTEM.								THE RESPI- RATORY SYSTEM.	THE BRAIN AND NERVOUS SYSTEM.	THE URIN- ARY AND GENITAL ORGANS.	THE SE- ROUS EX- HALANT VESSELS.	THE FIBROUS AND MUSCULAE STRUCTURES.												
	Feb. cont. comm.	Feb. int. quotid.	Feb. int. tertian.	Feb. int. quart.	Feb. remittens.	Cholera.	Colica.	Diarrhoea.	Dysenteria acuta.	Dyspepsia.	Obstipatio.	Peritonitis.	Tonsillitis.	Asthma.	Bronchitis chron.	Catarrhus.	Pleuritis.	Cephalalgia.	Delirium tremens.	Gastralgia.	Paralysis.	Gonorrhoea.	Orchitis.	Anasarca.	Ascites.	Periostitis tibialis.	Pleurodynia.	Rheumatism. acut.	Rheumatism. chron.
1852.																													
June . . .	7	3						2		1						2												1	
July . . .	33	2	2					7	2	1																			
August . . .	46	1	1	2	1	1	5	1	1																		3	1	
September . . .	44	2				3	1	3				2			2	2	3										2	1	
October . . .	33	2		1		5	9	6							1	4	1											1	
November . . .	20	1				1	5	1		4		1			4	2	1												2
December . . .	16					2	1			2					20	3									1				3
1853.																													
January . . .	14					1	3								4														4
February . . .	23	1				1	2		2						2					1								7	4
March . . .	26	3						2		8	1									1							2	5	1
April . . .	29	1				1	1	2		1					1												1		1
May . . .	23									2		1	1		1													2	
June . . .	18					1	1			1																			
July . . .	29		2	1	2					2																		1	
August . . .	66		3		1	2	1		5						1													1	
September . . .	42					2	2																						
October . . .	63							2		4								1						1					
November . . .	43					1	4			1					1	3												1	
December . . .	22	5				1	8			1																			
1854.																													
January . . .	23	2	1			5									1												1	2	
February . . .	16					3				1																			
March . . .	23									1																		1	
April . . .	21					2																		1					1
May . . .	24	2				1			1																				1
June . . .	1	24	5			3			3						1							1							
July . . .	36					3			10						1							2							
August . . .	40					1	1	1	5													1							
September . . .	30	2				2			1	1					3		1												
October . . .	36	1													1	1													
November . . .	14									1					1								1						
December . . .	7	4				1	1			1					1														
1855.																													
January . . .	6					1																1							
Total . . .	1 897	37	1 11	2	25	79	17	1 59	1 5	1 1	52	1 13	1 1	2	5	1	2	5	1	2	1	2	1	1	8	22	19		
Causes of death . . .	1					3												1	1					1					

thirty-two months, from June, 1852, to January, 1855, inclusive.

ABSCESS AND ULCERS.		WOUNDS AND INJURIES.										ALL OTHER DISEASES.														MEAN STRENGTH.								
Phlegmon et abscess.		Ulcers.	Abrasio.	Amputatio.	Contusio.	Fractura.	Injury of knee-joint.	Subluxatio.	Vul. inelusum.	Vul. laceratum.	Vul. polypetecum.	Anemia.	Cachexia.	Debilitas.	Epilepsia.	Furunculus.	Hemorrhoida.	Morbi cutis.	Odontalgia.	Onyxia.	Ophthalmia.	Otitis.	Scorbutus.	Toxæmia.	Varix.	Morbi varli.	Total.	Returned to duty.	Discharged service.	Died.	Remaining sick.	Officers.	Enlisted men.	Total.
..	..	1	..	1	18	3	43	46	
..	1	1	1	50	3	41	44	
1	1	1	66	3	41	44	
..	2	1	66	5	41	46	
4	1	..	1	1	1	1	1	..	1	1	1	8	84	..	1	1	7	92	99	
4	2	1	1	2	53	1	7	88	95	
1	2	2	1	1	1	2	58	1	7	87	94	
..	2	1	1	3	35	1	7	86	93	
1	..	2	1	1	1	49	2	8	158	166	
1	1	..	1	2	..	1	1	1	..	1	3	61	7	153	160	
1	1	2	1	1	1	..	2	46	..	2	..	6	118	124	
..	2	1	1	..	1	1	35	..	3	1	5	71	76	
1	1	1	1	1	..	26	6	65	71	
..	2	1	1	41	6	69	75	
1	1	82	7	63	70	
..	2	1	49	3	28	31	
..	2	1	74	6	60	66	
..	..	1	2	4	61	6	50	56	
..	..	1	1	1	40	5	43	48	
..	1	..	1	37	5	43	48	
..	1	21	5	43	48	
..	1	1	1	29	5	42	47	
..	1	1	27	6	43	49	
..	1	..	3	1	34	6	43	49	
..	1	1	40	7	90	97	
1	4	1	1	2	1	2	..	2	..	66	6	75	81	
..	..	1	1	1	1	1	2	..	1	..	57	5	71	76	
1	2	41	1	5	69	74	
1	2	1	43	5	73	78	
..	3	20	6	73	79	
..	1	16	6	64	70	
..	1	9	6	62	68	
18	4	3	6	29	1	1	5	12	1	3	1	1	2	15	6	1	6	5	4	3	4	1	10	1	1	22	1432	1417	6	8	1	Aggreg.	2568	
..	1	8	Average	80.25	

In point of climate and salubrity, the description of this part of the country is applicable to much of the country lying between the range of the Sierra Nevada Mountains and the Coast Range.

The *tribes* to which the Indians who rove about here belong, are not distinguishable by our present mode of communicating with them. They consist of a few small bands, speaking different languages, and each known specifically by the name of its chief. They are all classed under the term "digger," and are farther arranged according to the names of the streams, which are severally their usual haunts. They will average about the medium size of white men, and are well-proportioned. The head is not below the medium size, and is not deficient in local developments. The superciliary ridge projecting unusually, gives the forehead the appearance of retreating more than it would otherwise have. The eyes are not oblique, and the powers of vision are very extraordinary; the cheek-bones are high; the nose regularly arched—not long—the alæ spread out less than in the African—the orifices present downwards; the teeth are regular, white, not large; the lips are thicker than in the European; the inferior maxillary bone is not at all disproportioned to the rest of the face; the chest is well developed; the abdomen is protuberant; the hands and feet are remarkably small; their general expression, when not hungry, is that of happiness and benevolence; at other times, they are pensive, grave, subdued, and seemingly *wretched*. They are armed with the bow, made elastic by a layer of some animal fibrous tissue on its back, and the wooden arrow pointed with flint. They live on small game, fish, insects, acorns, roots, and grass. They dwell in small, round-top huts, made of dirt supported by skins and branches of trees. Among themselves, and between the different bands, exists a remarkable charity: when starving with hunger, they will divide the last morsel of food with all to whom they can have access. The men go naked, the women wear an apron of twisted grass. When not aggressed upon they are harmless. Poor creatures! They are fast disappearing before the strides of the white man—even his cattle rob them of their sustenance.

ART. III.—*On the Physiology of Exercise.* By WM. H. BYFORD, M. D., of Evansville, Indiana.

It is seldom that any remedial means receives the importance due to it from the great body of the profession, unless its physiological *modus operandi* is rational and well understood. Although the importance of voluntary exercise has been recognized for centuries and prescribed to its most useful extent by many of the profession, its great practical advantages in a large number of diseases have not been appreciated to their full extent by all. The only rea-

son I can ascribe for this is that its effects upon the animal economy have not been thoroughly investigated and understood. It is with a view to draw the attention of the profession to the importance of more research in this direction, that I wish to record my views upon the subject.

The subject is so extensive and so intimately connected with several other questions in physiology, that I shall not attempt to give more than a very brief sketch of what I consider the proper physiology of exercise.

What I mean by *exercise* is the voluntary discharge of any or all the animal functions, as intellect, sensation, locomotion, and voice. The obvious phenomena of exercise are, vascular excitement, increased heat, redness of the surface, and augmented secretion and excretion. Degrees of voluntary functional activity and the number of organs engaged greatly modify the effect on the constitution, or, in other words, on the organic functions. Functional activity, however, either of the animal or organic system, has the same general effect in accelerating the circulation, promoting secretion, excretion, &c., differing in degree, owing to the extent of tissue engaged. For example: uncommon activity of digestion (which includes the functional action of several organs) increases the circulation, secretion, &c., just as increased activity of the muscular system does the same. Languor of function in some of the great organs of the organic system produces languor and inactivity of the rest, just as inactivity of the muscular system does the same.

How these phenomena are produced, I shall now attempt to explain. As the blood is the fountain from which all organs draw the material necessary to supply them in the discharge of their functions (as well as their substance); as every organ must be supplied with its own peculiar material, and as the materials for the support of all their diverse and separate functions are equally distributed throughout the whole mass of circulating fluid, it necessarily follows that the supply of blood must be increased in proportion to the increased activity of the function of any organ. When the organ is small, and the increase in function slight, the supply will take place without any perceptible acceleration of the general circulation; but, if the organ is large, the number is great, or the increase of action considerable, then the general acceleration of the circulation is essential to the continuance of their vigorous action, in order that the material they are expending in the discharge of their function may be duly delivered, and the waste material removed. This acceleration is not only necessary, but is in all cases the effect of increased function, as may be seen by the following reflections. As each muscular fibril contracts in the body of a muscle, nutrition and disintegration take place as an accompaniment of that contraction. Nutritive material is taken from the blood, and effete matter deposited into it. If the muscle acts slowly, this exchange of material between it and the blood is less rapid; but if by more active voluntary exercise its action is increased, the rapidity of exchange must be increased in the same ratio; and as this rapidity of exchange increases, the nutritive substance for the sustenance of waste in the capillaries in immediate proximity

is rapidly exhausted, and the functional action (contraction) of the fibrilla would cease but for a continued and abundant supply of unexhausted blood. This is readily effected by the elective affinity between the blood in the right state for the purpose and the acting tissue; thus every contraction of the fibrilla produces elective affinity in itself for the appropriate nutritive material contained in the vessels surrounding it, which draws it forward from the arterial extremities of the capillaries where the most perfectly elaborated and oxygenated blood is to be found, with a rapidity varying with the quickness and frequency of contraction, and after exhausting it of the material necessary for its own support, losing the attraction for it, the blood is abandoned to be urged forward into the nervous extremities of the capillaries by the stream from behind, which is moved by the same elective force. Or, perhaps there may also be some real repulsion between the exhausted blood containing effete substance from the muscles and the tissues through which it has passed, thus gathering a new impetus in an onward direction.

This is the capillary force of the circulation. If the muscles are acting quickly and frequently, they will attract to themselves in this way a large amount of blood from the capillaries, and pass it on through the veins to the heart. This last organ receiving more than its ordinary supply, is stimulated to increased frequency of pulsation to get rid of the increased amount of blood forwarded to it.

Thus commences and is kept up the vascular excitement of exercise, and when a large number of muscles are acting, the general excitement is great. Accompanying, and as a consequence of the excitement of exercise, some of the organic functions are increased as the excretions and some of the secretions proper, the former particularly, such as the functions of the skin and kidneys. This, no doubt, depends upon the rapid delivery into their capillaries of the waste materials derived from the acting organs, which is their natural stimulus. Hence the perspiration and increased urinary excretion of exercise. Now, although the above explanation applies particularly to exercise of the muscular system, it is to some extent equally true of great activity of the nervous system also. And it is not improbable that some of the emotions which so intensely excite the circulation, may cause a great part of it by the atonic or molecular changes going on in the nervous centres engaged, increasing the circulation in a large capillary plexus, and thus the general circulation as above described.

It may be objected to this that the effects of many emotional phenomena are too instantaneous and intense to be thus explained. After stating that I am not prepared to believe implicitly that such is the case, I would reply that of course the organic changes going forward in the substance of organs, as above explained, correlatively with the discharge of function, must vary in the different organs, and perhaps the effects of the muscular system fall short of the nervous in celerity and power of action in this respect. Moreover, there are but few emotional affections of the nervous system but that calls

more or less suddenly, extensively, and powerfully into action a portion or the whole of the muscular system. I wish to insist here only that immediate functional action stimulates the bloodvessels to increased action through the capillary system by pouring the blood more rapidly into the heart (not merely, perhaps), but for the most part by the atonic change going on in the substance of the nervous centres as a necessary accompaniment of their activity, but also the consentaneous and extensive actions of other of voluntary or animal organs.

It has hitherto been generally held, that the mechanical action of the muscles upon the veins, by pressing forward their contents, is the main cause of accelerated circulation in muscular exercise. This theory, however, is certainly inadequate to the explanation of the effect of exalted cerebral and other functional action in this respect.

And direct experiment proves that muscular *tension* alone is sufficient to increase the circulation to even a greater extent than alternate contraction and relaxation. In the former condition, in fact, they are more intensely active than in the latter, and a greater number partake in the function. One individual whose pulse, when sitting at ease, was sixty-eight beats to the minute, was directed while sitting to stretch one of the lower extremities, and retain all its muscles in a state of forcible tension for one minute. This increased the frequency of the heart's action to seventy-eight beats. Upon maintaining both lower extremities in the same state of tension, the pulse was increased to eighty-six in the minute. The first experiment increased the heart's action ten beats, the second, eighteen above the natural standard. These were repeated in the same individual several times with about the same results. They always increased the number of beats very considerably to a greater extent in the second than pretty rapid walking. In another person, whose pulse was eighty-eight to the minute, when sitting at leisure, the forcible extension of one extremity as above, increased the pulse to one hundred and four to the minute, and of both to one hundred and eight. In the first sixteen, and the latter twenty beats to the minute above the standard. At another time, the first individual held a book weighing three pounds and a half at arm's length with the right hand for one minute. His pulse was increased six beats in the minute. Upon holding one book in each hand in the same manner for the same length of time, the pulse was increased fourteen beats to the minute. As in all these cases the muscles, although rigid and tense, were as near as practicable motionless, the inference is that the mechanical effect of pressure could have had but little if any effect in producing the acceleration.

It must be borne in mind that the muscles of the extremities were not the only muscles in action, but that those on the back, hips, and abdomen, in the experiments with the lower extremities, and those of the chest, back, and abdomen in the upper, were also in a state of contraction. Of a like character, and admitting of the same explanation, is the experiment of standing under a

heavy load. I think the inevitable inference deducible from the above experiments, is, that the functional activity of the muscles must have increased the circulation, as no other cause was apparent.

The question will no doubt be asked: "Is this explanation sufficient?" The consideration of this question involves the whole of the present received theory of the capillary circulation, which, of course, I cannot discuss here, but have glanced at above. I think it may be accepted as a demonstrable truth, that the alterations in the frequency of the heart's action depend more upon the capillary circulation, as influenced by activity of function in organs, than any other physiological cause. In fact, the present state of physiology and pathology, I think, justifies the conclusion that the activity or inactivity of the capillary vessels always influences the heart and arteries, and the latter is almost always, if not quite, *governed* by the former, both in health and disease. The more rapidly the blood is transmitted through the capillaries into the veins, the more must arrive at the heart; and the larger the quantity poured into this organ, the more frequently must it contract in order to pass on the accumulating blood. Supposing, as is stated by Volkmann, that the combined capillaries of the whole system are in vascular area, four hundred times larger than the arteries, and in ordinary activity transmit the blood just fast enough to keep the heart steadily beating at sixty-eight in the minute; it is no difficult matter to see how a very slight increase of action in one-half of them may, when added to the influence of the other half in their natural grade of action, increase the motion of the heart and arteries a few beats in the minute, by stimulating them with the additional blood forwarded to it. This is precisely what takes place when all the muscles are exercised. Probably the whole of the voluntary muscular system does not fall short of being equal in weight to all the rest of the organism. Now, if we consider that in the experiments with the lower extremities, the blood in one-third of the whole of the capillaries (and including the nervous function called into play, there must be this much) is passed along for the purpose of supporting the increase of nutrition thus demanded, and carrying off the effete material from the organs, with but a slight increase of rapidity, we have sufficient cause of several additional pulsations to the minute in the heart and arteries. But this is not all. The additional amount of carbon in the blood, and the demand for oxygen would quicken the motion of the blood through the capillaries in the lungs, and the additional amount of material for other excretions would, on the general principles of capillary circulation, accelerate the motion of the blood through the other excretory organs into the heart and arteries; and thus it is that a larger portion of blood than usual is carried through the capillaries of one-half of the capillary system; and unless the frequency of the heart's action is increased, there must be an accumulation in this organ.

When mathematically calculated, I apprehend that, viewed in this light, the power in the capillary circulation would be found to exceed that of the heart and arteries; and, could it be possible, to increase the functional action of all

the organs to their utmost extent, at the same time that the heart would be unable to bear a sufficient part in the circulation to keep it up. For although the power of a few capillaries amounts to very little, yet multiply it by millions, and the sum would be considerable. We find, in fact, that when the walls of the heart are thinned by dilatation, or rendered fragile by osseous deposition, severe muscular exertion, or a sudden burst of passion causes it to give way—to break. This may occur under the increased action of a small part of the organism, and it is altogether probable that there is sufficient energy in the combined action of all the capillaries to break a sound heart.

Exercise produces effects varying according to its degree, character, and duration, upon the composition of the blood, the secretions, and the solid tissues themselves. Exercise of the whole of the voluntary muscular system is attended with general and equal distribution of the blood, and temperature of the body. The regular or exalted action of the whole of the muscular system requires efficient distribution of the blood in the extremities for the support of themselves under exertion, and by these very wants draw a large amount of blood through their capillary vessels in the manner above explained, and being distributed all over the system, must thus produce a general and equal supply of blood and warmth to the whole body. The active nutrition and disintegration thus brought about, and, if exercise is continued, kept up, give a degree of comfortable temperature that no artificial heat can produce. A very important effect of exercise is its influence on the composition of the blood. The doctrine so ably set forth by Paget, in his *Surgical Pathology*, of the necessity of the development of one organ to the right state of the blood for the development of another, and that the arrest of the development of one would hazard the integrity and efficiency of certain others showing the material dependence to a certain extent of all the organs, is no less true than that the discharge of function of all the organs in the body affects the capacity, by altering the composition of the blood of each other to perform their respective functional duties, and that the functional action of each contributes to the right "state of the blood," to enable others to perform their duties. For wise purposes, this mutual relationship in the higher classes of animals between the voluntary and organic system is indispensable. In more respects than one, the efficient capacity and action of each are necessary to the support and integrity of the other. The procuring food by the animal organs, the conversion of it into nutriment, moulding it into the organization, and then excreting it by the organic, are all mutually dependent phenomena. They are dependent upon each other physically and physiologically. They were made to perform for each other those mutual tasks.

The processes by which the composition of the blood is affected during functional action are not certainly known, but the general principles by which they are governed are becoming perceptible in the physiology of the present day. Although there is great similarity in the composition of many organs, there is a peculiarity in the appearance and composition of each

which will chemically, physically, and physiologically distinguish it from every other, and which requires a peculiarity to some extent in the substances intended for the support of them separately. And, as all this variety of material comes from the blood, we can appreciate in a general way the effect upon the composition of this fluid, the abstraction of a certain material by one organ, and again, a different one by another. It is also apparent that the failure of a large organ, or set of organs, to withdraw from the blood the substance intended for its or their use in the discharge of their functions; and there should be no other way of getting rid of it, that its accumulation might and most likely would embarrass the functions of some or all the others, besides producing disease in other ways. This observation will have more force, when it is recollected that, although nearly, if not quite, all the material for the support of the organs in the discharge of their functions, may be found in the blood; they do not exist in precisely the same condition, and require to be submitted to the elective affinity of the tissue they are intended to support, in order to undergo the necessary alterations; so that, in the blood, we may have the materials for the composition of two or three different organs in the same *blood-compound*, the initiative analysis of which must begin in a certain organ or not at all.

The most important of all the organs in the body in this respect, because of such large volume when taken together, the most active in their operations, or should be, and requiring for their nourishment a network of capillary vessels a hundred and fifty times larger in volume when computed together than all the arterial system—is the muscular system of organs. The active, continued, and judicious use of them in discharging their functions, induces more essential and material changes in the composition of the blood in a direct and indirect manner, take more initiative steps in the elimination of an abundant class of compounds in the blood, which to be sure have been considered innocuous, but where accumulated to an improper extent give origin to some of the most fatal diseases, than any other class of organs in the whole animal economy. Of course we must take into consideration the associate changes induced in the blood by the evolution of the nervous energy required to actuate them as an important correlative circumstance. I allude to the albuminous or protein compounds. They exist in the blood in larger quantities than any other substances. Although many other organs are formed out of this ingredient of the blood, and their functions supported by it, the muscles seem to be almost exclusively supported from albumen. Taking into consideration the great extent, therefore, of this system, and the rapidity with which it converts albumen into its own composition, and again eliminates it by presenting it to the kidneys in a form readily convertible into urea—and the albuminous excretion from the skin under a state of exercise, the effects of active exertion of the whole voluntary muscular system assume an importance not easily overrated and not hitherto awarded to it. More than half of the albumen of the blood must most likely be consumed by the muscles, and

thus eliminated from the bloodvessels *per vias naturales*, when this system is exercised to its intended natural degree.

When exercise of the voluntary system is carried to an undue extent by consuming faster than the powers of digestion, or the vessels of food can reproduce the albuminous portion of the blood, it affects that fluid and through it the vital organs in an injurious manner. If the solid contents of the serum is in this manner reduced, and it is rendered thinner and of less specific gravity than natural, it induces anemia by swelling up, and finally bursting many of the red corpuscles of the blood, upon the recognized principles of endosmosis and exosmosis, whereby a thinner surrounding fluid will permeate the walls of cells contained in it and cause them to burst. On the contrary, when, from chronic want of exercise the albumen of the blood is increased, the reverse principle will act in the destruction of the red corpuscles by causing their contents to exosmose until they shrivel and die. That anemia may be brought about in the former way, but few will deny. The last process, however, will no doubt be denied by many. For, although an undue increase in the albumen ought to produce these effects, some physiologists believe it to be directly consumed in the formation of urea and other nitrogenous compounds excreted by the kidneys. It certainly must be imperfectly eliminated in this way in persons of very sedentary and inert habits. For, in the absence of exercise, the languid condition of the circulation prevents that efficient state of capillary circulation through the secreting organs necessary to their energetic action. And further, that urea, in this view of the case, must be formed by the kidneys, while it is certainly much more in accordance with the present state of physiology to consider excretion the results of organic disintegration, the discharge of effete material instead of material in a high state of plasticity, and advancing instead of retrograding in vitality under functional action.

This, I think, will soon be found to apply particularly to the formation of urea and uric acid. It at least is certain that the former substance is greatly increased by muscular exercise, and should be considered, I think, as a muscular excretion in the qualified manner before expressed.

In the experiments which prove the increase of urea by an increase of food which affords the protein compounds by Lehmann, we are not told whether exercise was taken into consideration or not. If there was only a moderate amount of exercise, which is probable, the increase material of nutrition for muscular substance would most facilitate the passage of the protein compounds through the muscles, and thus into the kidneys.

Some writer states, I believe it is Becquerel, that protracted paraplegia brings on albuminous cachexia. And one of the most potent causes of tubercular cachexia, which I regard as always being attended with an increase of albumen—and its effusion in the tissues as the effect of its superabundance—is want of exercise and consequent non-consumption of the albumen of the blood. One more fact I would bring to mind here which, I think, bears upon the subject, and that is that in the organic system of organs; for the most

part there are two processes going on, between which there should be a distinction made; one is the histogenetic, by which the solid substance of the organ is removed and replaced, or simple nutrition, while the other is devoted to the elimination of some secretion or product of some kind. This is true of the lungs also; so that, while an organ is excreting the effete materials of one or several other organs, its own effete substance is thrown into the circulation to be eliminated by some other. Now, in many of these, the albumen of the blood is no doubt the form of substance from which this nutrition is carried on as well as out of which the histogenesis of secretion takes place. In the animal organs, it will be remembered, this nutrition is the cause, or at least the necessary accompaniment of their functional phenomena. They do not take from the blood anything that is not returned to it again in an altered form. That altered form being an approach towards, or an excretory substance to be eliminated through some of the secretory or excretory organs, and corresponds in this respect (perhaps in no other) with the effete substance from other organs in a state of molecular change from functional activity.

This leads me to the consideration of the effects of exercise on the *secretions*. The competency of the different secretory organs to the entire elimination, or so far as necessary to health, being a supposed condition which in the nature of things ought to obtain, the amount and character of the secretions must vary very much with the quantity and quality of exercise. The different excretions being furnished by the effete material set free from the organs in use with the material for the elaboration of their product, the excretory organs are urged to functional activity corresponding to the rapidity of the circulation through their tissues fraught with such substances. And, as before said, these conditions are the result of active muscular exercise. The secretions proper and secreto-excretions are all equally prompted to a like energy of action. The secretion pouring into the alimentary canal from the lips to the anus, which form the solvent and menstrua through which the nutritive portion of food finds its way into the blood, and the excrementation is carried along down the alimentary canal, are all more efficiently evolved under the influence of exercise. No doubt but the same reasoning as to the cause of increase of these secretions will apply as in the cases of excretions. The blood changes wrought by exercise present to the organs concerned in their formation the material for their characteristic products in an advanced degree of maturation, or probably entirely formed, and in quantity correspondent to the amount of exercise and consequent increased rapidity of circulation through their tissues. The effect of all this is greater demand for and capacity to digest food. It is this beautiful mutual dependence and reciprocal stimulation, by affording the material ready prepared as it were to each other, both in a physiological and physical sense, that the health, efficiency, and even integrity of the animal and organic are preserved in their proper condition for the support of the whole system.

I may here, I think, pertinently reflect upon the physiological effect of a

failure from any cause whatever of a large part of the voluntary organs to perform their functions. If, for instance, the muscular system is constrained or kept still by nervous inability, indolence, or any peculiar habits, the parts become weak, soft, shrivel, and finally, if carried to a great extent, their peculiar tissue is lost and replaced by the more simple structure. If no extraneous aid is afforded to the individual, the organic is not furnished with food, and the whole system perishes from starvation; but, if the services of this part of the organism is dispensed with, and the food is procured by others, then may be seen those changes going forward which result from the physiological relations before mentioned. That portion of the muscular system which has ceased to act, as it has become useless, requires less nourishment for support than natural. Hence if the same amount of food be taken, indigestion prevents its introduction into the system, and thus to some extent regulates the supply by the demand. The indigestion is itself the result of inefficient circulation in the secretory tissues, and a want of the "right state of the blood" for the production of healthy secretions necessary to efficient digestion, both of which conditions depend on want of muscular action or muscular wear and tear. A lack of secretions may be seen everywhere. The feces are dry and light-coloured; the urine is deficient in richness of solid material, if not in quantity; the skin is inactive, all of the functions partake of the inactivity of the voluntary system; and if it cannot be restored to something of its former activity, permanent ill health is, in a short time, succeeded by death. The effect of inactivity of the organic system may be inferred from what we every day witness. We have seen that the quickened arrival of blood to the right side of the heart caused by the augmented activity of the capillary circulation in muscular or other exercise, excites the general circulation, that functional activity of the voluntary part of our organism produces energy of circulation and enhances excretion.

There is a condition of the organic functions which also stimulates the heart's action and causes general vascular excitement, that this is the only analogy between it and voluntary exercise. Supposed secretion and nutrition, one or both, occurring either from a condition of the blood or solid tissues, obstructs the capillary circulation, and, by damming back the blood upon the left side of the heart, stimulates that organ to increased exertion, and as the heart is so organized that a stimulus to one part excites the whole, general vascular excitement is the result.

In the case of suppressed secretions, as of the skin, liver, kidneys, partial or complete, &c., in fever, the blood is delivered into the capillaries, but the entire or partial arrestment of the elective and repulsive forces exercised by the function of secretion leaves these vessels passive membranous tubes through which the blood must be forced by cardiac and arterial power alone. The arrestment of these forces causes the blood to accumulate behind and eventually be felt as a stimulus to the heart and arteries, which are actuated thereby to increased exertion. Now, in inflammation, much the same thing

occurs in the process of nutrition. The mutual elective attraction exerted between the blood and the tissue involved in a state of normal nutrition, is subverted by the morbid process, and the capillaries are passive membranous tubes through which the blood is driven with great difficulty. Accordingly, we find that when the extent of capillaries involved is small, increased throbbing is confined to the vessels of the neighbourhood; but, if extensive, no matter where situated, by causing a larger quantity of blood to press back upon the heart, it stimulates it to increased action also.

In proof that this is a true explanation of the phenomena occurring in fever and inflammation; in the first, anything that restores the play of elective affinities above mentioned, and consequently the secretions, removes the vascular excitement; and in the last, we have only to restore the right state of nutrition to get rid of the inflammatory fever. Another circumstance which bears upon this point is that in cases of fever, both idiopathic and inflammatory, the former, in which there is always a larger capillary surface involved, muscular exertion is impracticable. Now, although the blood may not be in the right state, for want of the depurating influences of excretion, to allow the nutritive changes in the muscular system indispensable to exercise, and hence render it not only weak but utterly unable to act much, there can be no doubt that the distressing state of oppression and languor, under exercise in fever, arises from the additional stimulus to the right side of the heart by increased capillary action; for it is quite conformable with observation that the capillary inertia in certain organs where functions have been arrested may be accompanied with capillary activity in other parts of the system. What I mean by capillary activity and inertia, is, that in the first there is elective affinity from a discharge of the functions of nutrition and secretion between the blood and the tissues through which these vessels pass forward the blood in aid of, or independent of arterial and cardiac force. In the second, no such affinity is exercised, or at least in a minor degree if at all.

ART. IV.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. Army.

DR. STROBEL states, that the yellow fever was transmitted from Charleston to Augusta, Savannah, and St. Augustine, Florida, in 1839. He says:—

“The yellow fever made its appearance also in Augusta during 1839. We have conversed with several intelligent gentlemen, who believe most firmly that the disease was transmitted to that place from Charleston. It is true that they admit there were some cases of fever which occurred early in the season,

but it was not until several cases had been transported there from this place (Charleston), that it began to assume that particular form." p. 187.

Let us examine this assertion. Dr. Robertson, in his Report to the Medical Society, presented December 10, 1839, observes:—

"There were three cases of yellow fever in our city, in the month of July, occurring in individuals brought from Charleston, S. C. An Irish woman and her daughter arrived from Charleston on the 27th July—both were unwell on the evening of their arrival. One of your committee saw them on the 29th, and the mother died, with yellow skin, black vomit, and the other usual symptoms of yellow fever, on the 30th; and the daughter, who was immediately removed to the extreme western part of the city, recovered. These cases occurred on the north side of Broad Street, opposite the upper market, and were visited by numerous individuals who resided in the neighbourhood. The third was a gentleman, who arrived, at the Eagle and Phoenix Hotel, sick, on the 31st July. This case was attended by Dr. Cunningham, who states it to have borne all the marks of yellow fever, though the patient recovered, without either yellow skin or black vomit. Your committee, after minute examination, have been unable to ascertain that any other cases were introduced into the city, before or since the three just named."

"The attention of a community is seldom drawn to the existence of an epidemic until its general prevalence, or the mortality, reaches a point sufficient to excite general alarm; and hence it is, that the first cases that occur, are usually entirely overlooked, or merely considered of a sporadic nature. The earlier cases are, also, of a much milder character than those that occur after the cause has acquired its maximum degree of virulence. These early cases should not, however, be overlooked, as we shall see in the course of our investigations, that they form an important link in the chain of cause and effect."¹

It is stated that several severe cases of fever occurred in Mrs. Thomas Gardner's family, about the 8th of June, which the committee believed "arose from the same cause, though in a less virulent state, that produced the prevailing epidemic."

"On the 5th of July, John McGuire, who had labored in the upper 'trash wharf,' was taken sick and remained violently ill for ten days. His physician, and those who visited him, consider his case one of the same nature as the prevailing epidemic. On the same day, a son of Mrs. Bush, who resided nearly midway between Lincoln and Houston Streets, on the north side of Broad Street, was taken with fever, and died, with yellow skin, and (as his mother states) passing from his bowels large quantities of matter resembling coffee grounds, on the 14th. She further states that numerous purple blotches appeared on him soon after his death. His attending physician, Dr. J. A. Eve, concurs in the opinion that it was a case bearing the marks of the late epidemic. On the 7th July, a negro named Sophia, was taken sick of fever, at the house on Broad Street, next below Augustine Fredericks. On the same day, Isaiah Burton was also taken at his residence on the south side of Reynold Street, just below Centre Street. Both these cases are stated by Dr. Cunningham to have been decided cases of the prevailing fever. The same physician states Mrs. A. Sibley was taken violently ill on the 16th of the same month—no other members of the family were attacked until the 25th."

"Mr. Tutt's daughter was seized on the 14th July, and died, with all the marks of the prevailing fever, on the 24th. Mr. Bottom's child, on the south

¹ In the season for endemic and epidemic fevers, the first cases, always termed sporadic, should never be overlooked. These are the most important cases of all, and are neglected and forgotten, until some phantom ship comes into port with imported contagion, or contingent contagion. The first cases in every summer ought to be carefully recorded; and we believe that, in nine epidemic years out of ten, the fever has begun before the arrival of the ship which is said to import the contagion.

side of Broad Street, a few doors above the lower market, sickened on the 16th of the same month, and died on the 18th. This was pronounced by Dr. Cunningham, the attending physician, to be a decided and violent case of the prevailing fever. A negro boy of Mr. Wagner's, on the south side of Broad Street, a few doors below the lower market, was attacked with the prevailing fever on the 23d July. Henry, a child belonging to Mr. Dalby, on Green Street, just above Lincoln Street, was taken with the fever on the 26th July. Numerous other cases, forming a regular connection, could be mentioned, but your committee deem those already named quite sufficient. From this time the disease became general."

Here are at least nine (9) cases of yellow fever before the arrival of a single case from Charleston. The Augusta cases proper extend from the 5th to the 26th of July inclusive, and the first cases of fever from Charleston did not arrive until the 27th of the same month, when the disease was already epidemic.

"After having thus given the date of the commencement of the prevailing fever, the concurrent testimony of the attending physicians, and the period at which the cases of yellow fever were introduced from Charleston, and their different localities, we feel satisfied that further argument is unnecessary. That the epidemic had commenced, fairly and decidedly, before the introduction of a single case of disease from Charleston, must be evident to every unprejudiced observer."

"Moreover, if the disease had been introduced by contagion, it certainly should have been governed, after its introduction, by the same laws that regulate the spread of contagious affections. But, on the contrary, the prevailing fever certainly exhibited none of the phenomena that ordinarily accompany the spread of contagious diseases. We find it commencing on the same day, and almost at the same hour, in different houses, at the time attracting no unusual degree of attention."

"The fact of a number of persons having been taken with the disease, who nursed the sick in what was termed the infected district, has been urged, by some, in favor of contagion. As an offset to this, your committee would bring forward the fact, that no one was taken with the disease, who nursed the sick, who were removed out of the infected district, or to the Sand Hills, or other parts of the county. This we consider an unanswerable argument against the contagious nature of the prevailing fever. If the disease was contagious, how could a removal of the sick, half a mile or more, from the infected part of the city, deprive it of its contagious properties? On the grounds of contagion this cannot be explained; but, considering the disease of miasmatic origin, the fact is easily accounted for. Those who nursed the sick in the infected district, were exposed—and that too at the worst period of the twenty-four hours, to the same miasmatic exhalations that had produced the disease in the patient. When the patient was removed, however, to an atmosphere free from malarial poison, the nurses escaped, though they were exposed to the exhalations from the diseased body of the patient, who often expired in the most frightful agonies, with black vomit, hemorrhage, and all the evidences of extreme putridity. Not a single instance can be found, in which an individual, who had not been exposed to the contaminated atmosphere of the city, took the disease after communicating with those who were removed from the city, labouring under it, or who took it after their removal. There were instances, in which persons slept, night after night, with those who had the disease on the Sand Hills, and yet they escaped entirely. Several instances occurred, of persons being attacked with the prevailing fever, who had confined themselves to their houses, and had neither seen, nor held intercourse whatever with the sick. Norris, a criminal, under sentence of death, who was confined in jail, was taken with the prevailing fever on the 12th of September, and died on the 10th day after his attack, with black vomit and hemorrhage. He was immured in a cell; no person had communicated with him, previous to his attack, but the

jailer and turnkey, and he was the first person in the prison who took the disease."

"After the first black frost on the 8th of November, great numbers of the citizens returned permanently to the city. If the disease had spread by contagion, why did it not extend to them, from the numerous cases that were then under treatment? These facts are unanswerable, and sufficient, in the opinion of your committee, to convince any reasoning individual that the prevailing fever was neither introduced nor spread among us by contagion."

"But again, if the disease was introduced by contagion, whence did it come? Charleston is pointed to as the most natural source, as the yellow fever existed there at the time the prevailing fever broke out here. We have shown most conclusively that the epidemic had commenced before the introduction of a single case of fever from Charleston."

"If we examine minutely the history of yellow fever, wherever it has prevailed, we shall find that it invariably obeys most of the laws which govern other miasmatic diseases. It has usually commenced its ravages during the heat of summer, increased in violence as the summer advanced, and ceased as soon as the temperature ranged below a given point, or after a severe frost. Contagious diseases conform to none of these laws. What climate, what temperature, or what season can arrest the ravages of smallpox, for instance?"

Perhaps it would have been well, if Dr. Strobel had conversed with the intelligent members of the profession in Augusta, as well as with those "intelligent gentlemen." A journey from Charleston to Augusta by railroad is not an arduous undertaking, not so great as the trip to St. Augustine, four months after the fever began in Augusta; and it was as easy for the doctor to communicate with Dr. Robertson, Chairman of the Committee, as it was for Dr. Robertson to address Dr. Geddings. So with the other physicians.¹ Dr. Strobel's account of this epidemic appears to be erroneous. But to the Report.

"The theory of the second class, who assume the ground that our atmosphere was in a vitiated state, and that the simple introduction of a case imparted an epidemic constitution to this vitiated atmosphere, and thus spread the disease, must share the fate of the first. Indeed, if such an occurrence was possible—which is contradicted by all the facts connected with the history of miasmatic diseases—it will be impossible to account for the introduction and spread of the prevailing fever in this manner."

"The principle which vitiated our atmosphere was the cause of the disease; that, without which, the epidemic could have had no existence, and which did not require the introduction of foreign cases to produce an explosion. The torch had been applied before the introduction of the foreign cases, and was, silently and unobserved by the multitude, performing its work of death and destruction. Indeed, cases of yellow fever have been brought into our city almost every season in which it has prevailed in Charleston, and hitherto we hear nothing of their imparting an epidemic constitution to our atmosphere. The same can be said of other parts of our State."

The yellow fever of Augusta, in 1839, was owing wholly to local causes, but our limits will permit of only a brief notice of them. The causes proceeded from "what is commonly called the *Upper Trash Wharf*, situated between Elbert and Lincoln Streets, which we believe to be truly the *fons et origo mali*."

A vast quantity of city offal, not less than 200,000 cubic feet of animal

¹ Dr. W. E. Johnson, of Augusta, wrote to Dr. Strobel on the subject of transmissibility, but disagreed with him.

and vegetable matter, had accumulated at this wharf or platform. In April, the City Council very unwisely resolved that this immense mass should be removed, and the work commenced on the 8th of May, and was partially finished on the 29th of the same month. A further levelling was made between the 26th of June and 2d of July.

"During the operation of the workmen on the mass, as they penetrated deeper into the interior, *the heat evolved was so great that they were compelled to desist from their work for two hours at a time, so as to suffer it to cool sufficiently to enable them to resume their labour, although they wore thick shoes.*"

After all this levelling and disturbing the mass in midsummer, upwards of 117,000 cubic feet remained, and the river continuing to fall, portions were gradually exposed to the sun which before had been under water.

"From the 10th of June," says Dr. Robertson, "to the last of August, the thermometer in the shade, at 3 o'clock P. M., ranged from 75° to 96°. The dryness of the summer was unparalleled; at one time there was not even a shower of rain for 71 successive days."

"Mrs. Gardner's is a short distance below the Trash Wharf." Her family was attacked about the 8th of June. John McGuire, taken with yellow fever on the 5th July, was a labourer engaged on this accumulation. The son of Mrs. Bush, taken July 5th, took shelter, from a shower of rain, under the platform a few days before. The first cases of the fever from Charleston arrived on the 27th of July.

The physicians of Augusta unanimously adopted the report and resolutions submitted with it, the first of which is as follows:—

"*Resolved, That, &c.*—we are of the opinion that the cause of the late epidemic was not introduced into our city, in any manner whatever, from foreign sources; nor do we believe the disease to have exhibited, in the slightest degree, a contagious nature."

The second resolution refers to the "Trash Wharf" and the 200,000 cubic feet of animal and vegetable matter; and the third to its entire removal. Dr. Robertson's whole report is most interesting.

Dr. Strobel refers to the report of the "Medical Society of Georgia," on the "Origin and Causes of the late Epidemic in Augusta, Georgia," only "to express our dissent from their conclusions," without giving a single reason; but he significantly inquires: "How happens it, that the Trash Wharf, and the low state of the river, have never before given rise to the disease—for, until this season, Augusta has been exempted from its attacks?" p. 188.

We will tell how it "happens." The report disavows the belief that the low state of the river alone produced the fever, for Dr. Robertson says expressly, p. 21, that it was not the cause.

"Again, in 1830, the river was nearly as low as it was during the past summer, and the bar on the Georgia side presented about as large a surface for the action of the solar heat, and yet our city was never blessed with a greater degree of health than during that summer and autumn."

The Trash Wharf was not in existence in 1830, but was commenced in 1834, and at a subsequent period was neglected. But the crowning piece of

folly on the part of the City Council was in disturbing the mass of animal and vegetable matter during the heat of summer, in the same way that the authorities of Charleston made extensive excavations in 1849, 1852, and we may add 1854, and with the same results.

In 1839, Dr. Strobel carried the yellow fever from Charleston to Augusta, then to Savannah, and says that, "finally, in September, we are told of the arrival of the Abel family at St. Augustine, from Charleston, and very soon after, the disease prevails epidemically in that place." Pp. 148—171, 212.

Now, it so happens that not a single member of the Abel family had yellow fever, common bilious fever, or any illness whatever, sufficiently severe to require the attendance of a physician, during the summer and autumn of 1839, in St. Augustine. It can scarcely be supposed that we would venture to say this, if there were not evidence in proof of the assertion, which will be produced hereafter; and we have no hesitation in affirming that, so far were the Abels from introducing yellow fever into St. Augustine, there was not a single case of it, or of any fever, in the family.

Dr. Strobel says that yellow fever was carried from Charleston to Savannah in 1839. No proof is offered. Dr. Arnold is supposed to know something about the fevers of Savannah, and we will hear what he says, October 4, 1852:—

"In 1839, which, like this season, was noted for the unusual quantity of fever all over the country, I saw several cases of yellow fever in our city, which I considered sporadic, and which were not in sufficient number to entitle it to be considered epidemic. Once in every five or six years I meet with a sporadic case."

So much for the importation of yellow fever from Charleston to Augusta, Savannah, and St. Augustine, in 1839.

1843. The fever broke out on Charleston neck, no yellow fever appearing in the city proper, or below Boundary (now Calhoun) Street, showing that the disease was local, and owing to local causes. Dr. Hume says that this epidemic was brought from Havanna by a Mr. E. E. Hughes, whose narrative is as follows:—

"On the 27th of July, 1843, I arrived in Charleston, in the brig *Hague*, Capt. Trescott, from Havana, suffering under fever. Dr. Simons came on board, and was told by Dr. Wurdeman (also a passenger) that no yellow fever was on board, he thinking that I had common bilious fever. The day before I left Havana, an Englishman, who soon after died, was lying for a few hours with a high fever on my mattress. I took him off with much difficulty, as he was delirious, rolled up my mattress, and sent it on board the *Hague*. I think I put it in my berth and slept on it during the passage. The mattress went with my baggage, and was given to a negro in the house. I was taken to the house northwest corner of Wolfe and Meeting Streets, as soon as possible after the vessel touched the wharf, where I remained until I recovered, fifteen days in bed, and then went to Aiken. The first deaths I heard of in my neighbourhood were two Dutchmen in a small house on the southwest side of Wolfe Street, nearly opposite my room."

This is circumstantial, and let us take the rest from Dr. Hume:—

"The first case admitted to be yellow fever occurred on the 14th of Septem-

ber (Dr. Toomer), on the southwest corner of Wolfe and Meeting Streets. The cause, of course, was referred to general and local conditions of the climate and soil. Heavy showers of rain had fallen at the end of August, and a hot sun had succeeded. There were present, perhaps, all the elements to constitute a bad atmosphere, such as has previously been engendered in this neighbourhood without producing epidemic yellow fever, but the specific modifying agent had probably been previously absent, but was present on this occasion, and produced the peculiar modification of fever which characterized the season."

First: Hewes's letter. It does Dr. Wurdeman injustice. Dr. Wurdeman never supposed the man to have "common bilious fever," but he knew, and so stated to Dr. Simons, Port Physician, that there was a man on board who had been drinking spirituous liquor much too freely, and had *delirium tremens*. Dr. Wurdeman never stated that this man, or any other man on board the Hague, had "common bilious fever," or any other fever. The man and the mattress went to the house, and were given (at least the mattress) to a negro, and this mattress, it is supposed, must be regarded as fomites; but we hear of no contagion from this source in the house or yard, which ought to have been the case if either the man or the mattress was the focus of disease. The remainder of Hewes's letter is of little consequence.

Next: Dr. Hume's statement. According to Dr. Hume, the first case of yellow fever occurred on the 14th of September. Dr. Wragg "reports the city and suburbs remarkably healthy until the latter part of September. According to Dr. Wragg's testimony, it was not far from two months after Hewes and the mattress arrived before the fever broke out on the Neck; and, according to Dr. Hume, forty-nine days, just seven weeks, elapsed from the arrival of Hewes to the first case, on the southwest corner of Wolfe and Meeting Streets. As well might the contagion have remained latent and smouldering from July to January, or from July to a succeeding July. If the period of incubation in yellow fever is seven weeks (forty-nine days), how long ought stringent quarantine to be kept up, for each case, in the harbour or port of a commercial city?

We will refer to the writings of contagionists for opinions in relation to the Bulam fever.

Dr. Stevens says:—

"The poison which causes the marsh remittent may remain in the system even for months before it produces fever; whilst that which is the cause of the African typhus (Bulam fever) generally remains dormant about four days, but when applied in a less concentrated form, it is often longer than this before it paralyses the heart to such a degree as to bring on the cold stage, which, in general, precedes the continued reaction."—*On the Blood*, p. 219.

Sir William Pym: "He may be infected by the Bulam fever in a close and warm atmosphere, at the distance, perhaps, of several yards; and in an open, airy situation, may approach a patient in the same disease, perhaps even to contact, with very little or no danger."

Sir William Pym says that the Bulam fever is "produced by a specific contagion, which, when taken into the system, carries on its work of assimilation until about the fourth day, when (with high fever) it exerts its influence upon the villous coat of the stomach," &c.—*On the Bulam Fever*, p. 219.

Dr. Chisholm, the father of the Bulam fever, uses the following language:—

“But whatever were the predisposing causes, the contagion always acted within four days from its application to the body. I am aware of the difficulty of ascertaining the time which contagion takes to act on the system after its admission into it; but my situation afforded me many opportunities of knowing it with sufficient exactness. In some instances, signs of its action have appeared in six hours; in others, in twenty hours; in others, in forty-eight; and in others, not before the expiration of the fourth day; so that, in general, we may consider the space of time required for the production of the disease, consequent upon the application of contagion, as a trifle short of two days.” Again: “Although the disease may be in the same house, avoiding the chamber of the sick prevents infection; merely entering the chamber of the sick, without nearly approaching the diseased person, has never communicated infection, &c. * * * * From hence it is evident that the infectious effluvia do not extend themselves beyond a limited distance from the person or thing from which they are emitted; and that this distance may be fixed, at the utmost, at six or ten feet.”—*On Bulam Fever*, vol. i. pp. 304, 305, 306, 310, 311.

Here we have the views of the standard contagionists in relation to the period of incubation in the Bulam fever, and also of the distance at which the contagion acts on the system—about four days for the stage of incubation, and ten feet distance, “at the utmost.” Meeting is one of the broadest and best ventilated streets in all Charleston, being nearly 70 feet (66) wide, yet Dr. Hume extends the latent period of this fever to forty-nine days, and makes the contagion active, across a well ventilated street, at the distance of 70 feet. The fever which Dr. Hume describes as being so contagious must have been the genuine Bulam fever, imported from the West Indies, for the indigenous yellow fever of those islands is never contagious, nor are the “sporadic cases” of Charleston. This was certainly a malignant and dangerous fever, having a stage of incubation of seven weeks, and being propagated across a well-ventilated street to the distance of 70 feet. Even smallpox, the most contagious of all diseases so denominated, could scarcely cross Meeting Street.

Heavy rains had fallen, and great heat followed. Not a street in this part of the city was paved or drained, and Wolfe, Reid, Mary, Columbus, and other streets in the vicinity, were in bad condition, as they always are, in regard to sanitary police. The railroad track, parallel with Meeting Street and next to it, has been already described as one of the most filthy spots in all Charleston. Here was fever, but it did not spread to other parts of the city. Here, according to Dr. Hume, imported contagion remained dormant; perfectly innocuous, exactly seven weeks; there was no case other than Hewes's on board the Hague; there was not a single case of yellow fever at the wharf where she lay; everything contagious, contingent contagion, the man and the mattress, went from the brig and wharf to the northwest corner of Wolfe and Meeting Streets; the fever did not break out in September where we would expect, at the northwest corner, but at the southwest; not a single case of the fever occurred, so far as we learn, at the northwest corner; it prevailed in only a small district of the Neck, its ravages being circum-

scribed; and it did not get into the city proper at all, notwithstanding the constant intercourse, and that "many who sickened were brought to the city and died."¹ A more singular account of the origin of a disease has never, we venture to say, been heard of, the floating bedding, near St. Augustine, 1821, as related by Dr. Strobel, not excepted.

A Charleston physician of reputation, in active practice, whose name would gladly be given were we at liberty, gave us the following account:—

"We, who do not credit that Hewes could introduce the fever into Wolfe Street and vicinity in the manner stated, believe the cause to have been (in addition to bad drainage and bad police, the intense heat, and the heavy rains) the cutting down and removing, in midsummer, a large mound of earth and rubbish, which had been allowed to accumulate. The fever was local, much of it severe bilious remittent, and no one took the disease, although several sick persons were removed to other and more healthy localities in the city."

ART. V.—*Pulmonary Lesion treated as an event arising in the progress of Constitutional Decline under the Tubercular Diathesis.* By J. P. HALL, M.D., of Glasgow, Kentucky.

THE topics with which I design more especially to deal have reference to the events to which tubercular, as distinguished from strumous diathesis, gives rise; but, under the conviction that there is here implied a distinction when there does not exist any essential difference, I will occupy a brief space in explanation of my peculiar views. But I must content myself with a bare statement of conclusions, without adducing the reasonings by which my position is supported.

To state that the habit or diathesis resulting in the elimination and deposition of cacoplastic and aplastic products—establishing pulmonary phthisis in one case, in another scrofula, and in a third tabes mesenterica or cachexia Africana—is identical, would be assuming grounds no broader than could be sustained by well-established facts, as I verily believe; yet such a view would seem to require qualification.

The above enumerated morbid states clearly present diversified pathological features, and have accordingly been arranged into nosological varieties; nevertheless, I regard them as the offspring of a common progenitor. The features by which the individuals are ultimately so easily distinguishable, are the result of, and determined by predisposition, temperament, manner of life, &c. The prevailing diathesis or dyscrasia constitutes a general disposition to disease, which the local manifestation or the peculiar tissue which may become the seat of structural lesion, in the progress of constitutional disorder, indicates the proximate pathological distinction.

¹ Dr. Simons.

From the remotest dawnings of medical philosophy up to the period when theories are shaped by the teachings of men of our own day, struma, phthisis, and other allied diseases have been, in some sort, treated of as bearing a certain relation to a pre-existing state of constitutional depravity. Cachectic habit, strumous diathesis, tubercular predisposition, &c., are the designating terms that have been employed, according as the incipient indications were to develop one or another of the definite forms of resultant structural disease.

In all of this there is an inherently distinct and dissimilar morbid element implied, capable of exercising a specific control over the events of disease, independently of constitutional idiosyncrasy. Now, to my mind, the proposition resolves itself into this: that, prior to the development of that assemblage of morbid phenomena, expressive of a local lesion and susceptible of a distinctive appellation, as tubercular consumption, scrofula, &c., there is not a vitiating agent at work endowed with peculiar and intrinsically dissimilar properties, but that diversities inherent in the economy and the peculiar influences brought to bear in the individual instance must essentially determine the specific character of the final disease.

I will now proceed to a consideration of that peculiar perversion of the nutritive functions resulting in tubercular degeneration of the pulmonary structure—regarding the pulmonary implication as merely constituting a prominent event, although usually a fatal event, in the progress of the general morbid actions.

We are not accustomed to witness that group of morbid phenomena, exhibited upon the supervention of material change in the pulmonary structure, constituting true phthisis pulmonalis, developed independently of a prior departure from health; yet, until signs and symptoms have demonstrated the existence of such structural lesion, we are taught not to recognize an instance of absolute disease, but rather a negative condition of health.

The doctrine to which I hold, and upon which I am obstinately inclined to act, is, that the organic implication is only a local event, developed in the progress of a series of general morbid actions, and to subserve a salutary end. But in order correctly to appreciate those laws, to the operation of which the dissolution of tissue is due, they must be studied in connection with phenomena displayed in the processes going forward in the sources which originate organized tissues.

According to the most generally received physiological doctrines, the ultimate structure of the animal tissues consists in minute cell-formations and cell-germs; that these primordial cells and nuclei are capable of reproduction so long as they are supplied from the blood with appropriate organizable materials; the plastic elements of this fluid being resolved into nucleated cells also. When the entire group of organic functions are so perfectly performed as to place the animal body in a thoroughly normal condition, there is a perpetual disintegration and reproduction of cell-structures; while one process of the economy destroys another repairs, so that as long as this state of healthful

integrity obtains there can take place no material diminution of the original cell supplies, but any interference to the functions of nutrition must necessarily arrest their production, and, as the process of disintegration is still going forward, the result cannot be otherwise than a rapid waste of tissue.

Hence the primary forms assumed in the processes of developing solid animal tissue are traceable to a sanguiferous source, thereby assigning to the circulating current an instrumentality of such essential importance, that without it vital changes must at once stop, and vital phenomena cease to be displayed. The blood, then, is the universal *pabulum vitæ*, the grand source of formative supply for the fashioning of the epithelium cilia to the formation of those intricate structures whose exercise gives rise to the complex phenomena of voluntary motion and thought; while from the same source are drawn materials for continued structural maintenance.

But that the purposes of normal organization be answered, a completely balanced set of machinery is requisite, as well as a due supply of assimilative materials; the latter, by the exercise of the former, to be elaborated and converted into those primary tissues whose aggregation forms a complete organism.

A set of organs otherwise capable of the most complete assimilative integrity, without appropriate organizable elements subjected to their elaborating functions, and the formative process, must prove an abortion, no less than would be the case on the other hand with a due supply of the materials of nourishment in the absence of functional apparatus adequate to the changes to be consummated in assuming the forms and properties of vital tissue. For an organism, after being placed in circumstances every way conducive to its physiological welfare, to maintain these circumstances implies the exercise of very many vital as well as physical forces; but to particularize minutely would be foreign to my present object.

The conditions essential to the elaboration of a pure current of vital fluid, the changes through which this fluid must pass in view of its adaptation to the physiological wants of the economy and the pathological modifications of which it is susceptible under the multiplicity of morbid influences to which it is exposed, constitute elements of prime interest to an inquiry such as we are engaged with; but opportunity permits me to devote, at present, only a passing remark to some of them.

The component elements of the plastic fluid, by which the several corporeal tissues are traversed, and from which they derive their supplies for growth and renovation, are supposed to exist in such relations as will best fulfil the purposes to be answered in the healthy economy. At a given period in the progress of functional events, this equable and perfect relation no longer subsists between the sanguiferous elements; the fibrinous element ceases to be duly elaborated, as evinced in the impaired tone and inelasticity of the tissues; the abnormally low proportion of red corpuscles gives rise to pallor and other indications of a general state of asthenia.

The rich materials of nutrition formerly abounding in the vital fluid are no longer retained; the cell-germ, which was formerly conveyed to the remote tissue and deposited as the nucleus of a plastic structure, has been replaced by the degraded element, susceptible of no higher organization than the cacoplastic and aplastic product. The ordinary nutritive process has undergone a complete metamorphosis; the plasticity of the blood has degenerated into dyscrasia; and the plastic forces, instead of their wonted compliance with the laws of normal organization, are now addicted to flagrant vices. If we consider how prejudicial to the vital apparatus must be the effects exerted through the blood, thus vitiated, conveying to the remotest recesses of the organism extraneous materials of a highly offensive character, we may readily perceive how absolutely indispensable to the continued operations of the economy is the office to which nature has doomed the lungs, or it may be a less vital organ. The economy sustains not only a negative injury in the arrest of plastic forces growing out of a deficiency of organizable constituents, but the positive evils resulting from noxious elements present, demand their speedy withdrawal, and the organ or organs evincing the greatest proclivity to participate in the progressing decay become a receptacle for the effete deposit.

As would, *à priori*, be expected, the great depurating organs, the lungs, seldom fail to become the chief instruments in the process of ridding the system of the excrementitious materials we have described. And if we reflect how vitally dependent upon the integrity of the pulmonary function is the adequate preparation of the blood for the uses which it is destined to serve in the economy, we are at once prepared for the series of perverted actions and torpid functions made manifest. The abnormal elements which give rise to the blood-dyscrasia finding efficient emunctories in the lungs, the vitalizing functions of these organs become impaired in consequence of the impediment offered to respiration by the amorphous deposit, which, by still further depraving the quality of the nutritive current, accelerates the elimination of plastic elements, thereby redoubling the burthen of the emunctories, either disposition reacting upon and aggravating the other, or alternately becoming cause and effect, until the elaborating power is paralyzed, and molecular nutrition permanently arrested.

The proximate element of disease in the peculiar constitutional habit characterized as tubercular diathesis, under which the functions of the body are so extensively involved in deviations from their wonted exercise in health, consists in the transformation of plastic blood into a fluid not only notably deficient in organizable constituents, but prone to the elimination of products in the highest degree embarrassing to the operations of the vital apparatus. Those materials so essential to the maintenance of a perpetually changing mechanism are not merely withheld, but the blood has become the medium of bringing into contact with the delicate tissues ingredients directly subversive of physiological relations, and capable of effecting the dissolution of

existing' tissues. Such organ as may predominate in selective affinity for the degraded element becomes a scavenger for the depraved circulation, affording a receptacle of deposit for the eliminated tuberculous matter.

Although it may seem a melancholy alternative, yet the organic lesion is an event of absolute necessity; an event salutary in its design, but direful in its consequences. In the progress of a pre-existing constitutional malady there has been created an office, associated with which are duties the performance of which involves the self-sacrifice of whatever organ assumes them, yet they must be performed, else speedier and more certainly fatal results ensue. Should this fatal service have been assumed by the pulmonary organs, as by far most frequently happens, upon the lapse of a period of uncertain duration, the function of these organs becomes manifestly impeded; the work so extensively involving the welfare of the body, even its existence, ceases to be adequately performed; efforts are now provoked for the expulsion of the accumulating burthen, and it is expelled; but alas! the source from whence the deposit is supplied yields with continually increasing lavishness. The *materies morbi*, which could not, with impunity, be retained, diffused throughout the circulating current, must prove a canker to the reservoir upon which it is concentrated; and at last nature's provision, which for a time held forth the promise of protective relief at least, becomes impotent; the lungs decay; the vital fabric, attenuated and exhausted, succumbs.

Thus it is that the hand, so gently laid on at first, gradually and insidiously but firmly tightens its grasp until the victim is completely crushed.

The morbid alterations which have been but imperfectly traced clearly originate in a lesion of nutrition; but this lesion does not, in every instance, by far, lead to the development of the event which entitles the morbid state to the denomination of tubercular consumption. But if the incipient tendency does progress to the establishment of the event, or pulmonary lesion, we may uniformly, by retracing the steps by which disease has advanced, convict the nutritive system of delinquencies, bearing, to the *event*, the relation of primary cause. Not a single essential morbid change arising in the progress of the original diathesis could have been developed independently of this relation. No process of argumentation could demonstrate to my mind that a tubercle was ever formed independently of a prior state of disease.

A tubercle is an eliminated morbid product, and must of necessity be the result of a morbid action engendered by a still further antecedent diseased impulse.

Having already extended this communication beyond the limits originally contemplated, I will conclude by offering a few very general reflections as to therapeutical relations.

The first indication is to remove the constitutional disease, or rather correct the diathesis; thereby averting the untoward event which its final progress involves, namely, organic lesion of the respiratory apparatus. If this event should not have been forestalled, the second step indicated is still to reclaim

the constitution from original vices as an absolute condition to the permanent arrest and repair of organic lesion.

The engrossing idea of structural lesion must give place to more extended principles of investigation, else therapeutical deductions arrived at can only serve to alleviate and retard, while the problem of restoration must remain unsolved in reference to the ravages of tubercle.

Remedial systems must be predicated upon a broader basis than is afforded by stethoscopic disclosures. So far as regards a strictly pathological diagnosis, no one has a higher appreciation of the utility of this instrument than myself; but when the diagnosis sought is one from which wholesome, sound, therapeutical deductions are to be drawn, its value rapidly degenerates into inutility. The doctrine that I hold, and which I would inculcate, is, that the disease designated pulmonary phthisis does not originate in the pulmonary structure; that the structural lesion here is an event rendered inevitable in the progress of a pre-existing morbid habit; and that no process of exploration, it matters not with what degree of accuracy the condition of the lungs may be revealed, if thus restricted, can materially aid in supplying data for efficient remedial procedure. In this I would not be understood to intimate that the state of the lungs should be unheeded, or that this accurate method of determining what that state is must be discarded; yet such information, for the most part, only enables us to estimate the gravity and general import of the case, while an expansive field beyond must be scrutinizingly traversed for such discoveries as can be made available to the aims of salutary treatment.

Therapeutical measures must be shaped with the fact constantly in view, that the structural lesion is a consecutive event, and that the indications to be fulfilled are derived, not from the manifestation of a single organ upon which stress has incidentally fallen in the progress of a more general pre-existing functional lesion, but the expressions of a morbid organism constitute the indications to be sought and obeyed. The process of organization begins with the cytoblast, and here it is that the first link is wrought in the chain of morbid events, displayed in the progress of those functional and organic changes which mark the stages of tubercular consumption from the incipient diathesis to the period of its latest ravages.

There is, probably, no other disease, the history of which furnishes a commentary upon medical art, more justly humbling to the pride of its votaries than the one under consideration. No lengthened argument, no array of authentic testimony, is wanting to substantiate the assertion that the present state of our science affords no resources worthy to rank as remedies in the armory of therapeutical weapons.

The most judiciously ordered remedial systems which science, wisdom, and experience have been capable of devising, have most signally failed in conferring upon the melancholy victim of consumption the welcome boons which it is the mission of the healing art to bestow. When we remember that this insufficiency of our art leaves to an undisturbed progress a relentless monster

whose ravages so fearfully contribute to abbreviate the span of human existence, we cannot, as members of an enlightened and philanthropic profession, fail to be impressed with the exalted nature of every effort, the tendency of which is to advance us a step in the pathway of useful discovery. The inquiry is a noble one, and will conduct the inquirer to a fertile field; but he must go forth prepared to break the surface.

This article has unavoidably partaken of a discursive character; many highly interesting details have been altogether omitted, and those noticed have been introduced regardless of system or order; but if the line of inquiry which I have attempted to indicate has been made obvious, more especially if the claims which I am persuaded should be allowed it have been in the least advanced, my main object has been achieved: for I verily believe, and willingly commit to record the opinion, that if in the progress of medical discovery, the great desideratum—the successful treatment of tubercular consumption—which has so long and so effectually baffled the champions of the noblest science relating to earthborn things should yet be attained, the solution of the mysterious problem will not be due to the stethoscope, auscultation, or percussion.

ART. VI.—*Inquiry into the Physiology of the Organic Nervous System.*

By ISAAC CASSELBERRY, M. D., Evansville, Ind.

EVERY material of the universe consists of *matter in motion*. This motion is termed *force*. Heat, light, and electricity are only states of matter. When a bar of iron is heated, its molecules are thrown into undulations which are propagated with definite form and character to contiguous particles of atmosphere; and from them to the hand or thermometer.

The motions of light, electricity, sound, colour, &c., are entirely analogous to those of heat, and the manifestations which they all present are dependent on the form of the substance from which they are evolved.

The causative motions must not be supposed to involve any transference of particles from point to point, which is the kind of motion most commonly recognized by our senses. There is a wide difference between the motions of a wave and the motions of the particles among which it is passing.

When we disturb the middle of a watery surface, circular waves are propagated from the centre to the circumference of the surface. A feather, or other light body placed upon the waves, is not drifted forward with them, but it remains motionless. We know nothing of matter itself; all our knowledge of it is confined to its motions. There is no such a thing as a vacuum; the term has only a relative import. A substance is heavy or light, hot or cold, wet or dry, green or red, according to the form of motions

which emanate from it, and impress our senses. The laws and phenomena of motion are, therefore, the fundamental elements of all inquiry.

The various motions of matter are not only analogous, but mutually convertible. Prof. A. D. Bache has shown conclusively that light may be converted into heat; colour alone does not modify the radiating power of a surface. Expose cloths of the same texture, but of different colours, on a surface of snow to the action of sunlight, and the quantity of heat absorbed is the same for all the pieces. But the quantity of light absorbed by the black cloth is evidently the greatest, because none is reflected; and it is the black cloth which always sinks the deepest into the snow. More heat has, therefore, gone from the under surface of that cloth into the snow than from any of the others, although no more heat was received by it as heat. What has become of the light, and where did the increased heat come from?

The light cannot be said to be *latent*, because by no possible means can it ever be reproduced from the cloth. The undulations of light must have passed into the cloth, modified by its organic texture, and it was absorbed by the snow as undulations of heat.

The original heat from the sun received increments from each cloth, but most of all from that which reflected no light; because all the light passing into that one was converted into heat.

Friction, which is only impeded motion, is productive of all the physical forces. When two smooth surfaces are rubbed together, heat is evolved; if there be any inequality, electricity is evolved; and if the motion apparently lost in resistance be very considerable, light is also generated. Now this motion is not lost or annihilated by friction, but it has passed into the forms of heat, light, and electricity; for the quantity of these produced depends exactly upon the degree of motive force expended in the friction.

This theory applied to the facts of caloric evinces the fallacy of the obscure doctrine of its *latency*. Boiling water and steam are always at the same temperature, 212° F. But steam in being condensed into water gives out 950° F. of heat; that is, it requires the mechanical force of 950° F. of heat to convert water into steam; and, by condensation, this force is again returned to its equivalent condition as 950° F. of heat. In the conversion of water into steam, the heat is, therefore, changed into mechanical force.

Heat also produces electricity and chemical actions; and both may be increased or diminished by the increase or diminution of the caloric, which is undergoing the change into these new forms of force.

Chemical affinity produces heat, light, electricity, magnetism, and mechanical power. This is strongly exemplified in the explosion of gunpowder.

In all these cases, the amount of new force generated is exactly proportional to the rapidity and extent of the chemical action.

The transmutation of electricity into heat, light, chemical affinity, and mechanical power, is so common and obvious, that philosophers regard that agent as the predominant element in nature. While the forms of matter are

continually changing, the matter itself is persistent. None is ever created, and none is ever lost. The motions of matter are, however, in a state of incessant mutual transmutation. Every organized being, vegetable or animal, gives back to inorganic matter, not only the material, but the forces which it received from it. The forces of nature are motions of matter capable of being submitted to mathematical analysis, and mutually convertible into each other. The laws by which heat and light are radiated, reflected, absorbed, and polarized are identical.

Faraday has demonstrated that the five species of electricity—the common, the voltaic, the magnetic, the thermal, and the animal—are identical. That great electrician has also demonstrated the identity of electricity and magnetism, to the entire satisfaction of all philosophers.

The influence of heat on magnetic bodies, and of light on the vibrations of the compass, completes the link of the occult chain which binds all the forces or motions together. These facts demonstrate the *unity of forces*.

Those physiologists who do not recognize the convertibility of forces have been obliged to affirm, that the *vital force* exists in a dormant condition in all matter capable of becoming organized.

In the lowest form of the vegetable germ-cell, heat and light are required as specific stimuli, prior to the commencement of development. The rate and extent of development are exactly proportioned to the amount of luminous and calorific forces appropriated.

According to Boussingault, the same annual plant in arriving at its period of development, and in going through all the processes of flowering and maturing its seeds, everywhere receives the same amount of solar light and heat, whether it be grown at the equator or in the temperate zone; its whole period of growth being in a precisely inverse ratio to the quantity it received in any given time, and its rates of growth consequently in a direct ratio.

Hence, it appears that the organizing force of plants bears a relation of equivalence to the light and heat which act upon them. The agency of light is particularly directed to the fixation of carbon in the vegetable structure. The amount of carbon fixed is always in accordance with the degree of illumination it receives. The maturation of the seeds of plants, and the eggs of insects, may be at pleasure retarded or accelerated by the mere regulation of temperature. Dr. Edwards has made some experiments upon the tadpole, which show the agency of light in organization.

When carefully excluded from the sunlight and well supplied with aerated water and food, they grow to twice, and even thrice their size as tadpoles. But they underwent no change into frogs. Mr. Higginbottom's experiments confirm those of Dr. Edwards. According to Dr. Draper, the chemical rays or undulations excite or determine the arrangement of the molecules into particular groups, so as to produce developments. The inference from these experiments, and a vast mass of similar facts which might be adduced here, is, that the *external* forces of nature, particularly light and heat, passing into

organized structures, become converted into *new manifestations* of force, which are termed assimilative, organizing, reproductive, &c. Chemical action and mechanical motion constitute the fundamental basis of all these processes. By the former an extensive range of new compounds is generated, such as have never been imitated in the laboratory of man. Light and heat are the forces, which, acting through the vegetable and animal cells as their material substratum, produce these new chemical attractions, which determine the formation of the new compounds.

In other words, a portion of the force known as heat and light ceases to exist as such, and is manifested in a modified form, as chemical affinity, which again, in the act of decomposition, is reconverted into heat and light.

All the external forces are in this manner transformed by the organic nervous force into the different organic forces or processes manifested in the human organism.

Vitality in man is produced and maintained by the organic nervous system. The highest production of vitality in any individual, plant, or animal, is the production of an organic substance or being, capable of beginning in a new individual the wonderful circle of phenomena which characterize the life of a parent. There are two incident actions necessary to accomplish this, the elaboration of an ovule or organic cell by the female, and the impinging upon that cell of another cell, the spermatozoon, secreted by the male. The spermatozoon transfers the incipient force to the organic processes of the ovule in which it produces a series of transformations of the parent cell, which results in embryonic development.

The spermatozoon is a secretion; it is vitalized matter in motion.

As the pollen is the specific form for the embodiment and transference of organic force from plant to plant, so is the spermatozoon that of the organic nervous force of animals. The spermatozoon presents the anatomical appearance of a nerve vesicle and its filamentary appendage. It is not improbable that the infinitesimal motions going on in its globular portion have the precise mathematical character of those which occur in the vesicular, so that not only physical peculiarities, but *hereditary* diseases, are transferred *materially* from parent to offspring. The organic nervous centre is the first tissue formed in the embryo.

Marchesseaux, in his *Nouveau Manuel d'Anatomie Generale*, says, that, at this stage of development, we notice a fact which confirms with irresistible power the doctrine of the centripetal succession of organs. Each one of these germinating sacks consists of three layers or laminae, differing in nature. Of these, the external or *serous* always begins to organize first; and from it arise successively the spinal cord, brain, vertebra, cranium, the organs of sense, and their dependencies. When the external lamina has thus sketched out the forms of the organs of animal life, the middle or *vascular* lamina commences in its turn, and in a similar manner marks the outlines of the peripheral vessels, the *venae cavæ*, the aorta, and the heart. Up to this period,

the internal or *mucous* lamina has been inactive; but now its movements begin, and we see it successively delineate the alimentary canal, the lungs, the liver, the spleen, &c. This order is invariable; universally does nature proceed in this manner. This is given as a part of the *resumé* of the vast embryological researches of Serres and St. Hilaire.

Prof. Agassiz, in his lectures on comparative embryology, accords with this description.

Ackermann, Rolando, Blumenbach, Gall, and others, state that the solar ganglion is the tissue or part first formed in the foetus. What Blumenbach, Elliotson, and other physiologists claim for the *nisus formativus*, is here claimed for the solar ganglion, the centre of the organic nervous system.

Dr. Carpenter, in common with other distinguished physiologists, says, that all the phenomena of cell-growth are explicable on the hypothesis of the *convertibility of forces*.

The more obvious motions of the animal are evidently dependent upon the nervous systems. Everything to which the term motion can be applied, in man and animals, belongs to the nervous apparatus. Both the voluntary and involuntary muscular contractions are a continuation of the organic nervous force. The simplest and most universal of these motions are a series of cellular transmutations usually known as capillary attraction and ciliary motion. These perform an important part in the changes of all organized beings. Matteucci has demonstrated them to be chemical attraction.

Both this attraction and chemical affinity can be shown to be remote modifications of heat. Capillary attraction can be accelerated or retarded, by increasing or diminishing the temperature. The correlation between electricity and heat has been demonstrated. Becquerel asserts that electricity is a constant attendant upon the phenomena of capillary attraction. Ciliary motion, although seldom discovered except with the microscope, is common to plants and animals. Nothing seems absolutely indispensable to their activity, but the integrity of the epithelial cell to which they are attached.

Dr. Carpenter thinks that this ciliary motion may be a partial expenditure of the organic force derived by *convertibility* from the solar emanations. He considers this view to be remarkably confirmed by the fact that, in the history of the zoospores of the algæ, we have two distinct periods, one of ciliary motion and the other of growth and multiplication. So long as the ciliary action continues, no further organic changes take place in them; but so soon as this ceases, and they become stationary, they begin to exercise chemical transformations and to grow and multiply as cells. In the higher order of living beings, ciliary motion is the expression of the excess of organic force in the subjacent tissues. The contraction of involuntary muscles is independent of any connection with the cerebral and the true spinal nervous systems. The *inherent irritability* of muscular fibre, as taught by Haller, is still the prevalent doctrine of the schools. But if the mutual convertibility of forces has any foundation in nature, this *inherent* or *latent* irritability is as

unphilosophical as latent heat and light, or latent force of any other kind. The force which produces muscular contractions is not *educed* from the muscles, but it is *transferred* to them. The cells which constitute muscular fibrillæ can undergo no modification but shape; they exercise no power of chemical transformation. They undergo no histological changes, and appear to be entirely destitute of the power of self-multiplication. The expenditure of their organic nervous force in the single act of contraction involves their death and disintegration; their renewal is accomplished by the production of new cells from the myolemma, which itself possesses no contractility. The force which produces the contraction of the muscular fibrillæ must therefore always be *transferred* to them by the organic nervous branches. It is for the conservation of this nervous system that the anatomical and histological tissues are added to it as embodiments and properties. It is the seat and source of their vitality. They are regulated and maintained in co-ordinate life by its force.

Neurine, which is the technical name for nervous matter, is of two kinds, the *vesicular* and the *tubular*. The peculiar power of the nervous system lies in the vesicular portion; it generates force, while the tubular only conducts it. Solly and others have aptly compared the vesicular neurine to the secreting cells of a gland. It eliminates a force from the blood as the nucleated cells of the liver or testicles eliminate new and different organic materials from the same substance. The tubular neurine has been compared to the efferent ducts of the glandular structures, because it is designed to convey something from one point to another. The creation of organs in the embryo is precisely identical with the nutrition of organs in the adult. The original organic cell of the ovule receives the organizing force of the spermatozoon, and reproduces from the surrounding amorphous matter, by assimilation, a vast number of similar cells with their filamentary appendages of tubular neurine. These occupy the points which will afterwards be occupied by the various peripheral organs. The cells are provisional centres, and act independently. They eliminate organic nervous force from the maternal blood, and construct the tissues at the terminus of their tubes or nerve-trunks, just as in the adult body. The growth or extension towards the central axis is effected by the successive addition of cells like beads attached to beads; the last bead or cell formed being the provisional centre of the thread, the remaining cells running together by coalescence into tubes, as in the case of the arteries.

Thus each nerve is separately constructed, and they all converge simultaneously towards the solar ganglion, the centre of the organic nervous system. They do not, however, reach that axis simultaneously, for they may be imperfectly developed; the original order of the different formative processes may be perverted or suspended. A total arrest of development would give us no centres; it would be death. A partial arrest, however, might give us a spinal cord and no brain, or a brain and no spinal cord. The thoracic

viscera may be imperfectly developed, as evinced in an interesting case published by Prof. Byford. In the remarkable case cited by Dr. Hall of a foetus born without either brain or spinal cord, yet perfectly developed, we have a very satisfactory proof of the manner in which the organic nervous system is first formed, and of its independence of the cerebral and true spinal nervous systems. In the very interesting case of acephalous monstrosity published by Mr. Lawrence, in which the brain only was wanting, we learn that all the organic and true spinal functions were performed with integrity. The existence and function of provisional cells or centres, which disappear entirely when their use is fulfilled, are well recognized in general and special physiology. The motion of the blood begins in the vascular area of the middle lamina, some time before the formation of the heart. This motion is from the periphery towards the centre, which is afterwards occupied by the heart. Nothing so fully and consistently accounts for this motion as the transmutation of the organic nervous force from a provisional centre. These provisional cells are to the capillaries what the cardiac centres are subsequently to the heart itself. "There can be," says Carpenter, "no reasonable doubt that the production of nerve force in the central organs is dependent upon the development of the peculiar cells constituting the ganglionic or vesicular neurine; and, as already remarked, the progress of physiological inquiry seems to justify the belief that either cells or cell-nuclei are agents in the origination of nerve-force at the peripheral extremities of the nerve fibre." The spermatozoon is a secretion from the testes of the male; it is organizing matter in motion, which is *force*. This force is communicated to the female ovule, which is organizable. An uninterrupted series of cellular formations and transformations produced in the organizable matter or force of the ovule results in the development of the embryo. When matured, the relation between the mother and the embryo, or foetus, is dissolved by the process of parturition. The organic functions of the infant retain the organizing force; but the organizable matter is no longer supplied by the maternal blood. It must be derived from external sources, and introduced into the infantile stomach.

The organic nervous system may be divided into *three* series: the *first* connects with the nerves of the cephalic domain, whether cerebral or true spinal, and passes with them to the muscles and the viscera; the *second* plunges directly into the viscera; the *third* embraces the arteries and veins, and forms a plexus around their tunics, surrounding and penetrating them to their most remote distribution. The cerebral branches of the organic nervous system are continuous with the cerebral and true spinal nerves, and must be considered as the originators and conductors of all impressions, whether normal or abnormal. The organic branches communicate impressions or emotions of the mind to the cerebral nerves, by which it is conveyed to the cephalic centre, and by reflexed action conveyed back to the tissue or organ first impressed. They perform a similar function in relation to the true spinal

system. Every emotion, every sensation, which takes place in the human organism, must be attended or preceded by a change of matter or force, produced by the organic nervous force. The cerebral nerves can produce no sensation, except by means of the cerebral branches of the organic system. Neither can the true spinal perform any excito-motory function, except by means of the organic branches which are continuous with the inter-communications between the cerebral and true spinal nervous systems, and may produce a reciprocity of stimulation or debility, by which the functions of either one of the three may be increased or diminished.

The organic system can perform its functions independently of either or both the other systems. In support of this may be adduced the well-known cases of monstrosity related by Mr. Lawrence and Dr. Hall. In the interesting case of acephalous monstrosity published by Mr. Lawrence, the brain only was wanting; all the excito-motory functions were duly performed; it gave evidence of pain, and at first moved briskly. The sphincters performed their office. This, of course, was referable to the integrity of the true spinal nerves generally. The child breathed, and its temperature was natural. It discharged urine and feces, and took food; the latter circumstance indicated the operation of the functions of the organic nervous system and its collateral parts, without any aid from the brain. In the remarkable case cited by Dr. Hall, of a fœtus born without either brain or spinal cord, yet perfectly developed, we have a very satisfactory proof of the independence of the organic functions of the cerebral and true spinal nervous system.

The fact has been anatomically demonstrated that the solar ganglion, the centre of the organic nervous system, is formed before the existence of any vascular formation; that branches of the organic nerve surround and penetrate the coats of the bloodvessels to their most minute ramifications; and that they augment in size and increase in number commensurate with the function these vessels are designed to perform in the animal organism.

The cerebral, the true spinal, and the organic nervous systems, regarded separately as the centre of the several nervous systems of man, though executing distinct functions in the economy of life, are, by means of the innumerable ramifications of their subordinate parts, and their union with each other, brought into so close a relation that their physiological separation is impossible. Though they are not dependent mutually, they, nevertheless, exercise such a reciprocity of functions that the health and well-being of man are regarded as inseparable from their union. They may be compared to a tree, the root, the trunk, and the extremities of which may be said to represent them; for the root has been the source, and continues to be the life of the other two; it existed without them, and might again do so, though during their maturity it fails not to be a recipient of their toil. The root will sometimes live without the trunk and its extremities, and so will the paralyzed idiot. The organic nervous system exercises the architectural power which is employed in man and in animals to exhibit the wonderful and suc-

cessive changes which characterize not only intra and extra-uterine existence, but also normal and abnormal actions.

The organic functions in the lower class of animals, which have neither brain nor spinal marrow, are not less completely performed than the same functions in man. Secretion, nutrition, &c., are as elaborately performed in the polypus and oyster as in the mammalia.

The organizing force derived from the spermatozoon of the male, together with the heat and organizable force of the blood of the mother, are subdivided into all the organic affinities and molecular motions which occur in the development of the embryo. At birth all the tissues are formed and the blood in circulation, and as soon as the lungs absorb the atmosphere the organic nervous force becomes the universal and predominant power in the animal organism.

The organic force of the organic nervous system has the same relation to the whole organism that the steam has to the several parts of the engine. The organizing force and the steam are equally the motive power. To their different states must be attributed every kind of change of which the animal organism or the machine itself is under any circumstances susceptible.

The elements of the blood are generated, elaborated, and depurated of their effete constituents by a series of cellular formations and transformations by the organic nervous force, which manifests itself in the form of affinity, nutrition, or attraction, by which each nutritive element is attracted by identical elements in the elementary composition of the tissue of which it is destined to become a component part by cellular formation and coalescence. Every tissue of the organism is, in this manner, formed and nourished by its appropriate element or elements from the blood. It will be observed that I maintain that the blood is formed by the organizing force of the organic nervous system derived from the spermatozoon of the male out of the ovule of the female, which is organizable; that both of these are secretions; that out of these embryotic life is developed; that, when foetal life terminates at maturity by birth, the organizable matter is furnished by the food of the infant or adult, which is decomposed by the organic force of the organic nerves, and converted into a new compound; that this compound undergoes a series of conversions and elaborations by the organizing force of this nervous system, by which it is converted into the various secretions, tissues, and forces of the animal organism; and that all these changes of organic matter into living organized matter, or into vitality, takes place in the capillary bloodvessels.

That the functions of all the bloodvessels—arterial, venous, and capillary—are generated, controlled, and maintained by the organic nervous system, appears evident from the anatomical fact that the solar ganglion, the centre of the organic nervous system, is formed before the existence of any vascular formation; that branches of the organic nerve surround and penetrate the coats of the bloodvessels to their most minute ramifications into the capillary system; that these vessels have no other nervous endowment; that the

organic nervous branches augment in size and increase in number commensurate with the function these vessels are designed to perform in the human organism, and that this function is independent of that of the cerebral and true spinal nervous systems. These nervous systems impart increased energy to the organic functions, as light, heat, and electricity do to the vegetative.

Pathology teaches us that the organic processes proceed in those parts of the organism in which the cerebral and true spinal nervous systems are paralyzed exactly in the same way as in other parts where these nerves are in a normal condition. Sir B. Brodie shows, by his experiments, that the destruction of the crural and sciatic nerves, and even the lumbar spinal cord, neither retarded nor impaired in any way the reparative processes in the lower extremities; thus, wounds and fractures made in the limbs, deprived of cerebral and true spinal nervous influence, healed and united as readily as under all ordinary circumstances. The organic nervous branches distributed to the extremities were normal, and the organizing force of these branches caused the wounds and the fractures to unite. Many other cases in illustration might be adduced.

Secretion.—The literal meaning of the term secretion is separation. The process of secretion is performed by the agency of *cells*. How complexed so ever the structure of a secreting organ, these simple bodies constitute its operative parts. The process is strictly analogous to that of nutrition, since *every cell* in the progress of its development forms certain peculiar products out of the alimentary materials supplied to it; and just as the cells at the extremities of the villi select from the chyme the nutritive portion which is to be introduced into the absorbent vessels, so do the cells which line the secreting tubuli select from the blood the effete particles which it is their peculiar province to assimilate, and discharge them into the canals by which they will be carried out of the organism. There are not, therefore, two organic processes going on at the same time in the same gland. The only difference between this kind of growth and that which occurs in other organs is that a portion of the product is thrown out of the organism from the anatomical condition of the part. There cannot be a better illustration of this view than the nature of fat, the production of which is exactly the intermediate link required to connect the two processes.

The adipose tissue consists of cells, by the action of which the fatty matter is elaborated from the blood instead of being thrown out of the organism. It remains stored up in their cavities until it is required for use within the organism; and it must then be taken into the circulation by a process resembling the first absorption of aliment.

A certain portion of fatty matter is normally formed in the secreting cells of the liver, and this quantity may be very much increased, as Mr. Bowman has shown, especially in diseases which obstruct the pulmonic circulation. The fat elaborated by the cells is destined to be thrown off from the system;

and thus we perceive how much the anatomical position of the cells have to do with the function of secretion. It can scarcely be questioned that the chief source of the secretion is to be formed in the continued decomposition of the various tissues of the organism. They are derived more from the blood itself than from the fluids returned into the blood by the lymphatics. During the circulation of the blood, it parts with one portion of its constituents in one tissue of the organism by the transformation of the cells containing them, and with another, in a different tissue by a modification of the same process. The abstraction of organizable matter does not occasion any chemical change in the composition of the blood, because the fibrin and red corpuscles which are thus removed are continuously renewed at the expense of the albumen; while of the albumen, a new supply is absorbed by the absorbent system. The elaboration of gelatin, however, which is deposited so carefully in the solid tissues, must produce considerable alteration in the blood, since in its production from albumen a certain residuum must be left. This residuum is, probably, another source of the products of secretion. In several other instances, peculiarities of secretory action in different tissues will deprive the blood which passes through them of its due proportion of certain of its elements; these are partly restored by its admixture in the heart with the blood which has been returned from other parts; but still a general alteration in the character of the blood is the result of its circulation. For this alteration it is the province of the secretory functions to compensate.

A striking illustration may be found in the change of colour, and in the proportional amount of free oxygen and carbonic acid, which take place in the systemic capillaries by the normal transformations of the cells containing the elements of the blood. Hence it may be regarded as a physiological fact that no chemical change can be produced in the products of any secretory process except by a chemical change in the elementary constituents of the blood in that tissue by means of their cellular mutations. Medicinal agents must, therefore, induce cellular formations and transformations in the elementary constituents of the blood in the capillaries in any gland or tissue, before they can chemically change the products of the secretory function of that gland or tissue. No therapeutical agent can produce any effect unless it chemically alters the elementary composition of the blood. This is the only mode by which medicinal agents can act therapeutically.

The *heart* is a muscular organ. It is usually regarded as the motive power of the circulation. But no physiological fact is more clearly proved than the existence in the lower classes of animals, as well as in plants, of some power independent of the *vis à tergo*, by which the circulating fluid in man is caused to move through the vessels. This power seems to originate in these ultimate tissues, and to be closely connected with the state of the nutritive and secretory processes, since anything which stimulates them to increased energy accelerates the circulation, while any check to them occasions a corresponding stagnation.

For convenience, this motor force may be called *organic nutritive attraction*. The movement of the blood through the capillaries is in a great degree, if not entirely, independent of the action of the heart; since it may continue after the heart's action may itself cease in particular organs, when the heart itself is still acting vigorously; and it is constantly affected in amount and rapidity by causes originating in the part itself, and in no way affecting the heart. The movements of the blood in the capillaries of cold-blooded animals after complete excision of the heart have been repeatedly observed. In warm-blooded animals this cannot be satisfactorily established by experiments, since the shock occasioned by so severe an operation much sooner destroys the organic nervous force.

After most kinds of natural death, the arterial system is found, after the period of a few hours, almost or completely emptied of blood; this is partly the effect of the contraction of the arteries themselves; but the emptying is usually more complete than could be thus accounted for, and must, therefore, be due to the continuance of the capillary circulation. When death takes place suddenly from such a cause as an electric shock, which destroys the organic nervous force of the whole organism, the arterial tubes are found to contain their due proportion of blood. It has been well ascertained that a true process of secretion not unfrequently continues after the cessation of respiration, and the action of the heart and arteries. Urine and perspiration have been secreted, and other peculiar secretions formed in the glands. These changes could not have taken place unless the capillary circulation were still continuing. In the early embryonic condition of the highest animals, the movement of the blood is unquestionably due to some diffused power independent of any central impulsion; for it may be seen to commence in the vascular area before the development of the heart. The first movement of the blood is towards the central organ, or heart, instead of from it; and even for some time after the circulation is fairly established, the walls of the heart consist merely of vesicles loosely attached together, and, therefore, cannot have any great contractile power.

Cases are of no unfrequent occurrence in which the heart is absent during the whole of embryonic life. We have described the origin and development of the organic nervous force. We have shown that it is created prior to the heart, and that it controls and regulates the early embryonic circulation before the central organ is formed. It is the formative and controlling force of every tissue in the organism. It is to the organism what the steam is to the engine. The heart is a muscular organ of wonderful mechanism; its power is derived from the organic nervous system by its organizing force in the production and transformation of the cells upon which the muscular action of the heart depends. Without the creation and disintegration of these cells, the heart could not act. Its muscular contractions and relaxations are accelerated or diminished, weak or strong, in direct proportion to the force of the organic nervous system in the mutations of its histological cells. The heart affords

the organic nervous system the mechanical apparatus necessary for the propulsion of the blood from it into the arterial vessels. There is a constant change going on in the cells of which the blood is composed. When the blood is carried into the capillaries of the lungs, the cells containing its elementary constituents are rapidly formed and transformed.

The Lungs.—Whatever may be the view entertained of the ultimate structure of the lungs, there is no difference in opinion as to the main physiological fact that these organs consist of a congeries of minute air-cells, whose cavities are capable of dilatation and contraction, and on whose walls a very minute plexus of capillary bloodvessels is distributed. The air-cells and air-tubes are endowed with a considerable amount of contractility, which resembles that of the intestines or arteries, and by which the absorption of oxygen from the atmosphere and the secretion of carbonic acid gas from the blood are favoured; because it enables the air-cells to receive and retain more resident oxygen within these cavities, which greatly facilitates the absorption and combination of this gas with the carbon of the blood by its continued approximation to and gentle pressure upon the minute absorbent cells in the ultimate glandular structure of the lungs. The production of cells as an integral part of absorption or secretion has been demonstrated by Mr. Goodsir in a variety of experiments; and he has further shown that what is ordinarily termed an acinus is nothing more than a parent-cell filled with progeny. This statement may be applied to the lungs in which the air-tubes do not terminate in a dilated sac, but open into a system of communicating beaded canals forming a kind of acinus. These beaded canals are evidently composed of cells partly fused together; and, by the comparison of their state in animals of different ages, it seems that they are all developed from the cell in which the air-tube terminates, and that they continue to increase in number from the period of their birth to adult age. The fact respecting the function of the red corpuscles of the blood and their connection with the respiratory function, supplies the required proof that respiration takes place through the medium of cells. Respiration is a process of absorption and of secretion, and governed by the same laws by which these processes are in the other glands of the organism.

Arteries.—Every manifestation of organic nervous force must take place in the capillaries. That the movement of the blood through the arterial vessels in man is, in a great degree, dependent upon the mechanical action of the heart, there can be no doubt. But the bloodvessels have an influence in producing both local and general modifications of the effects of the heart's action. The middle or fibrous coat of the arteries is alone endowed with contractile properties. The arteries have the same organic nervous endowment as the heart. They have vascular and nervous branches of the organic system which control and regulate their functions in the organism, and bring them in relation to the external world. The fibrous coat of the arteries is endowed with muscular branches of the organic nerve, which causes it to contract and relax alternately like the walls of the heart, but not in a manner so manifest,

because the fibres are shorter, and the organization of the adjacent tissues is different.

The force of these alternate contractions in the fibrous coat of the arteries is supplementary to that of the heart's impulse relaxing to receive the blood from it, and contracting upon their contents with a power superior to that by which they are distended. The muscular coat regulates the diameter of the arteries in accordance with the quantity of blood to be conducted through them to any organ or tissue. Local changes are continually to be observed in the various phases of normal life as well as in diseased states; and they will be found to be in harmony with the particular condition of the processes of nutrition, secretion, &c., to which the capillary circulation ministers. Of this kind, are the enlargement of the trunks of the uterine and mammary arteries at the periods of pregnancy and lactation; the enlargement and strongly increased pulsations of the radial artery when there is any active inflammation in the thumb; the enormous diameter to which the spermatic artery will attain when the testicles is greatly increased in size by diseased action. This dilation is due to increased nutrition in the coats of the arteries, since we find that their walls are thickened as well as distended.

Capillaries.—We have already indicated the independence of the capillary circulation of the heart's action. In an acardiac foetus the heart is never formed, yet the organic nervous force manifested in the capillaries in the form of nutritive attraction supplies its place up to the period of birth, after which the circulation ceases for want of due aeration of the blood. The capillaries may, by excessive and perverted nutrition, afford a complete check to the circulation in a part, even when the heart's action is not impaired, and no mechanical impediment exists to the transmission of the blood. Thus, cases of spontaneous gangrene of the lower extremities are of no unfrequent occurrence, in which the death of the solid tissues is clearly connected with a local decline of the circulation; and an examination of the limb, after its removal, shows that both the large tubes and capillaries were completely pervious; so that the cessation of the flow of blood could not be attributed to any impediment except the destruction of some force which existed in the capillaries, and is necessary to the maintenance of the current through them.

Normal reaction between the blood and surrounding medium, whether this be air, water, or solid organized tissue, is a condition necessary to the regular movement of the blood through the capillaries. When the ordinary respiration of an animal is interrupted, and the asphyxia is proceeding to the state of insensibility, the first lesion induced is that of organic innervation, which produces a lesion of circulation, the blood accumulates so as to distend the arteries, even though it is at the time nearly venous in its character. This indicates that the fluid, now so perverted, is unable to pass with facility through the systemic capillaries, because the organic nervous force has lost its controlling influence over the elements of the blood. The elements are not normally transformed; oxygen is neither normally absorbed, nor carbon nor-

mally secreted. An accumulation of blood in the venous system, and a deficient supply of arterial, are the necessary consequences. When the blood is not normally arterialized, it has a depressing influence upon the brain and organism generally, because its cells containing its different elements cannot undergo normal transformation by which they would form component elements of the different tissue, or be depurated from the organism.

It is a general fact, unquestioned by any physiologist, that when there is any local excitement to the processes of nutrition, secretion, &c., a determination of blood towards the part speedily takes place, and the motion of the blood through it is increased in rapidity; and, although it might be urged that this increased determination may not be the effect but the cause of the increased local action, such an opinion cannot be sustained. Local determination may take place, not only as a part of the regular phenomena of growth and development, as in case of the entire genital system at the period of puberty, but also as a consequence of a strictly local cause. All these facts harmonize completely with the phenomena which are yet more striking in the lower classes of organized beings, and are evidently the result of the same law. The capillaries are endowed with an organization analogous to that of the arteries; they appear to be a more delicate and highly organized prolongation of the same tissue. That the contractile coat of the capillaries is largely endowed with organic nervous branches, which control and regulate their calibres, scarcely admits of doubt. Local stimulants produce a contraction of the capillaries by their action upon the organic nervous branches distributed into their coats. The effect of stimulants is to accelerate the capillary circulation, unless an abnormal condition of the organic nervous branches results from their sudden or continued influence. While the introduction of blood into the capillaries of every tissue of the organism is facilitated by the action of the heart, its rate of passage through these vessels is generally modified by the degree of activity in the processes to which it should be normally subservient in them. The current flows more rapidly by an increase in their activity, and becomes stagnated by its depression or total cessation. The capillaries possess a distributive power over the blood, regulating the local circulation independently of the heart, in obedience to the function of each gland or tissue. The dilatation and contraction of the capillaries have only a secondary influence on the movement of the blood through them; the primary is derived from the organic nervous system.

A gentle stimulant, which excites the contractility of the capillaries, accelerates the motion of the blood by rendering more energetic the cellular formation and transformation of its elements, and the combination of its nutritive elements with the surrounding tissues. The combination of the nutritive elements with the adjacent tissues is in obedience to the laws of organic nutritive attraction, and any abnormal influence which impairs this attraction impairs the nutrition of the tissues in which these capillaries circulate, because it impedes the cellular mutations of the elements of the blood upon which the normal nutri-

tive attraction for the surrounding tissues depends. When the elements of the blood do not undergo normal mutations, they may be attracted towards the solid tissues surrounding the capillaries; but, as they do not undergo that series of cellular changes which completely liberates the nutritive elements from the effort, they are to some degree repelled by the solid tissues, because these tissues attract nutritive elements strictly in obedience to the laws of their ultimate elementary composition. Alterations in the chemical condition of the blood are, therefore, caused by a lesion of organic innervation, and produce a lesion of capillary circulation by which the elements of the blood aggregate in obedience to the laws of chemical affinity. This proposition enables us to comprehend the magnitude of morbid action produced in the whole organism when the lungs fail to absorb a normal quantity of oxygen, or secrete a normal quantity of carbonic acid gas; when the liver fails to secrete normally its carbonaceous compounds; or the kidneys, the effete azotized elements of the transformed tissues. When all these depuratory glands at the same time fail to perform their normal functions, the blood is soon rendered unfit for the purposes of the organism; a diseased transformation of all the tissues of the organism is produced.

ART. VII.—*On Incontinence of Urine in Children.* By D. D. SLADE, M. D.,
of Boston.

INCONTINENCE of urine in children is one of those affections with which the practitioner constantly meets, and although in general it is one of no serious character, yet it often proves extremely rebellious to all treatment, and becomes exceedingly annoying to the medical man, not only from its obstinate persistence, but from the very fact of its apparent insignificance.

The discharge of the contents of the bladder and rectum take place involuntarily in early infancy; but as the child advances in age it acquires a control over the sphincters, being prompted to this by a sense of decency as well as by other influences. The involuntary discharge of the contents of the bladder may, however, from a variety of causes, be kept up to the age of puberty, and even later, proving a source of inconvenience and extreme mortification to the unhappy subject of it.

We shall arrange these children who suffer from incontinence of urine into three classes. In the first class, the affection is constant and due to special causes which need not occupy our attention here. In the second, it is intermittent in its character, occurring in the day as well as in the night; while in the third class it is nocturnal only.

In the second of these classes, which constitute cases by no means rare, and much less amenable to treatment than those where the affection is only

nocturnal, the urine passes off involuntarily not only during sleep, but also during the day, the patient not unfrequently finding his clothes wet without having been conscious of having emptied the bladder. Moreover, he is at all times obliged to urinate more often than natural, and can scarcely ever resist the desire the moment it is felt. This form of incontinence is intermittent, the patient at certain periods being almost entirely rid of his complaint.

It is also, we think, more often found among children of a lymphatic temperament, and who require a tonic treatment; and it even appears to be hereditary, as we have seen several members of a family afflicted as were their parents before them. This hereditary tendency is also seen in the nocturnal form.

In some of the cases of the above class, the urine is copious, pale-coloured, and of low specific gravity, but very frequently neither the quality nor the quantity of urine seems to be at fault, but there is a peculiar morbid condition of the urinary organs. This condition, as Gross remarks, is undoubtedly due to a morbid irritability of the neck of the bladder, to an exaltation of the natural sensibility of that part, often unaccompanied by any appreciable change of structure.

It may also depend wholly, or in part, upon sympathy with a diseased condition of the rectum, vagina, uterus, or kidney.

The cases coming under the third class do not always constitute a malady, properly speaking, but often merely a habit. M. Petit separates children suffering with the nocturnal form of incontinence only into three categories. The first is made up of those who are too lazy to rise from bed in order to satisfy Nature at her first bidding. The second, of those who sleep so soundly that the sensation which precedes the desire to urinate is not sufficient to awake them; the neck of the bladder is the only portion which feels, so to speak, and which, accustomed to obey this sensation, opens mechanically and suffers the urine to pass without the brain being warned. The third, of those who dream that they are urinating against a wall, or into a vessel, &c.

The incontinence of the first and last categories does not depend upon any pathological condition of the urinary apparatus, and its treatment consists entirely in the adoption of moral means. That which comes under the second category is alone to be referred to a morbid condition, and is thus explained by M. Barrier, in his *Traité Pratique des Maladies de l'Enfance*.

"It is easy to conceive how infancy predisposes so powerfully to nocturnal enuresis. At this age the muscular system of organic life enjoys a contractility much more pronounced; all the reservoirs which are provided with this are more frequently emptied. Now, the bladder has a muscular coat, the contractions of which, at least in part, are under the influence of the ganglionic system of nerves, and, therefore, removed from the influence of the will. During the day, the effects of these contractions are prevented either by the simple tonic force of the constrictor muscles of the neck of the bladder, or by their voluntary contraction; but sleep, which abolishes all voluntary contractility, has no such effect upon the sensible organic contractility.

"It would, perhaps, be even easy to show that this latter is augmented during sleep. However it may be, the suppression of the will during sleep destroys the equilibrium existing between two powerful antagonists in favour of that which naturally predominates in infancy.

"Sleep also abolishes the functions of sense and of intellect more completely than those of instinct, consequently many of these last are performed during that state.

"Other influences also act in a similar manner, such as those dependent upon a feeble constitution, upon a lymphatic temperament, and upon inheritance. As to the occasional causes, it is easy to understand that drinking in large quantities, or forgetting to make the child empty the bladder before retiring, the warmth of the bed, &c., ought to favour the involuntary emission of the urine.

"A circumstance which tends to keep up this affection, is the habit which the bladder soon acquires of emptying itself at certain hours, under the influence of the causes of which we have spoken. We know that the frequent repetition of the same act contributes to its maintenance by the most trivial causes; all our organic functions, as digestion, the excretions, &c., are subject to the laws of habit. The influence of habit, then, partially explains the obstinate persistence of nocturnal enuresis, which is often prolonged to adolescence in spite of all remedies."

M. Civiale differs from most authors as to the cause of nocturnal enuresis in children. He remarks that he has observed this condition in subjects whose bladders, being in a sluggish state, were continually distended by a certain quantity of urine, and that the incontinence here observed is due to a veritable overflowing, analogous to that which happens in every form of retention.

If such is the cause of this affection, and as frequent a one as M. Civiale would lead us to suppose, how does it happen that this overflowing takes place only during sleep? We cannot deny that such a cause may operate in rare cases, and the very existence of it should lead us to be very guarded in our diagnosis and treatment.

M. Caudmont, of Paris, has advanced the opinion that incontinence of urine in children is often dependent upon what he terms "a contraction of the neck of the bladder," comprehending under the term "neck of the bladder," not only the urethro-vesical orifice, but the prostatic and membranous portions of the urethra, extending from the above orifice, to the triangular ligament. The involuntary and permanent contraction of the muscular fibres of this part of Guthrie's and Wilson's muscles, gives rise to a variety of symptoms, which are generally described by authors as dependent upon a neuralgia or *névrose* of this region. To this permanent contraction, or rather to its consequences, M. Candmont attributes incontinence of urine, and accounts for the beneficial action of belladonna, of which we shall soon speak, by its power of overcoming this condition.

M. Civiale describes this very contraction in speaking of vesical neuralgia—

"The sensibility and contractility of the neck of the bladder being inti-

mately connected, the augmentation of the one ought necessarily to modify the other, and thus create trouble in the functions of the bladder. We shall see, in fact, when we come to examine the diseases of this organ, that this contraction of its neck plays a great part in most of the affections by which it is attached."

We saw, while under M. Caudmont's instruction, sufficient to convince us that his views upon this point were very just and well founded; and, moreover, that they were confirmed by his successful mode of treatment. We shall, at some future time, explain more fully this "contraction of the neck of the bladder," and describe its intimate connection with several other common and obstinate affections daily presented to the notice of the practitioner, the causes of which have been heretofore very obscure.

We shall not speak here of those specific causes of incontinence of urine, such as stone, paralysis, &c., but shall pass on to the treatment of this complaint.

If our knowledge respecting the causes of the common forms of enuresis in children is thus vague and hypothetical, can we offer anything more satisfactory respecting its treatment? This may be quickly decided.

First.—Of the treatment of those cases where the incontinence is by day as well as by night. The treatment of this form should be general and local.

The first has for its object to overcome the influence of hereditary disease which has a tendency to debilitate the constitution. This end may be best brought about by proper attention to diet, which should be plain, and at the same time generous and nutritious; exercise in the open air, regulation of the bowels, cold bathing if it can be supported, and the use of a proper course of tonics. As in this form of incontinence particularly, there is more or less morbid irritability of the neck of the bladder, we advise the use of sedatives. Any unnatural condition of the urine must, of course, be corrected.

By the local treatment we propose to modify any undue sensibility or irritability of the bladder. This comprehends douches of cold water to the sacro-lumbar region, the application of blisters to the sacrum and perineum, the occasional passage of the bougie, and in extremely obstinate cases even a slight cauterization of the neck of the bladder.

Second.—Of the treatment of those cases where the incontinence is entirely nocturnal. As with many children this form of enuresis is cured spontaneously towards the age of four or five years, we should be careful not to pursue a too active treatment before this age, in endeavouring to overcome what, after all, with them is an affection of small importance; but, when it is proper to interfere, we must first advise the influence of moral means, which should never be harsh or cruel, but just sufficient to awaken the child to a sense of shame.

This has especial reference to those cases where we suppose the trouble to be the result of laziness; if this be the case, we cannot make the habit too disagreeable to the individual. What we have already said as regards the

general treatment in the other form, is also applicable here. The diet should be mild and unirritant. The supper should be taken at an early hour, and nothing should be given after this. The child should sleep on a mattress, with just sufficient clothing to be perfectly comfortable, and should be aroused two or three times, if necessary, in order to empty the bladder. Most writers speak of the importance of the position to be kept during sleep, viz: upon the side, and not upon the back. We agree with Dr. Condie, who remarks that the position of the child during sleep is of little or no consequence; the discharge from the bladder taking place, no matter what may be the position. The trouble is doubtless often kept up by mere force of habit, and if the particular hour can be ascertained when the involuntary discharge takes place, and the patient awakened just before the occurrence, and made to empty the bladder, he soon acquires the power of retaining his urine during the entire night.

Sometimes also the affection is due to an irritation produced in the neighbouring parts by the presence of ascarides. We should always bear this in mind in making up our treatment, and direct it accordingly.

We come now to speak of internal remedies, some of which have received high encomiums, without, we think, sufficient grounds. Strychnine and cantharides are highly recommended by almost all writers, the latter carried to the extent of producing slight strangury. Gross speaks favourably of this remedy, which he states has succeeded where everything else has failed.

When any irritability exists about the bladder, copaiba in small doses, and cubebs in powder, have lately been highly extolled. Dr. Deiters recommends from his own experience, the effects of the latter medicament in tolerably large doses given twice a day during a period of from three to eight weeks. A small anodyne at night, in the form of a Dover's powder, often exerts a beneficial effect. But it is to belladonna that we wish particularly to call the attention of the profession. M. Trousseau has been most instrumental in bringing this substance into notice as being applicable to these particular cases of incontinence in children, and we can testify to its highly beneficial effects in several cases in our own practice, besides those which we observed in the *Hôpital des Enfants Malades*.

The following is from the clinical lectures of M. Trousseau:—

"This infirmity I consider a neuralgia or *névrose*, and it is much more common than is generally believed. It has been thought nearly incurable; but, with belladonna, is one of the most easily cured of all diseases.

"Those children who are troubled in the daytime are not cured by belladonna."

"A child going to bed with an empty bladder will pass his water during the first two hours, sometimes during the first hour; placed in bed at 9 o'clock, the bladder will throw off its contents at 10 o'clock, and then retain them until 8 o'clock the following morning, being full at that time. The accident generally happens once only during the night, but sometimes twice. During the first hours of sleep, you generally find in young men and children an erection. The question may be asked, whether something of the same kind may not take place in the bladder. This complaint is most common among

young girls, and is generally cured spontaneously at the age of puberty ; but when this is not the case, do what chance has shown to be efficacious.

"Two young girls afflicted with whooping-cough, were treated with belladonna, and were cured both of the cough and of the affection now under consideration.

"*Treatment.*—The first precaution is to break up the bad habit of the organs. Wake the child at the end of an hour, and make him pass his water; after several days, make him at the end of an hour and a half. This is only accessory.

"Give at the moment of going to bed, ext. belladonna in pill, commencing with gr. $\frac{1}{2}$, waking the child as has been described. After 8 days, increase the dose to double; at the end of 8 days more, treble it, waking him later and later, and finally not at all. When during 15 days he has been free from the difficulty, diminish the dose, or give it only every second day, then every third day, &c. Even if the child does not regain its bad habits, renew the medicine after two, three, and sometimes five months' cure, for he may then again begin to wet the bed. Resume the medicine as first given, several times, making the intervals longer and longer.

"The incontinence may be aggravated by eczema, caused by the constant irritation of the urine; and the urethra becoming inflamed, the desire to pass water is increased. For this state of things a sol. sulph. zinci is the best application.

"If belladonna fails, try strychnine, flagellation, and stinging with nettles. The two latter may be useful as means of intimidation, or perhaps they may have some reflex power."

So far as our experience goes, we have derived very satisfactory results from the use of belladonna in several cases of nocturnal incontinence. But as Trousseau explicitly says, it did not have the same good effects in those cases where the enuresis occurred during the day, although it greatly relieved the trouble. We have, however, found that a larger dose was required than is recommended by that physician, in order to have the desired effect.

Reasoning from what we have stated as to the probable causes of this affection when it occurs during the day as well as night, we should be led to suppose that belladonna was even more applicable in this than in the nocturnal form only. Practical experience, however, shows that such is not the fact.

It remains for us to speak of mechanical contrivances for the removal of this complaint. The old method of tying the penis is absurd; but certain compressor bandages, such, for instance, as the one which Trousseau recommends, may be of service when other means have failed. This is composed of an elastic band placed about the body, supporting at the back a steel spring which reaches to the anus. To this is fitted a metallic plate, on which is fixed a truncated cone of India rubber, about two-thirds of an inch in diameter at the base. The pressure should be made directly upon the middle line of the perineum, and may be regulated by introducing the cone more or less deeply. The compression thus acts as a sphincter upon the prostatic portion of the urethra. The same beneficial effects may, as Gross remarks, be derived from the use of a bit of bougie placed along on the under surface of the penis, the point projecting a short distance beyond the gland, and retained in

its place by strips of adhesive plaster. In this way, the walls of the urethra are so approximated that no urine can pass. Of course, it must be removed when necessary to empty the bladder.

For girls, M. Trousseau makes use of an India-rubber bag, which is introduced beyond the hymen, and by means of a tube inflated with air, which is retained by a stopcock. This contrivance presses upon the lower part and neck of the bladder, preventing the escape of urine. In the morning, the instrument being emptied of air, it is removed or reapplied according to circumstances.

Such are the principal considerations which we have to offer upon this *bizarre* affection. Much is yet left to be discovered as to its causes and proper treatment, and this knowledge is only to be gained by long-continued and persevering observation. We may hope at some future day to contribute the results of our experience towards such a desirable end.

ART. VIII.—*Three Surgical Cases.* Reported by J. C. NOTT, M.D.,
of Mobile, Alabama. (With a wood-cut.)

THE following cases possess sufficient novelty and practical value to deserve publication:—

CASE I.—*Spina Bifida cured by Excision.*—The subject of this case, aged one month, was a male, and the child of an Irish woman; it presented, about the middle lumbar vertebra, a tumour an inch and a half in diameter, nearly circular, and elevated about three-quarters of an inch. The appearance of the tumour was unusual, and the first impression on my mind was that of fungus hematodes; the summit was nearly flat, of a reddish-chocolate colour, and in the centre was a thin pellucid membrane of about three-fourths of an inch in extent, through which could be seen serous fluid. The most graphic description I can give of the appearance of the tumour is that it resembled a half ripe carbuncle with a Malaga grape buried in the centre; had it not been for this deficiency of skin in the centre I should have been much embarrassed to form a diagnosis. Guided by a case somewhat similar, though smaller, reported by Dr. Mott, of New York, in his Appendix to Velpeau, I determined to extirpate the entire mass. I accordingly, on the 15th of March, 1855, in the presence of Drs. J. Hamilton, Vetchum, and Anderson, and my student, Mr. Childs, inclosed the tumour by two elliptical incisions in the direction of the spine, and dissected it out completely; the tumour was found to consist simply of skin, cellular tissue, and the membranes of the spine distended with serum. After the sac was removed, an opening into the spinal canal was ex-

posed about the size of the end of the finger, and a tablespoonful of fluid escaped.

It was then dressed by bringing the edges together by a single pin and twisted suture, and placing above and below strips of adhesive plaster.

The dressing was removed on the third day, and complete adhesion had taken place by first intention, except the portion included between the pin and ligature, which sloughed; this left a narrow gaping ulcer immediately over the opening in the spine, and I felt some apprehension about the result. I did not reflect on the extreme vascularity and tenderness of the skin of a child a month old, and put too much stress on a single point; it would have been more scientific to have made a longitudinal cut on each side to free the skin, and to have used two pins instead of one. The case, however, did well; granulations were thrown out, and the ulcer soon closed, and at the end of two weeks the healing was perfect and the parts firm and solid.

The child had no constitutional disturbance whatever; slept and nursed as usual. Two months have now passed, and the cure seems to be complete.

CASE II.—*Testicle containing Hair—Removal.*—The subject of this case, aged 22 years, was a patient of Dr. F. A. Ross, who called me in consultation. Says the right testicle was larger than the other in childhood, but never painful until the last five years, since which time it has been enlarging, and has given a good deal of pain. On examination, a tumour was found larger than the fist, about half of which was hydrocele, and the remainder enlargement of the testicle; pus had formed and pointed at the epididymis, but no opening had ever occurred.

It was determined to remove the testicle and sac together, which was done. The dissection of the mass after removal revealed the only points of special interest. The tunica vaginalis was much thickened, and contained a gill of limpid fluid. The testicle itself was enlarged to about four times its normal size. On opening it, the tunica albuginea was found a good deal thickened, and the glandular contents, if any ever existed, entirely destroyed by suppuration; the tunica albuginea was a complete sac, entirely filled with thick pus, about the consistence of boiled custard, mixed up with *hair*, from half an inch to four inches long. I sent specimens of the hair to Peter A. Brown, Esq. and Prof. Leidy, of Philadelphia, but they could detect nothing on examination with the microscope peculiar in the hair, worthy of remark.

It is more than probable that this development of hair was congenital, and I have no speculations to offer on the subject. We have many instances of development of hair, in cysts, in various parts of the body; still, this case is interesting both to the physiologist and pathologist, as well as to the surgeon.

CASE III.—*Necrosis of Lower Jaw.*—The subject of this case was a mulatto boy belonging to Major Haden, of Selma, Alabama, about 8 years old. He had been kicked by a horse on the lower jaw six months before he was brought to

me in Mobile, but I could not obtain any satisfactory history of the case. The jaw seemed to have been fractured, and when I first saw him there existed enormous swelling, with suppuration, and two openings, one at the angle of the jaw and the other near the chin. On passing a probe from one opening to the other, dead bone was detected. It was determined to make a free opening with the view of removing the diseased bone, but the extent of the operation could not be determined beforehand. A free opening was made externally down to the bone, from the angle of the jaw along its base to the chin; and, on introducing the finger, the body and ramus of the bone were found extensively denuded and bathed in pus. The base of the bone was enlarged in thickness to about three times its natural dimensions; and, on passing the finger along the outer surface of the bone from the ramus forwards towards the chin, a deep suture was felt along the shaft of the bone; this sulcus seemed to be the line between the living and dead bone. The diseased bone was not yet separated at its anterior extremity, but was still firmly adhering to the sound bone at the chin. I dissected loose the soft parts freely, and divided the bone at its anterior part with strong bone nippers; the ligamentous attachments of the condyloid and coronoid processes being destroyed by ulceration, I had little difficulty in extracting the bone with a pair of large forceps; the whole of the ramus and lower half of the body of the bone as far forward as the canine tooth was all brought away in a solid piece; the teeth, alveolar processes, and upper margin of bone were all left *in situ*. The boy had none but his twenty milk teeth, and the interesting point of the case was to see whether the teeth and portion of bone left behind could still be nourished and remain healthy after so large a piece of bone was removed, *including the nutritious artery*. The subjoined wood-cut will give a correct idea of the extent of bone removed; which is indicated by a dark line. The case did well, and was soon sent home to the country.

I received a letter, a few days ago, from Dr. Wm. P. Reese, from which I give the following extract, dated about two months and a half after the operation:—

“I have this day examined the little boy William, of Major Haden; he is quite well with the exception of a very small half closed fistulous opening at the anterior terminus of the cicatrix, from which there is very slight exudation; the teeth, gums, and alveoli are *in situ*, and apparently perfectly healthy; his general health is good.”

As I had to make an artificial division of the bone at the chin, I expected slow healing then, and probably the discharge of some small fragments of carious bone.



This case will be a curious one to watch, both in a pathological and physiological point of view. Will such a section of bone be able to sustain itself and the inclosed teeth? will new teeth replace the old ones when cast off? will this fragment of jaw have sufficient strength for mastication? I hope to be able to keep a watch upon the case through my friend, Dr. Reese.

ART. IX.—*On the Use of the Foramen Sœmmering of the Eye.* By F. LEAMING, M. D., of Tippecanoe Co., Indiana. (With two wood-cuts.)

PRECISELY in the axis of the eye there is a yellow spot on the retina, one-tenth of an inch in diameter, with a small hole in the centre; between the yellow spot and the optic nerve there is a bifurcated fold of the retina, the opening of which is towards the yellow spot. These three marks, viz: the yellow spot, the foramen, and fold, are called after Sœmmering, their discoverer; but their uses, so far as I can learn, have not hitherto been explained.

If we close one eye and look upon the page of a book, we shall notice that the word in the axis of the eye, as well as the words immediately above and below it, are distinct, while the rest of the page is illegible. Perfectly distinct vision is confined to a very small space of the retina, and is bounded by the limits of the foramen in the centre of the yellow spot. But an opening in the retina, instead of perfecting, would destroy vision; we must necessarily conclude that, under the circumstances alluded to, the foramen is closed. Now, the foramen has sometimes been found closed by anatomists, but then the bifurcated fold had disappeared, and the only mark of its previous existence was a dent in the vitreous humour corresponding precisely to the fold. An open foramen with a fold of the retina; a closed foramen, and no fold of the retina; all this implies motion of the parts.

If we look at a distant object with both eyes open, and pass an ordinary ruler before one of them so as to exclude the distant object, the central part of the ruler will be invisible to that eye; that is, the central part of the retina has become insensible to light. The bounds of this insensibility can easily be defined, and they will be found to correspond with those of the yellow spot of Sœmmering. The following diagram will illustrate this sufficiently, the ruler being held about 12 or 15 inches distant, and made to pass before the left eye:—

Fig. 1.



Fig. 2.

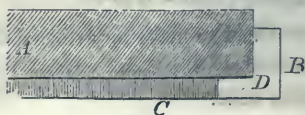


Fig. 1, A, represents the ruler seen by the right eye; B, that by the left; the outline of the extremity being faintly visible; the central part as far as C is transparent or invisible, while the distant object appears at D.

The ruler may now be passed further to the right, when the extremity at B will become visible again; showing that the power of becoming insensible to light, under these circumstances, is possessed only by the yellow spot of Sœmmering, and not by the retina at large. It is curious to watch the play of sensibility; sometimes the transparency expanding widely and in a moment contracting to a mere point.

The use of all this is evident. When two objects are presented (a very frequent occurrence), one in the axis of each eye, the mind is not perplexed by the blending of the two objects, but contemplates the one while the other is withdrawn. This may be further illustrated by Wheatstone's *Stereoscope*. Place before the glasses a printed page on which two pencil marks have been drawn vertically about two inches apart. Let the lines be thrown into one by the action of the eyes, and fix the attention on any word the lines appear to run through. At first, perhaps, there will be a blending of letters, so that no word can be made out, both foramina being closed and sensitive; presently a word will be distinct, and either be retained or alternate with a word through which the other pencil mark passes. We may infer that this is owing in the latter case to the alternate action of the foramina, and not to the alternate action of the eyes, for the vertical pencil marks remain blended.

I have made other experiments than these on this subject, but as they are more difficult, and all result in proving that the central parts of the retina have an action independent of the retina at large, and as those already given are sufficiently conclusive, I shall detail no more.

ART. X.—*Knife-Needle for the Operation for Cataract by Solution or Absorption.* By ISAAC HAYS, M.D.

THE operation for the removal of cataract by solution or absorption, has been deemed, by many surgeons, entirely inapplicable to hard cataracts, mainly in consequence of the difficulty of dividing such a lens by the needle ordinarily used for the purpose. The common straight needle cannot be made to cut well, beyond a short distance from the point, without being so thin as to endanger its breaking; and it is not possible to cut with a curved one. I have entertained the belief for several years; as is well known to my friends who are interested in the subject, that a needle better adapted for the purpose might be made something after the fashion of an iris knife, and more than three years since, I had such a one constructed by Mr.

Schmidt. This instrument was described and figured in my last edition of Mr. Lawrence's Treatise, p. 726.

The instrument, not having been made of the exact form I desired, though described as having a *very acute point*, is not so represented in the cut. Not being satisfied with the instruments made for me in Philadelphia, I sent a model to Messrs. Phillip & Wicker, surgeon instrument makers in London, with directions as to the improvement I desired in its form, and an order to make me some of them. The instruments which they sent to me were made with the point sufficiently acute, but the back was straight to the point, and the cutting edge was too long, the makers having followed the model of the ordinary iris knife too closely.

Fig. 1. Fig. 2.



A model was afterwards sent to M. Luer, of Paris, with an order for some of his make. These, on their arrival, I found not sufficiently slender, and the point was not sufficiently acute.

I have since had them made of different shapes, and finally Mr. Kolbe, of this city, formerly a workman of Mr. Luer, in Paris, has succeeded, under my directions, in making me one which has given me entire satisfaction, and is represented of its natural size in the accompanying cut, Fig. 1.

This instrument, from the point to the bead near the handle (*a* to *b*, see Fig. 2), is six-tenths of an inch, its cutting edge (*a* to *c*) is nearly four-tenths of an inch. The back is straight to near the point, where it is truncated, so as to make the point stronger, but at the same time leaving it very acute; and the edge of this truncated portion of the back is made to cut. The remainder of the back is simply rounded off. The cutting edge is perfectly straight, and is made to cut up to the part where the instrument becomes round *c*. This portion requires to be carefully constructed, so that as the instrument enters the eye it shall fill up the incision, and thus prevent the escape of the aqueous humour. In the diagram (Fig. 2) the proportions of the blade are not very accurately represented, the rounded part being

rather too slender, and the handle should be octagonal, with equal sides, and of the same thickness its whole length.

We have now used the knife-needle in a sufficient number of cases to be convinced of its superiority over any of the ordinary cataract needles; and of the different forms of the former which we have tried, the one represented in the accompanying drawing is, we are satisfied, the best; and we confidently recommend it as such to ophthalmic surgeons.

ART. XI.—*A Case of Vicarious Secretion of Milk.*

By S. WIER MITCHELL, M. D.

INSTANCES of vicarious secretion are so rare that the addition of even a single case cannot fail to excite interest. It is not uncommon to meet with instances of vicarious menstrual flow; but this secretion, if such it be, is so nearly allied to the blood itself as to require no elaboration, and thus, when the body is overloaded with blood we can hardly feel surprised at any hemorrhage which, by replacing the uterine flow, relieves the congested vessels.¹

The difficulty of comprehending this subject is greater in cases of vicarious performance of the urinary function. Here, we have a secretion for whose perfection cell-action is in all likelihood essential. Metastasis of this excretion involves, therefore, the existence of such cells in some remote part of the body, where nature has not primarily placed them. Their function will be one of mere selection, and not of elaboration, since the elements of the urine exist, for the most part, preformed in the blood. The mammary gland secretes a fluid whose elements are not alone separated from the blood, but are also elaborated, to a certain extent, as, for example, sugar of milk, which has no being in the circulating fluids. The milk-globules, once regarded as true cells, are, in reality, globules of oil which have taken on a coating of albumen. Their appearance may be closely imitated by shaking together oil and albumen, when similar pseudo-cells are formed. The milk globules, therefore, cannot be regarded as presenting any added obstacle to the metastasis of the mammary secretion.

In the early part of July, 1853, Mrs. C., æt. 20, was delivered of a child. The supply of milk proved scanty. Four weeks after delivery, a large mammary abscess formed in the left breast. This was opened, and, under treatment, became well in the course of three months. During this period, she nursed her child with the right breast. As the milk continued to form, though in small amount, in the left breast, the child finally obtained milk from both mammæ. At the close of a year, the mother became feeble, and subject to constant bronchial irritation. Her cough increased to such a degree that, for obvious reasons, she was directed to wean her child. The change was effected too suddenly, and she was told, accordingly, to allow her babe a part, at least, of her own milk. In the interval of two days, the breasts had become swollen, and excessively painful. It was now found that the child could obtain no nutriment from this source, and that even the pump failed to

¹ The most curious case of this kind, of which I am aware, occurred a few years ago in the person of a seamstress. The menstrual flow in this instance took place with great regularity from beneath the finger nails, and obliged her to relinquish all work for a time. She recovered her health by the aid of iron, etc., and the uterus resumed its lost function.—S. W. M.

empty the mammae. Purgatives were ordered, and a water-dressing locally applied. Next day I found the breasts less painful, while the cough had become dry, hard, and almost constant. On the succeeding morning, she was greatly relieved by a loose cough, which enabled her to expectorate a quantity of white, firm sputa. I was told that Mrs. C. was spitting up milk, and the white substance in question was exhibited in confirmation of the statement. During the day, upwards of a teacupful was thus cast up. This strange formation continued for a fortnight, to the manifest relief of hitherto urgent symptoms of declining health. My visits were made at irregular intervals, and for a time my patient was closely watched by the family; but, as the secretion was persistent in amount for a time, and as it was often coughed up in my presence, I see no reason to suspect collusion or hysterical deception. At the end of a fortnight the milky sputa became more pale, and at length gave place to mere mucus.

On the third day, and at intervals afterwards, I collected portions of the sputa, and submitted them to rigid examinations.

I found the discharge to consist of white clots, floating in a thick fluid, also of a white hue, and mingled with ordinary mucus. Very often a clot of yellowish muco-pus was enveloped in a covering of thickened milk. It is to be noted that the milk was, for the most part, in this clotted condition, and possessed the lacteal odour to a greater degree than the ordinary secretion itself.

Microscopic analysis revealed the presence of very perfect milk globules, mingled with compound granular cells, mucous corpuscles, and epithelia. I evaporated about one ounce and a half of the fluid, and, by repeated treatment with ether, collected a small amount of fat or oily matters. Nitric acid was found to coagulate the filtered fluid, whilst acetic acid produced but a slight cloud. Lastly, I was told by the patient that the sputa tasted like milk.

Mrs. C. was delivered of her second child in February of the present year. Her cough never left her; and at the period last alluded to, she was far advanced in phthisis. Her breasts were well filled, but her extreme feebleness obliged her to resign her child to a wet-nurse. The breasts were artificially emptied, aperients employed, and no signs of her former trouble appeared until seven days after birth. At this time, for some forty hours, she coughed up a thin white fluid, mixed with the pus from a tuberculous cavity in the right lung. At the close of the period of time just mentioned, the pulmonary sputa regained its colour. I examined this specimen with the microscope only. It was thin, and small in quantity, but did not otherwise differ from the specimens obtained during the last year's illness. Both alike contained milk globules. The recurrence of the milky sputa was first observed by me, and pointed out to the patient. This, with the other facts above alluded to, induce me to regard the above stated case as indisputably one of vicarious or metastatic secretion of milk. We are thus called upon to admit that some

part of the bronchial surfaces *may* repeat the role which nature has assigned to the mammary gland.

It may be as well to add that, in stating a case of interest mainly to the physiologist, I have avoided all detail as to the remedial measures employed from time to time.¹

ART. XII.—*Castration; Recovery, followed by Phthisis Pulmonalis.*

By WM. T. TAYLOR, M. D.

CHARLES H., a cigar-maker, residing in this city, was in the habit occasionally of drinking to excess. He was a married man, and the father of three children. In the early part of the year 1853, he had been on a debauch for several weeks, when he was attacked with *mania à potu*. Being unable to sleep, and having his mind constantly disturbed with hallucinations, he applied to me on March 6th for relief, when I gave him a tablespoonful every two hours of the following mixture: Morph. sulphas. gr. i; spt. ammon. aromat. fʒi; sulph. æth. comp. fʒij; liq. ammo. acet. fʒij; aq. camph. fʒi. This composed him, so that he was enabled to sleep, which he had not done for several days.

March 7. He was quite delirious, requiring to be watched incessantly by his family. By using the mixture, he was quieted during part of the night; but, at 4 o'clock on the morning of March 8th, he escaped from home, and in the darkness having eluded his pursuing relatives, wandered out in a wood near to Girard College. There he picked up a piece of a porter bottle, with which he made a deep cut in the bend of his left arm, and also hacked off his penis and testicles, with the scrotum, at the pubis.

When discovered, he was sitting on the ground bleeding very profusely, and actually gnawing at his lacerated and bloody organs. He was brought home at half past 8 o'clock A. M., when I saw him; and, there being no hemorrhage from either of his wounds, whilst his powers of life were failing very fast, a glass of brandy was given to him, and he was sent to the Pennsylvania Hospital. Being an injury which resulted from *mania à potu*, he was not admitted, but sent from thence to the Almshouse Hospital, where he came under the care of Drs. Stillé and Gilbert, assistant surgeons of the institution. One of the spermatic cords was tied, but the other, having retracted into the abdomen, could not be found; a T bandage was applied, and a catheter introduced through the remaining part of the urethra. Reaction occurred in the afternoon, but there was no return of hemorrhage.

March 15th. On visiting the hospital to-day, I found the patient quite rational; he conversed very freely on his condition. Having slept eight hours during the previous night, felt better than he had since he entered the institution. The wound has a healthy appearance, but he complains of a stinging pain in the urethra; passes his urine in a full stream. Dr. Stillé informed me that, during the past week, in consequence of being visited too frequently by his relatives, he had had attacks of delirium, and required occa-

¹ A case very similar to the one just recorded is related in Prof. Carpenter's *Text-Book of Human Physiology*.

sional doses of morphia, with watchers constantly at his bedside, to prevent his committing suicide or some violence on himself. The late Dr. Stewart (then house surgeon) informed me that the patient gave him the following account of his strange act:—

Imagining that his relatives were accomplices of a crowd of demons who were constantly pursuing him, he succeeded in escaping from them in the darkness of the night, and ran towards Girard College, intending to hide in a small wood near by. But, on approaching the place, he was met by a greater number of fiends, who, having caught and secured him, told him that, to appease their anger and obtain his liberty, he must sacrifice his virility. Accordingly, he picked up a piece of a porter bottle which was lying on the ground near by, and performed the operation as before mentioned. Having so rude an instrument, he was three-quarters of an hour in excising the parts, whilst the lacerated manner of operating prevented much hemorrhage.

April 20th. Through the care and attention of his physicians and nurses, he was enabled to leave the hospital to-day, and return to his home. The wound at the pubis had nearly healed, with the exception of a few unhealthy granulations, which were kept down by the use of nitrate of silver and a wash composed of the sulphate of copper and quinia.

July 21st. I was requested to visit C. H., who, from exposure yesterday, had taken a heavy chill, and was then suffering with a high fever, and a spasmodic stricture of the urethra; this, however, was only partial, for the urine flows in a very small stream from a meatus urinarius, so contracted and hidden by the cicatrix on the pubis that it could scarcely be seen. With some difficulty, I introduced a probe, of the size of a knitting-needle, through the orifice; to allay fever and dilate the passage, I fomented the pubes with warm water, and gave an infusion of senna and salts, with occasional doses of neutral mixture. These greatly relieved him; so that, on the following day, I left with him a small German-silver bougie, to use daily in dilating the orifice as I had previously done. Finding the use of this bougie to produce some spasmodic contraction of the urethra, it was changed for a silver one, which caused no uneasiness. By its use, and one or two of larger size, the passage became dilated to its usual dimensions.

January, 1854. Met C. H.; he is very much improved in his appearance, being quite robust. His beard and whiskers are very thick. His voice is still masculine, but there is a slight hesitation or faltering in his speech, which I did not observe before the accident. This hale and hearty condition of his physical frame did not last long; for, during the month of August, he began to lose flesh, and was affected with night-sweats, together with great prostration, for which he took tonics. Obtaining no relief, and being also affected with a troublesome cough, he applied to me on the fourth of November last. He was pale and greatly emaciated, had a hectic fever, a quick and feeble pulse, a dry cough, night-sweats, loss of appetite, with a general debility. Gave him a table-spoonful of the cod-liver oil three times a day, with an anodyne expectorant for his cough; but, being unable to give him any encouragement or hopes of a cure, he left me, nor did I see him again, but ascertained from some of his family that an "Indian Doctor" had undertaken to cure him. This empiric, however, could not keep away the great destroyer; for, in the latter part of December, 1854, he fell a victim to phthisis pulmonalis. Can the loss of his virility have had any connection with the development of tubercular disease?

PHILAD., April, 1855.

REVIEWS.

ART. XIII.—*The Pathology and Treatment of Stricture of the Urethra, both in the male and female; being the Treatise for which the Jacksonian Prize for the year 1852 was awarded by the College of Surgeons of England.* By HENRY THOMPSON, F. R. C. S., M. B. Lond., &c. &c. London: John Churchill, 1854. 8vo. pp. 424.

IT is not often that a prize essay succeeds so well as has this excellent monograph, in sustaining, among all classes of professional inquirers, the award of its official sponsors. The reputation of Mr. Thompson's work, as in most respects the best treatise on the subject in the English language, is already too well established to require further commendation at this time in any quarter. Our purpose, therefore, is not to eulogize it but to endeavour, in a cursory sketch of some of its most interesting features, to afford our readers a tolerable notion of the advantage to be gained by a closer study of its pages.

To give an idea, at the outset, of the comprehensive and thorough manner in which our author has acquitted himself of his arduous task, we may here refer to the plan of arrangement adopted by him and described in the preface.

The possession of manual dexterity and a practical familiarity with the best mechanical appliances are, he justly tells us, not the only essential requisites in the successful treatment of stricture. An intimate acquaintance with the pathology of the entire genito-urinary system, with its sympathetic and functional relations in health and disease in all directions, is no less indispensable. He does not pretend to consider *in extenso*, within the compass of a single volume, the various topics suggested by the broad view thus taken of the question. Voluminous and rich in valuable matter, as his production obviously is, he would seem to regard it as presenting only a brief review of the most important considerations involved in the study of the subject. Be this as it may, our own impression is that few students and practitioners, however great their reading and practical experience, could rise from a careful examination of his clear, ample, and impartial expositions, without having acquired a material addition to their previous stock of knowledge in relation to the entire subject of inquiry.

In the first place: Mr. Thompson has freely and carefully collected from standard authorities in regard to all important points, quoting the writers' words in each case, and making the direct reference to page and edition of each particular citation.

Secondly: Original researches have been made whenever practicable, and their results compared with those of previous similar investigations by other hands.

"Thus the Chapter on the Pathological Anatomy of Stricture is mainly a digest of the facts now exhibited in the principal museums belonging to the medical schools of London, Edinburgh, and Paris, in which each preparation has been individually examined by himself. A reference is made in the text

to various specimens of importance, and an account of these is placed in the appendix, for the purpose of facilitating the student's acquaintance with unquestionable examples and illustrations of the facts stated." (Pp. 8, 9.)

Thirdly: Under the head of "reported cases," a large number of hitherto unpublished observations are collected for the purpose of illustrating numerous points connected with the natural history and treatment of stricture. Following these is a "table of cases," 220 in number, "condensed from fully reported cases only, upon the aggregate of which have been founded, in a great measure, the chapters on 'the symptoms,' and on the 'causes of stricture.'" (P. 9.)

Then, in connection with the much mooted question of perineal section, certain data required for its discussion at full length are presented "under the head of 'outlines of cases,' which are merely very short histories containing the principal facts bearing on this question." (P. 9.)

Lastly:—

"No pains have been spared in order to develop the best practical mode of conveying, as far as this can be done on paper, sound information respecting the anatomical relations of the healthy and diseased urethra." "It will be seen," continues he, "that a great number of bodies have been examined to supply the facts related. One, out of several illustrative preparations which were sent into the College of Surgeons with the Essay, contained portions of the corpus spongiosum from not less than twelve bodies, to illustrate a point in its anatomy referred to at pages 38-41." (P. 10.)

On looking over the list of contents, we find them to be divided into twelve chapters, occupying three hundred and eighteen pages; and an appendix, already alluded to, of six valuable notes, which, with the index, take up the remaining hundred pages. These chapters are furnished with marginal headings; a means of ready reference of so much utility, and so rare, unfortunately, in medical books, that we consider the author entitled to special thanks on their account alone.

Chapter I., "On the Anatomy and Physiology of the Male Urethra," is one of the longest and best of the book. It yields to none of them in interest and character, and would stamp its author as a profound investigator, even were it the only original portion of his treatise. The descriptions of previous anatomists are diligently and faithfully compared with the results of his own "repeated dissections and researches into certain conditions of the organ; which are illustrated in the tables and drawings accompanying this volume."

Comparing his measurements of the male urethra after death with those of Mr. Briggs during life, he calls attention to the difference in length under the different conditions of life and death.

"It will, therefore, be borne in mind that these two measurements of 7½ inches, and 8½ inches, respectively, relate to the average length of the urethra in the two conditions of life and death. That this difference exists, it will be particularly important to recollect since all accurate researches into the pathological anatomy of stricture are, of necessity, confined to an observation of the parts *after death*; while in relation to treatment, the measurement *during life* is that which alone must be remembered." (P. 4.)

In relation to width, or capacity for extension of the urethra, we have room only for the concluding remark.

"As regards the actual average of measurements met with in practice, it is seldom that No. 12 cannot be fairly introduced into the adult urethra, while Nos. 13 and 15 are often admissible. The diameters of these instruments are, respectively, three-tenths and three-and-a-half tenths of an inch." (P. 8.)

Passing over the anatomical divisions of the urethra, its mucous membrane, the perineal fasciæ, and the characters and relations of these parts respectively, we are arrested by a most interesting account of the muscular tissues, and especially of the "involuntary muscular fibre," which the researches of Hancock and Kölliker have already introduced to general notice. The speculations of John Hunter, Home, Wilson, and their adversaries for and against the probable existence of these fibres, are successively reviewed; and the microscopical demonstrations, first of Kölliker and then of Hancock, are given at full length in their own language. The views and reclamations of the latter author are well known to English readers through his admirable "Lettsomian Lecture," and subsequent tract "On the Anat. and Phys. of the Male Urethra, and on the Path. of Strictures of that Canal," published in 1852. Mr. Thompson endorses the authority of the *Cyclopædia of Anatomy and Physiology* in giving to the German microscopist the credit of "first publishing the fact of the existence of involuntary muscular fibres, although the account he (Kölliker) gives is not in all respects corroborated by the researches of English anatomists." (P. 18.)

Mr. Hancock, on the other hand, who concedes "the priority of noticing these fibres" (see Hancock, *op. citat.* p. 9), is supported by our prize essayist in claiming the honour "of describing their situation and arrangement, and their importance as bearing upon practical points." (P. 19.)

Hancock's description, according to Mr. Thompson, "is more definite and comprehensive than Kölliker's, and possesses additional value from the fact that it appears to have been rendered altogether independently of any other, and in unconsciousness of its existence. It moreover contains fresh information respecting the subject, while it is somewhat at variance with the statements of Kölliker, in one or two particulars." (P. 20.)

In corroborating the demonstrations of his *confrère*, our author gives the following directions as to the best mode of dissecting for the purpose:—

"Lay open a urethra from the upper part; stretch out a portion by means of pins upon a board, and dissect up carefully a small flap of mucous membrane from any part of the canal, that of the prostate or of the glans penis being the parts from which they can be most easily demonstrated; and the elastic and non-elastic fibres, before seen lying beneath the transparent membrane, are exposed; these being removed by degrees, a grayish layer comes into view, a small portion of which, placed under an object-glass of a quarter of an inch focus, with a small quantity of water, will exhibit the appearances peculiar to the unstriped fibre, which it is unnecessary to detail here."

He remarks, in continuation, that "it is, however, a subject well deserving further investigation, as it requires to be explained how it is that the adult urethra, in which we might naturally expect them to be most fully developed, affords them less readily than that of the foetus." (P. 23.)

Next in order come a very instructive, and in some respects original, series of reflections on the anatomy and physiology of the voluntary muscles of the urethral canal; on the structure and relations of the corpus spongiosum, especially of its bulbous portion and the fibrous covering and partition of the latter, together with the distribution of its arterial branches, in relation to hemorrhage; on the direction of the canal, and its relations with the fascia; lastly, on the urethral curve, with its variations in youth, adult age, and in individuals of spare, corpulent, or other conformation. The discussion of the urethral course and curve, and the practical inferences derived therefrom, are illustrated with the best diagram and dissection that we have ever found in Europe or America, although we have long been in the habit of looking for such a preparation at every opportunity within our reach. There is so much practi-

cal information given in this latter division of the chapter, that we would gladly transfer a considerable portion of it to our pages. Justice to the remaining topics of the work, however, obliges us to restrict ourselves to the *resumé*, with which it terminates.

"1. That the urethra is composed of a delicate and sensitive mucous membrane, exceedingly vascular and well supplied with nerves, the area of which is increased by numerous small glands and follicles; and that it is closely connected by its submucous areolar tissue with *involuntary muscular fibre in every part of its course*, the distribution of which is not quite equal in quantity throughout.

"2. That in some parts lying between the two, in others, often interlacing with these contractile fibres, but for the most part lying in longitudinal bundles beneath the mucous membrane, and united by transverse fibres, is also a varying amount of the *yellow elastic tissue*.

"3. That in the PROSTATIC AND IN THE SPONGY portions of the urethra, the glandular and erectile structures respectively, lie next in order to the above-mentioned contractile tissues (proceeding from within outwards), are both largely composed of involuntary muscular fibres, and enveloped by an outer layer of the same, which, while they act by evacuating, in either case, the contents of the organ—in the one, a glandular secretion; in the other, the blood supplied for erection—form also an agency which, in certain states, is brought to act more or less on the capacity of the urethral canal, and this agency may be somewhat increased by the co-operative action of the accelerator urinæ muscle.

"4. That in the MEMBRANOUS portion there is also *close contact of voluntary muscle*, the disposition of the fibres of which is such that it cannot be doubted that, whatever may be its degree or extent, its function is to close the canal at this point; the sphincteric character of the muscle being most strongly indicated by its structure, as well as by what we infer respecting its actions, as manifested by phenomena both natural and morbid.

"5. That not only does vascular or erectile tissue surround the whole of the spongy part of the urethra, but that a thin layer of it encircles the membranous portion also, and that from the peculiar structure and function of this tissue, laceration or division of it may be attended with considerable loss of blood.

"6. That while the PROSTATIC part is movable to a small extent in a direction upwards and downwards in obedience to muscular action, the MEMBRANOUS is nearly fixed and constant in position, from the application of unyielding structures (fasciæ) to it, in such a manner as greatly to limit the mobility of the part; and lastly, that, within certain limits, the SPONGY part is movable in any direction, the bulbous portion being less so in the ratio of its proximity to the anterior layer of the deep perineal fascia by which it is partially retained *in situ*; as well as by the corpora cavernosa, and by the triangular ligament above, uniting the penis to the pubes; the anterior two-thirds of the passage (more or less in different subjects) being, for the most part, perfectly free from mobile." (Pp. 47-48.)

"Classification and Pathology of Strictures of the Urethra," are the subjects of the second chapter. Under this head we have, in the course of forty pages, abundant evidence of the characteristically laborious, as well as practical and discriminating manner in which our author masters the various questions which have puzzled his predecessors in the same field of investigation. His unwearied examinations and analyses of the vast number of cases and preparations continually referred to throughout his book, have given him a vantage ground, which places him beyond the reach of most competitors, while they must save a world of trouble to all who may hereafter follow him in similar pursuits.

He adopts the classification of strictures into permanent and transitory, the former only being organic and the latter being inflammatory or spasmodic.

In studying the morbid anatomy of permanent organic stricture, he has deduced it, in a great measure, "from a close personal examination of more than three hundred preparations of stricture in the museums of this country [Great Britain] and of Paris, and of a number, almost equal, of preparations of the bladder, kidney, &c., which illustrate concomitant morbid conditions; as well as from the observation of recent specimens in the dead-house, which has been enjoyed by the author to a considerable extent."

We have room to note a very few of his conclusions. He believes that obliteration of the urethra by actual adhesion of its walls "probably never" does occur, unless as a consequence of fistulæ, "when, although very rarely, this accident may happen." "Nevertheless," continues he, "obliteration of the urethral canal does occur, but it is almost invariably of traumatic origin." He has "once, and once only, met with an impermeable stricture in the dead-house, and failed in attempting to pass a bristle or an eye-probe through it, after the urethra had been slit up to the point of contraction; urinary fistulæ were present." (P. 64.)

With regard to the locality of stricture, his observations lead him to coincide unhesitatingly with the prevalent opinion which places the most frequent seat of the contraction "at the posterior limit of the spongy portion." In designating the different points of coarctation, he considers that the only accurate method is to identify them with the anatomical regions of the urethra, since all measurements from the orifice merely must be wanting in precision. In accordance with this view, and desirous also of simplifying the arrangement as much as possible, he has comprehended all examples of the disease in three classes:—

"I. *Strictures occurring at the subpubic curvature, i. e., at the junction between the spongy and membranous portions and its neighbourhood; the latter term being understood to comprise an inch of the canal before, and three-quarters of an inch behind that point, thus including the whole of the membranous portion.*

"The junction itself is the point at which stricture is most frequently situated. Next is the extreme anterior boundary of the division, a spot which is one inch in front of the preceding, and almost as frequently affected; while, between these two points, six examples of stricture are met with for every one behind the junction, in which latter situation, therefore, they are very uncommon. Most rarely is a stricture found so far back as the posterior part of the membranous portion.

"II. *Strictures occupying the centre of the spongy portion, i. e., a region extending from the anterior limit of the preceding, to within two inches and a half of the external meatus, and measuring, therefore, about two and a half to three inches in length.*

"III. *Strictures occurring at the external orifice and within a distance of two inches and a half of it.*"

The following analysis of two hundred and seventy preparations referred to is then given; these preparations exhibit three hundred and twenty distinct strictures, viz:—

In region I. 215, or 67 per cent. of the entire number; in region II. 51, or 16 per cent.; in region III. 54, or 17 per cent. = 320. Of these there were—

185	examples of one stricture only, situated in region I.
17	" " " " " II.
24	" " " " " III.

There were 8 cases in which the urethra was strictured in all three regions.

"	10	"	"	"	"	in region I. and II. only.
"	10	"	"	"	"	I. and III. only.
"	13	"	"	"	"	II. and III. only.

Lastly, he confidently asserts "that there is not a single case of stricture in the prostatic portion of the urethra to be found in any one of the public museums of London, Edinburgh, or Paris." He is "disposed to believe that some observers have been deceived in reference to it, or that it owes its supposed existence to inferences drawn from the results of examinations of the living body, which can by no means be admitted as evidence on this subject. * * * * At present, therefore, the existence of prostatic stricture appears to rest on the observations of Leroy d'Etiolles and Ricord." We are under the impression that a case is reported also by Lallemand. At all events, as Mr. Thompson justly says, its excessive rarity at least is demonstrated.

Chapter III., which, like all the others, presents some very useful practical lessons, is occupied with the "Symptoms and Pathological Effects of Organic Stricture." The general and local consequences of long-continued or aggravated and even of slight affections of this kind, the fatal effects of slight injuries to the urethra in some cases, the dangers of rapid or extreme dilatation, the frequency of anomalous neuralgic pains dependent on the presence of stricture—are all judiciously impressed upon the reader's attention.

Chapter IV. presents us with a valuable discussion of the "Causes of Organic and Permanent Stricture." After quoting the leading authorities in relation to this topic, Mr. Thompson then turns to his tabulated cases. Of these, he has collected and arranged for the purpose from various reliable sources, two hundred and twenty, which are presented in the Appendix, Note F. We quote the analysis of these cases with which the chapter concludes:—

"Antecedents or supposed causes of organic and permanent stricture and gonorrhœal inflammation, in 164; injury to perineum, in 28; cicatrization of chancres, 3; ditto after phagedæna, 1; congenital, including cases in which the urethra may have been small from malformation, and those in which marked irritability of the organs existed from childhood, accompanied by an unusually small stream, 6; poisoning by nitrate of potash, lithotrity, masturbation, of each, 1 (Lallemand); met with among the ordinary published cases in the journals, 3; true inflammatory stricture, including temporary stricture and retention from a sudden acute inflammation, usually caused by some excess, and disappearing by resolution, 8; true spasmodic stricture caused by irritation about the rectum, 2; ditto without assignable cause, 2; ditto caused by undue acidity and alkalinity of urine, 3. Total, 220.

"Respecting the first class of cases, the following facts are elicited: Of the 164 cases attributable to gonorrhœa, in 90 the disease is reported to have been chronic or neglected; in 3, it was attributed by the patients to strong injections; in 6, the discharge is stated to have ceased entirely and rapidly under treatment; but in 5 of them, stricture appeared almost immediately after; in 4 other cases the stricture appeared to be almost simultaneous with the gonorrhœa; in the remaining 61, there is no report of chronicity, &c. Of the 164 cases attributable to gonorrhœa, 10 appeared immediately after or during the attack; 71 appeared *within* 1 year of its occurrence; 41 *within* 3 or 4 years; 22 *within* 7 or 8 years; 20 are reported at periods between 8 and 20 to 25 years." (Pp. 132, 133.)

Next in order comes Chapter V., "On the Pathology of Strictures of Transient Duration." This treats of a class of disorders which are continually urging themselves, and often most unpleasantly, on the attention of the general practitioner as well as the professed surgeon, since they more frequently prevail among the wealthier portions of the community. The hints it conveys are therefore especially important to every medical inquirer. We have marked several, but must leave them to be found in the book itself, while we hasten on to the "Diagnosis and Treatment of Stricture of the Urethra,"

which occupy our author in his remaining chapters, with the exception of the tenth, eleventh, and twelfth.

Here we are at once introduced to the endless armamentarium of sounds, bougies, and catheters, in all their conflicting varieties. Hard, soft, and flexible—long curves, short curves, and angular—straight instruments and twisted—blunt ends, olivary, fusiform, and conoidal—stylets, lancets, and porte caustiques, one after the other, pass before us in a motley crowd to usher in the no less varied arts and mysteries of cast taking, measuring, the *tour de maitre*, preliminary sounding, vital dilatation, gradual dilatation, permanent dilatation, immediate or forced dilatation, cauterizing, internal incision, and external incision. These, and other perplexities and terrors of present and former days, are marshalled in their wonted shapes of wax, gum, tin, lead, steel, German silver, and genuine silver; but they are not, as of old, allowed to overwhelm the bewildered student and practitioner. Each implement and process, that deserves it, is fairly and rationally considered; and many of both are put aside, we trust, forever.

The diagnosis of stricture can, of course, only be determined by physical exploration, which must be effected by the passage of an instrument along the suspected canal. For this purpose our author prefers, as most reliable and most easily directed, “a solid silver sound, or, which is much cheaper and almost equally useful, a well-polished steel one, silver plated, the chief advantage of the former material being that it permits slight alteration to be made in its form for special cases, without sustaining injury.” The reasons given for this preference are the usual ones, and need not be repeated here. Our own experience has long since led us to the same conclusion in ordinary practice.

With regard to the curve of a sound or catheter, he remarks, that it should naturally be adapted to that of the least movable portion of the urethra itself, which he has previously shown to be “equal to a portion of the circumferential line, equal to about three-tenths of a circle three inches and a quarter in diameter.” The instrument, in its transit to the bladder through a healthy organ, “must describe a curve, and turn round an axis which may be imagined to exist about the centre of the symphysis pubis;” and, necessarily, will most readily do this if its own curve corresponds with that described (p. 158). This principle is very clearly illustrated by a diagram representing a catheter and bougie of the size and curve proposed. He further informs us that he had adopted and advocated such a curve on the ground of practice after long trial of different forms, and before having arrived at any theoretical conclusions on the subject. We are glad to find an old and well-tried predilection of our own for a very similar bend confirmed by such authority. Although it is well known, as he reminds us, that different curves have been equal favourites and equally successful in many eminent hands; and that under the direction of an able operator almost any form may seem the best, still the direction which would be most likely to facilitate the manœuvres of unpractised manipulators is undoubtedly of serious importance. We, therefore, second his advice respecting catheters and sounds as, throughout, worth the especial attention of the great majority of general practitioners, and, above all, of the beginners in this exercise. A very few trials on the dead or living body, and especially upon themselves, would probably assure them of the value of the precepts which Mr. Thompson has explained and applied with mathematical clearness and precision, while he appeals to a prolonged and abundant experience for their practical demonstration. In speaking of the eyes of catheters, he objects to the usual size, and advises that smaller openings

should be made, on account of the protruding of the mucous membrane into them when too large. We have often been struck with the increased pain and difficulty arising from the suction of the mucous lining through the apertures of the ordinary catheter during the exit as well as entrance of the instrument, especially when quickly done. We have no doubt that it is a frequent though rarely appreciated source of trouble to the patient, if not to the surgeon; and hence have for years past preferred using an instrument perforated around its extremity, like the female catheter, with numerous small holes instead of the larger elliptical fenestra on the sides. The objection to these cribriform terminations, that they are apt to become clogged, is not found to be very great in practice, and is easily obviated by slightly enlarging the holes so as still to secure the object which our author seeks in reducing the dimensions of the lateral clefts.

Mr. Thompson's whole account of the mode of introducing the catheter may be as warmly recommended to our junior readers as that of the instrument itself. It is admirably clear, judicious, and complete. "The more quietly, gently, and unostentatiously these manipulations are accomplished, the more credit will the operator obtain for the possession of a light and easy hand."

We shall never forget the lesson long ago afforded us, and after a good deal of hospital practice and observation elsewhere, by the example of Civiale, in the slowness, steadiness, and perfect gentleness with which that unsurpassed, if equalled, catheterizer invariably operated on his patients. "Whatever the obstruction, it is never to be carried by storm. A patient, persevering, and unruffled spirit, with a dexterous hand, will work miracles in cases of difficult catheterism" (p. 167). How many a poor fellow, in his hour of agony, has felt the force, either for good or evil according to his trial, of the *caveat* thus announced?

A protest has been entered against the suggestion of Mr. Thompson, that the surgeon who wishes to excel in this kind of performance should try it on himself. The idea is not a new one. It is an old and favourite expedient with some practitioners who certainly did not think of martyrizing themselves, although they wished to save their patients. *Ad hominem* though it be, we willingly subscribe to this mode of training, and none the less so because we have some knowledge of its convincing properties from personal experiment. The only other mode of arriving at an equally positive idea of the action of different forms, sizes, and kinds of instruments upon a healthy urethra is to make one's self the *experimentum crucis*, under the tender mercies of a second party; a stretch of devotion to the cause of good catheterism which we freely confess to be even beyond our ambition. Cautious self-catheterizing, however disagreeable, ought not to be lost sight of by the enterprising student, as the very best means of acquiring a practical understanding of the art in general.

The operator who is unable to withstand such a trial of his strength of nerve can hardly be qualified to perform the office for another. Indeed, we hold that no man of an irritable temperament, or little self-control, should ever undertake the introduction of a catheter in any case, much less the treatment of a stricture. "*Festina Lente* must be a motto never for an instant to be forgotten; one moment's loss of self-command, and irreparable mischief may be done." (P. 180.)

The innumerable plans of treatment are resolved into three classes: Those which produce dilatation through pressure, mechanically and by absorption; those which effect a similar result through the chemical or absorbent action of caustics; and those in which the opposing tissue is divided from within or from without, by some cutting instrument, each process being, of course,

accompanied by the use of constitutional remedies. The employment of dilatation is placed first in order, as not only the oldest, but by far the most desirable and most generally applicable method of treatment. It is the one which is resorted to in the vast majority of instances as the rule, and only laid aside for other modes when proved ineffective or insufficient to maintain a cure.

The author's views as to the proper mode of ordinary instrumental and general therapeutic management, are very well shown in the statement of two hypothetical cases, the one simple and the other more difficult. In reviewing these and other illustrations we have not time to follow him in detail, and must content ourselves with an occasional note. He uses a metallic sound or catheter whenever the size of the opening through the stricture will admit of it, advises an attempt at gradual dilatation in all cases, and strongly deprecates the exercise of violence or haste. In certain difficult forms of stricture, he is satisfied that where no false passages exist, and when they can be avoided, "the employment of continued pressure on the face or in the commencement of a stricture is almost uniformly successful." The operator must take care, however, that he is really acting on the contraction alone. He must remember as an invariable test of genuine progress, that when the instrument is tightly grasped he "may infer that its point is safe within the strictured part, but that when the point feels free, movable, and capable of being withdrawn without appreciable effort, it is certainly not in the stricture; it may be, in such circumstances, in a false passage."

In the management of a narrow stricture, he advocates, with good reason, we think, the employment of conical sounds, and proposes three different sizes, made of solid metal and well polished, as an efficient set. Silver-plated steel sounds of this form have long been very widely used in Philadelphia, and, so far as we have learned, with general satisfaction.

Respecting the use of force, which may be allowed in particular exceptional cases, the following conditions are stated as indispensable:—

1st. When required, "it may only be made *after the point of the instrument has well and fully entered the stricture*, so that the operator is assured that it is in the right track. Force is never to be employed in order to *make a way into it*."

2d. "It is then to be increased very gradually, first trying one uniform rate of forcible pressure for two or three minutes before proceeding to increase it; and, when it is found necessary to do so, the increase must be continued in the same manner for a similar length of time. It is not to be accomplished by pressing onwards with more and more violence, until something gives way. This is never legitimate."

3d. "Complete knowledge of the anatomy of the passage, some acquaintance with its diseased conditions also, and long experience in the use of instruments in it, are the indispensable qualifications of the operator to whom the use of force is permitted." (P. 183.)

Not satisfied with these express rules, he follows them up with a warning as to *when* force should not be used, so anxious is he to be clearly understood.

In dealing with those most troublesome accidents, false passages, we find two valuable hints, which, although not new, deserve renewed attention.

1st. "That they almost invariably commence on a level *below* that of the proper opening; and, secondly, that the operator's finger when in the rectum, near to which the false passage is almost certain to run, will communicate information as to the route which the catheter is taking, whether it be too close to the gut, or deviating to the right or left of the median line; it will, moreover, be serviceable in assisting him to guide the point in the true direction." (P. 187.)

The remainder of Chapter VI. is occupied with the careful consideration of many topics of practical interest; such, for instance, as the relative merits and respective uses of flexible and inflexible bougies; Dupuytren's method of retaining a catheter for hours or days *against* a stricture, called "vital dilatation;" the method of retaining a catheter *within* a stricture, or "mechanical dilatation;" the injurious effects of rapid or extreme dilatation, &c. &c. The closing paragraphs of this chapter relate to the unfortunate fact that there are cases in which dilatation is only temporary in its action, and which are therefore incurable by this method. The question is consequently asked, In what manner can such cases be relieved? The answer to this follows in the two succeeding chapters, VII. and VIII., which contain a full and very fair discussion of the use of "chemical agents" in the shape of caustics, and of the resort to cutting instruments internally and externally.

His personal acquaintance with the caustic treatment does not appear to be extensive, and his impressions are certainly unfavourable. Still, he gives us a candid and sufficiently comprehensive history of the escharotic method in the hands of its most prominent British and French advocates, from Wiseman and Paré down to Leroy d'Etiolles and Wade of our own day.

Nitrate of silver and caustic potash are the only caustics used, the former being preferred exclusively by most authorities. All modes of application may be resolved into two categories. "Either a small instrument containing it is introduced into the stricture, which cannot then be a narrow one; or a small portion of the agent is carried down to it and pressed against its anterior surface." His conclusions are expressed in the following extract:—

"That these agents are never to be employed for the sake of their escharotic or caustic powers, properly speaking."

"That the nitrate of silver, lightly applied, exerts a salutary action on the diseased surface of the urethra, relieving inordinate irritability, and checking undue vascularity and disposition to hemorrhage, as it does in similar conditions of the skin and mucous membrane in other parts of the body, and thus it becomes a useful adjunct to dilatation."

"That the potassa fusa, as a caustic, is considerably more active than the preceding, and is therefore more dangerous of application. If used at all, it should be only in *very minute* quantities, such as fractional parts of a grain, inasmuch as it is exceedingly difficult to limit the action of so powerful an escharotic. It may perhaps aid dilatation in the reduction of some strictures, probably by facilitating the solution of some of their component tissues, when care is taken to employ it in obedience to the condition just named." (P. 221.)

We are inclined to consider this appreciation just enough, as far as it goes, but would have been glad to see it carried on a little further. Under the direction of a tyro or incompetent operator, the employment of any escharotic in the urethra might be very dangerous; but, in skilful and experienced hands, we know it to be not only safe in many instances, but highly serviceable. We have heard of cases in which frightful mischief has been produced by the improper use of caustic potash; but from what we have repeatedly observed of its careful application as a powerful stimulant to absorbent action on other surfaces, we have no doubt that, when properly diluted and otherwise modified or guarded in its action, and *opportunistically* employed, it may prove a most valuable aid to milder means. So, also, with nitrate of silver, with the action of which, combined and uncombined with milder salts, we have long been more familiar in the management of stricture. So much depends upon the strength, the mode and the time of application, that the philosophy of this species of treatment would seem to consist, as it does even with the bougie, rather in attention to the minor details of its application than

in the mere indefinite resort to one or the other of two powerful agents in their primitive shape. The objections and caveats are levelled against their abuses only, while their true applications and legitimate availability are scarcely estimated in the manner which the fulness and precision of the previous chapters had led us to expect.

We come next to the treatment by intra-urethral incision, which is the subject of Chapter VIII. Passing over the excellent history and principles of this plan of treatment, together with the author's brief but comprehensive descriptions of the various instruments and modes of operating, we shall proceed to quote his summary of indications.

"1st. When dilatation has been sufficiently tried, and does not afford relief, or that which is temporary only in its duration (and the stricture is not of an irritable character, in which case, as we have seen, a trial of the caustic may first be made);"

"2d. And the stricture is known to consist of a mere fold of membrane, or, at all events, is a short one, *i. e.* of small extent from before backwards;";

"3d. When this is situated in the anterior three or four inches of the urethra, the case is one which most probably will be successfully treated by division, according to the modes already recommended, the choice of which should be determined by rules already given." (P. 233.)

The objection to all internal cutting instruments is that the operator has no means of ascertaining clearly how far or what he cuts. Instruments for the purpose ought to be constructed with a guide.

"Thus the attempt to perforate an obstruction otherwise impassable by pushing a pointed blade into it without a guide, must be always somewhat hazardous; extremely so, if it be attempted in the curved part of the urethra. * * * Less dangerous is it, as we shall hereafter see, to lay open the perineum and divide the stricture from without, thus giving free vent to noxious fluids of all kinds, than to wound the urethra from within, at or behind the bulb, as we run great risk of doing when operating at six inches distance from the external meatus, and thus only make a channel for these matters into the erectile cavities and other structures round." (Pp. 224, 225.)

Chapter VII. concludes with a short summary of the general rules of treatment thus far established.

"*Dilatation* has been found successful for the great majority of cases, but certainly inefficient for the cure of some old and extensive strictures, as well as for some which are accompanied by a highly irritable condition of the urethra.

"*Cauterization* must be regarded as a useful adjunct to dilatation in some few cases, especially in some of those in which a considerable degree of irritability exists. It is wholly inapplicable to the removal of old and extensive contractions.

"*Internal division* is particularly suited to these last-named cases, when situated in the anterior part of the urethra.

"There remain, therefore, by process of exclusion, some *very irritable strictures*, and some *obstinate and extensive ones*; the latter being usually situated about the junction of the spongy and the membranous portions, or a little anterior thereto, for which at present no adequate remedy has been described." (Pp. 234, 235.)

These are the bad cases on account of which, in addition to those of retention and of urinary fistula, the more extended application of external urethrotomy has, of late years, been especially discussed. It is well known that the operation of perineal section, with or without a guide, but particularly in impassable stricture—the buttonhole incision, or *bouttonnière*, as the French call it—is a very old one, familiar in some shape or other as one of last resort for at least two centuries.

The earliest record which Mr. Thompson gives us of such an operation "performed for the cure of stricture, and not for the relief of retention," is that of an unsuccessful one related by Wiseman (*Chirurgical Treatises*, 5th ed. vol. ii. p. 427) as having been attempted in 1652. We find in Saviard's *Surgical Observations* (London Translation, by J. S., p. 167), a report of an equally unfortunate attempt made by himself in 1692.

Other and later French and British Surgeons appear to have tried it for the same purposes from time to time, with various success; and in America, it was advocated in 1824 by Dr. H. G. Jameson, who had practised the incision in the median line for the cure of obstinate stricture as well as for the relief of retention, first in 1820 and repeatedly thereafter, with results so gratifying as to induce him to recommend the operation as not only very efficient but entirely safe. (*Medical Recorder*, vol. vii. p. 283, and p. 713.) Mr. Thompson shows that the practice of the operation, both through the raphé and by its side, in cases of retention, appears to have been adopted by British surgeons early in the present century. It has certainly been frequently resorted to in the United States as well as in the British Islands and on the European Continent, throughout the last thirty or forty years, in the management of perineal fistula and as a favourite means of rescuing the victim of retention from the dangers of this accident. The operation has long been a familiar one in Philadelphia (see for instance Horner's lateral section, in H. H. Smith's *Operative Surgery*); and the recent interesting paper of Dr. Lente (quoted in the last No. of this Journal, from the *New York Journ. of Med.* for March, 1855) proves that it has been equally common in New York. Fergusson tells us, in his *Operative Surgery* (4th Am. ed. p. 583), that he has been accustomed to see and treat cases of the kind with the usual cutting operation for the last five and twenty years. Bransby Cooper says much the same. Mayo and others also refer to it in a similar manner. Guthrie describes the section in the perineal raphé as first taught by him in 1816, and published in 1830. Mr. Simon alludes to Guthrie's operation as the one in established use among the hospitals of London for the last thirty years. We have, also, among the recent French descriptions of it, that of Vidal de Cassis, who is decidedly in its favour as less formidable than is generally supposed; and that of Sedillot, which corresponds, in its method, with the procedure as modified by Avery and recommended by our author. All have regarded it as a very serious expedient, to be undertaken chiefly if not solely as a *dernier ressort*, although one that should not be deferred, as it too often is, until the patient is likely to sink under the effects of the mischief produced by the injury itself.

We must refer the reader for the detailed steps of the operation, as performed in cases of impermeable stricture, and for Mr. Thompson's appreciation of its value and particular uses, to his own pages; and shall proceed at once to the new application of perineal section as presented by Mr. Syme.

Although the idea of performing external incision, with a guide in the urethra, and for the cure of stricture alone, is an old one, and occasionally put in practice as well as recommended on both sides of the Atlantic long before the professional advent of its Edinburgh advocate; still that able and indefatigable surgeon is entitled to all the credit due to its establishment as a standard operation. He deserves, too, the perhaps more substantial honour of having first definitely ascertained and announced the principles of this mode of treatment, and, in accordance with these, the proper method of procedure before and after, as well as during the performance of, the operation.

Mr. Thompson appears to us to have succeeded in preparing not only "a fair and correct exposition of the much vexed question, but the best and most useful one yet published;" and in doing so has very materially enhanced the value of his monograph. It is, to use his own words, truly "a laborious, careful, and, as far as possible, an unprejudiced examination of the evidence presented in relation to the subject;" and, as such, must particularly interest our readers now that a considerable interval has elapsed since the discussion first arose. We regret, therefore, that the already tedious length of this review will oblige us to curtail the summary of this portion of our author's essay.

Putting retention of urine and urinary fistula out of view, and confining ourselves to the consideration of stricture solely, Mr. Thompson reminds us of the old axiom among surgeons, both in theory and practice, "*that when a sound of any size can be passed through a stricture into the bladder, division of the stricture from the surface of the perineum is certainly contra-indicated.*" Mr. Syme reverses the rule here stated, and proposes to make "*permeability an indispensable prerequisite to the performance of external division.*" He has some most respectable followers in this innovation, and, as a matter of course, many eminent and uncompromising opponents. The exclusiveness and ultraism, however, which formerly retarded the progress towards a reasonable investigation of the matter, are evidently giving way in the lapse of time to enlarged experience and calmer observation on both sides; and the conflicting parties are approaching to a common ground which will prove more tenable than either of the positions so bitterly maintained at first.

Mr. Syme and some others originally disbelieved in the existence of "impassable stricture." In this the positive experience of several unquestionable observers is decidedly against them; and even Mr. Syme himself has been obliged not only to qualify his first assertion, but to modify his individual belief. Mr. Thompson admits the possibility and actual occurrence of impermeable stricture, but adheres to the opinion that it is so rare as to be practically almost unknown; and that, therefore, in the vast majority of cases, the ordinary mode of perineal section without a guide inserted through the seat of stricture, is really unnecessary, as Mr. Syme avers; while the "external division" on the grooved sound suggested by the latter, may sooner or later be effected, and must be taken "*as the rule.*" The real amount of permeability must vary in different hands and under different circumstances in the same patient. It can only be approximatively determined in a number of exceptional cases already sufficiently large in the practice of undoubtedly skilful operators to warrant the more difficult and dangerous operation, notwithstanding the remarkable success of Mr. Syme in overcoming the difficulty which necessitates a resort to it by others. The difference, after all, is more verbal than practical among competent men. "Most assuredly," says Mr. Thompson, "the cases are few in which a sound may not be passed by a skilful and persevering operator, perhaps fewer than they have generally been supposed." A recent notice of the second edition, just published, of Mr. Syme's work *On Stricture of the Urethra and Fistula in Perineo* (*Edinburgh Monthly Journ. of Med.*, for April, 1855), affords us the opportunity of quoting his latest experience in relation to this and other points at issue in the controversy.

"Cutting into the perineum without the assistance of a precise guide, exposes to the serious danger of opening the urethra on the wrong side of the stricture, of breaking through the deep fascia and of wounding the artery of the bulb, so as to incur the risk of urinary extravasation and hemorrhage,

while pressure being the only means available to suppress the latter, must greatly tend to promote the former evil. It is, therefore, no wonder that this procedure has been looked upon as a forlorn hope, warrantable only in cases of impermeable stricture. But while admitting, as I have already done, that, in some rare cases, the urethra may be actually obliterated, I maintain that no stricture is impermeable, and that if a drop of urine is able to escape, a director of sufficiently small size may be introduced; and in support of this position I appeal to the fact that, although patients alleged to labour under impermeable contractions have come to me for relief from the most distant parts of Scotland, England, and Ireland, from the Colonies and from America, I have never, either publicly or privately, been unable to pass an instrument since I became satisfied that there was no true impermeability."

"Some advocates of impermeability, indeed, allege that those who deny the existence of this condition effect a passage by force; but as the stricture is tougher than the sound urethra, and as, therefore, any passage accomplished by force must necessarily be a false one, which would aggravate the patient's case instead of remedying it, the satisfactory result of treatment affords a most complete refutation of such statements. As already said, it is far from my intention to allege that the introduction of instruments may always be accomplished with ease. In general, I have succeeded at the first attempt; but in many cases have had to wait days, or even weeks, before the passage could be hit. Indeed, on three occasions—one in private and two in public—I found it necessary to open the urethra anteriorly to the stricture, so as to obtain the assistance of a finger placed in the canal, to guide the point of the instrument." (Pp. 33—36. See p. 331 of the Journal just quoted.)

Mr. Syme's adversaries cannot fail to be much edified with the new method of insinuation which he rather naively announces as having at last to be resorted to before the passage "could be hit." Such an admission looks extremely like a virtual surrender of his previous assumption, since it proves him to have been foiled, and ranges him inevitably with Liston and other champions of permeability, who have been weighed in the balance and found wanting. The manœuvre, which is here only the prelude to the final incision, strikes us as closely resembling the anterior opening which Mayo in his *Outlines of Pathology* (p. 404, Am. ed.) designates as a material part of the main procedure. It is most suspiciously like a "buttonhole" contrivance, and unavoidably suggests the idea of a back door in the operator's argument.

In one of the two cases that occurred in public, Mr. Syme, according to his reviewer, after mentioning that he had been for two months unable to pass a bougie through a stricture reported impermeable, gives the following account of the plan he adopted:—

"As I had little doubt, however, that the difficulty arose from the form of the urethra where the false passage entered it, rather than from mere tightness of the stricture, I resolved to lay open the urethra on a director in front of the stricture, and then endeavour to guide the director through the stricture by means of my forefinger introduced into the wound; for I had found, in a former case, that the tip of the index finger being inserted into the part of the urethra in front of the stricture as into a thimble, afforded the means of guiding on an instrument through the stricture with unexpected facility. Accordingly, on the 31st August, the patient being under chloroform, I measured with a large bougie the distance of the stricture from the external orifice, and having introduced a director rather larger than a No. 1 bougie for the same distance into the urethra, I pushed it in as far as it would go, and being thus sure that the end of the director was in the false passage, I made an incision in the middle line of the perineum, and laid open on the director the contiguous parts of the urethra and false passage. Having then introduced my finger into the wound, I succeeded in guiding the director through the stricture, and divided it in the usual manner by running the knife along the groove. I had now no

difficulty in passing a full-sized catheter into the bladder." (Pp. 94-96, from *Ibid.* p. 331.)

"This case," as the journalist justly remarks, "appears to throw an entirely new light upon the subject of relieving retention of urine by operation when the catheter cannot be passed in the ordinary way. And, although Mr. Syme tells us that he has never yet found puncture of the bladder necessary for retention of urine depending upon stricture, he must admit that if a case like the above had come to him with urgent retention, before he had hit upon the expedient there made use of, he must have had recourse to puncture of the bladder. For though the difficulty in that case arose from complication with a false passage rather than from tightness of the stricture, yet it was not on that account the less insuperable in the ordinary way. The great obstacle to the management of strictures at the bulb unquestionably is the difficulty of guiding small instruments with precision in that part of the canal, and the means now furnished by Mr. Syme for facilitating this process appears to us a most valuable addition to surgery.—(*Edin. Monthly Journ. of Med.* 1855, pp. 331, 332.)

The forms of stricture which are described by Mr. Thompson as not amenable to the treatment by dilatation or other action within the canal, have been already briefly sketched. We may as well again avail ourselves of Mr. Syme's reviewer to quote that surgeon's graphic picture of the cases which his plan is intended to relieve. These—

"May be distinguished as unyielding, irritable, and contractile. In the unyielding form, dilatation, though it may be carried on to some extent, is sooner or later arrested by resistance of the tough texture at the seat of contraction; and if attempts are made to remove this obstacle by forcible distension, the most serious consequences are apt to ensue. In the irritable condition, while all the symptoms of stricture are presented in an extreme degree of severity, the gentlest introduction of instruments is sure to produce great aggravation, not only at the time, but for days afterwards; so that the patient is distracted between the desire to obtain relief, and dread of the effects resulting from means employed with this view. In the contractile, or spasmodic stricture, as it is usually called, no difficulty is experienced in dilating the canal to its proper capacity; but unhappily with little benefit, the patient still making water laboriously, painfully, and frequently by drops, or in a dribbling stream, which is liable to complete obstruction, through the influence of any local or constitutional disturbance affecting the urinary organs."

"For the treatment of stricture in these three forms, whether existing singly or combined together, the means of remedy hitherto employed have proved quite unavailing; and the patients thus afflicted are deserving to be regarded as a great discredit to surgery. If poor, they frequent hospitals until dismissed by desire from despair of relief, or are declared incurable, or fall victims to practice more zealous than discreet. If rich, they run the gauntlet of European skill, having bougies introduced, caustic applied, internal incisions inflicted, and so on, until, with broken health, disappointed hopes, and perhaps empty purses, they retire in dreary seclusion from society to carry on a dangerous and ineffectual system of palliation through means of the various apparatus collected in the course of their wanderings. That this is not an imaginary or overdrawn picture must be admitted by every practitioner who possesses any considerable field of observation, and will also appear from the cases to be found in any large hospital." (Pp. 25, 26, from *ibid.* p. 330.)

With regard to the alleged dangers of the external incision, as performed by Mr. Syme—hemorrhage and extravasation of urine—Mr. Thompson is inclined, with Mr. Syme, to doubt them altogether as not proven by experience. Hemorrhage may be escaped by adherence to the middle septum of the bulb, and serious extravasation may be rendered equally improbable by limiting the external incision and avoiding the deep-seated fascia and the portion of the urethra posterior to the bulb. The only other risk is from purulent

infection or pyæmia, which is not peculiar to this operation, or as great in it as in the old one. There is, however, a train of nervous symptoms, consisting of rigors, with occasional vomiting, suppression of urine, and delirium, which become at times alarming, and may give rise to great anxiety and suffering during the first two or three days. Mr. Syme insists that they are transient in the great majority of cases, "passing off in a few hours without the slightest disagreeable consequences."

The principles on which this operation is founded are summed up in a recent lecture of Mr. Syme, published in the London *Lancet*, in the following terms:—

"First. Stricture is never seated posteriorly to the bulb, and therefore the incision should never extend further back than the bulb.

"Second. A grooved director must be insinuated through the contracted part, without injury or abrasion of the lining membrane of the urethra.

"Third. If the incision has been properly performed, there is no need to dilate sinuses, which are sure to close when the stricture is removed.

"Fourth. A catheter should be introduced into the bladder after the operation, and retained for forty-eight hours; not less, on account of the risk of the extravasation of urine; and not longer, because it is unnecessary, and apt to do harm." (March, 1855, p. 198.)

We do not believe that Mr. Thompson would endorse the above sweeping assertion that strictures are never posterior to the bulb. Mr. Syme makes this negative statement on the ground that in all his operative experience, which, at the date of his last edition, amounted to 108 cases, he has never had reason to cut further back than the bulbous portion.

The original anticipations of its author in respect to freedom from danger to life in the operation of Mr. Syme, appear to have been entirely fulfilled in his own experience, as well as in the results ascertained by Mr. Thompson.

"Having declined no case presented for treatment, and operated at all ages, from 77 downwards, as well as under every variety of complication from long existence, alleged impermeability, and the false passages of previous mismanagement, I have now performed the operation 108 times, with only 2 fatal results that can be ascribed to it." (Pp. 44, 45, Journ. citat. p. 333.)

Neither of the fatal cases (attributed to pyæmia) appears to the Edinburgh reviewer to be very clearly made out against the operation, since the one was not a fair subject for operation, and the death in the other was probably the result of cerebral lesion. Two cases in over a hundred is a sufficiently small proportion to satisfy every candid reader, whether fairly counted or not in the estimate of liabilities. Mr. Thompson's table gives 113 cases and four deaths, also from pyæmia. We give the table of Mr. Thompson for what it is worth, although, as far as the general results are concerned, we would put more faith in his individual experience, and in that of the original performer, than in that of a number of different experimenters, however able, unless we could be sure that the manipulation and general management were the same, *cæteris paribus*, in all the cases.

By Mr. Syme, above 70 times, no death; a large proportion of the cases successful.

By Mr. Fergusson, 4 times; 1 death; 2 tolerably successful; 1 doubtful.

"Outlines of Cases," Nos. 1 to 4.

By Mr. Cock, 5 times; 1 death; the remainder more or less successful.

"Outlines of Cases," Nos. 5, 6, 7, 8, and 9.

By Mr. Coulson, 8 times; 1 death; the remainder more or less successful.

"Outlines of Cases," Nos. 10 to 17.

By Mr. Erichsen, 5 times; the majority more or less successful; one or two doubtful. "Outlines of Cases," Nos. 18 to 21. "Reported cases," No. 17.

By Mr. Haynes Walton, 1 time; successful. "Outlines of Cases," No. 22.
 By Mr. H. Thompson, 1 time; successful. "Reported Cases," No. 11.
 By Mr. Mackenzie, 7 times; one death; the remainder more or less successful.
 "Outlines of Cases," Nos. 23 to 29.
 By Mr. Dunsmure, 3 times; two more or less successful; one unsuccessful.
 "Outlines of Cases," Nos. 30 to 32.
 By Dr. F. Thompson, 2 times; successful. "Outlines of Cases," Nos. 33, 34.
 By Dr. Cruickshank, 1 time; successful. "Outlines of Cases," No. 35.
 By Mr. Fiddes, 6 times; five successful; one doubtful. "Outlines of Cases," Nos. 36 to 41.

An examination of the detailed histories of these cases does not convince Mr. Thompson that any great danger was incurred from hemorrhage; certainly not more, if not less, than in other operations involving any portion of the urethral bulb. At all events, in the opinion of our author, if bleeding should come on, it can always be arrested. Only one instance of hemorrhage occurred in Mr. Syme's 108 cases.

In answer to the question as to how far external incision should be entitled to acceptance as a standard remedy for the specified forms of stricture, Mr. Thompson gives, as much as present limited experience will justify it, a favourable answer. We must refer to the essay itself, and to Mr. Syme's various articles, for an enumeration of the different causes to which they attribute failures and relapses. To these, also, and especially to Mr. Thompson's admirable account of it, would we invite the careful attention of our readers for an unmistakably precise and full description of the various practical details of the operation and the instruments employed.

The chapters on "Urinary Abscess and Fistula," on "Retention of Urine depending on Stricture," and on "Stricture of the Female Urethra," occupy the remainder of the regular work. They are, like the others, full of valuable information, and may be consulted with signal advantage by every one in search of guidance in the management of those formidable classes of disease.

The second of these final chapters closes, "in pursuance of the principle which has been adopted in regard to each section of this work," with a brief "recapitulation of the CONCLUSIONS arrived at in relation to the entire subject of treatment, in order to afford a summary of the main points which it has been my aim to elucidate in the foregoing pages."

We quote them in full as they follow:—

"1. That the process of dilatation, carefully and perseveringly employed, is the most safe, efficient, and generally applicable of all means for the treatment of organic and permanent stricture." (Pp. 173-4.)

"2. That, while it is successful in curing the majority of cases, there are unquestionably some in which either the effect is so temporary that the contraction reappears on the cessation of the treatment, however long continued, or in which the urethra is so irritable that its employment aggravates rather than removes the symptoms." (Pp. 205, 255-6.)

"3. That the nitrate of silver lightly applied is sometimes useful in the last-named cases, inasmuch as it exerts a salutary influence upon the diseased surface of the urethra, relieving inordinate irritability, and checking undue vascularity and disposition to hemorrhage, as it does in similar conditions of the skin and mucous membrane in other parts of the body, and that it is a useful adjunct to dilatation." (P. 219.)

"4. That the potassa fusa, as a caustic, is considerably more active than the preceding, and is therefore more dangerous of application. If used at all, it should be applied only in very minute quantities, inasmuch as it is exceedingly difficult to limit the action of so powerful an escharotic, and apply it as a solvent only. It appears occasionally to aid the process of dilatation in the reduction of some strictures, probably by facilitating the solution of their component tis-

sues, when care is taken to employ it in obedience to the condition just named." (P. 220.)

"5. That no agent should be employed in any case for the purpose of making an eschar or slough in the urethral canal." (P. 220.)

"6. That internal division is applicable only to strictures which are situated in that part of the urethra which is anterior to the bulb, and which have been found to resist dilatation." (P. 225-6.)

"7. That the distance at which a stricture is situated from the orifice, and the extent to which it implicates the canal, may be so great as altogether to forbid the practice of internal division; for the operation becomes more hazardous just in the ratio of the extent of the stricture, and extent becomes more formidable in the ratio of its distance from the external meatus; so that it is a far easier proceeding to make external division of a large portion of contracted urethra situated in the anterior part of the spongy body, than of a small portion at the bulb or behind it." (P. 234.)

"8. That dilatation having failed after an ample trial, the stricture being permeable and situated near to the junction of the bulb and membranous portion (a spot already seen to be the most frequently affected), external division made from the perineum upon a grooved staff is, for most such cases, a safe and efficient mode of treatment." (Pp. 256, *et seq.*)

"9. That when the urethra is impermeable, every available means having been patiently and perseveringly employed to pass a catheter through it, but without success, the perineal section may be performed as a means of cure." (Pp. 251-2.)

"10. That when it is necessary to make an artificial outlet to relieve retention of urine, an operation may be performed for the purpose of curing the stricture at the same time; but if the condition of the patient require the proceeding employed to be as simple as possible, the puncture of the bladder per rectum is indicated, unless the urethra be dilated in the perineum, when the making a single puncture there will be the best operation to perform." (Pp. 308-9.)

"That it is a matter of great importance in the treatment of old or severe strictures, in relation to the mode of treatment employed, to ascertain what degree of organic renal disease exists as fully as our means of observation enable us; inasmuch as its presence renders *all* operations upon the urethra hazardous, and, for the most part, in a degree corresponding with the extent to which the renal organs are implicated." (Pp. 102-3, and 269.)

"12. That since few permanent strictures exist, which are not considerably influenced at some time or another by the occurrence of inflammation or congestion in the parts around, or by the action of spasm in the adjacent muscular tissues, either separately or conjointly, treatment may be always most advantageously directed to the improvement of the general health, to the subduing of local congestion, and to the removal of those sources of irritation, whether in the urine, in the urinary passages, or in some other and more distant parts, which have been pointed out as liable to excite the phenomena referred to." (Pp. 203-5, and Chapter V., *The Pathology of Strictures which are of Transient Duration.*)

Last, but not least, our author treats us to an appendix, which is in itself a rich and extremely interesting book. It is a nearly inexhaustible mine of practical instruction in its abundant and varied record of clinical and pathological facts. As such, we recommend its frequent study to both students and practitioners. They could not more effectually improve themselves in their application of the principles so amply and distinctly developed in the previous pages of the volume.

Urethral stricture, of all the ills that flesh is heir to, does most notoriously expose its victims to the charlatan and the sciolist. A really sound essay, therefore, which covers the whole ground with the ability and candour of the one before us, ought to be welcomed as a more than ordinary boon to erring humanity. It cannot be denied that, however copiously and wisely the sub-

ject has been dealt with by authoritative teachers from the time of Paré and Wiseman until the present day, the thirst for gain and novelty, the spirit of ambition and adventure, are ever actively engaged, with often reckless and fearfully mistaken zeal, in devising new means and appliances for casting off the burthen of misery under which so many impatient sons of Adam are in this way doing penance for their sins. A contribution of the character and standing of Mr. Thompson's, should for its beacon-light alone be cherished as a work of peculiar necessity and value. Its appearance, along with that of a few others of similar tone and tendency, will be prized as the best evidence of decided progress in this branch of surgical pathology and therapeutics; since they are founded on the only true basis of extended personal observation and research.

E. H.

ART. XIV.—*Inquest on the Death of Agnes E. Lottimer*, before Dr. GEORGE C. BALL, Coroner, Brooklyn, N. Y. New York, 1854, pp. 44, 8vo.

It is not often that we have an opportunity of seeing quackery brought to such tests as will reveal its true character. Its system of deceptions ordinarily suffices to keep the public in ignorance of its falseness and its folly. But now and then the true tests are applied, and quackery is drawn out from its concealment, and its false pretensions and dishonest arts are exposed to the light of day. The coroner's inquest contained in the pamphlet before us has done this for that ridiculous but genteel form of quackery commonly called homœopathy. So far as one single case can do it, the case here reported exhibits in the clearest manner the ignorance and folly which belong to homœopathic practitioners of even high repute among its advocates, and the degrading influence which the beliefs and practices of homœopathy exert upon the mental character, and we may say upon the moral character also. We propose, therefore, to develop to our readers in this article, the character and tendencies of homœopathy, as they are exhibited to us in this case.

The facts of the case are briefly as follows: Agnes E. Lottimer, aged 12 years, was taken sick on the 2d of August. She was attended by Dr. Wells, a homœopathic doctor in Brooklyn, N. Y., with Dr. Dunham, another homœopathic doctor, in consultation occasionally. From the account which Dr. Wells gives of the case, it appears that the disease was intermittent fever. Both the physicians and the quacks that testified in the case agree on this point. The only difference of opinion was as to its accompaniments and complications. These will be spoken of in the course of our remarks.

Although the disease lasted so long (over two months), the homœopathic doctors assured the anxious parents and friends that the patient was doing well, because, as they testified, the paroxysms were becoming shorter and less severe. To convince the anxious father that they were right, Dr. Dunham read to him from Kunstadt (we suppose some voluminous driveller in homœopathic nonsense), thinking that he must, of course, be satisfied with what is put down in one of their books. But this was not satisfactory, for the parents had just lost a son that had intermittent fever under the homœopathic treatment of Dr. Wells, and a beloved daughter was now suffering under the same disease, which a continuance of homœopathic medicines for weeks had not sufficed to arrest. The parents were, of course, exceedingly anxious about

the result. Mrs. Lottimer says, "I nearly daily, during my child's illness, expressed strong anxiety about her. I said I thought my child was not getting better, and that they let the chills last too long. This was both to Dr. Wells and Dr. Dunham. They said they would soon break them, and they expected every day they would slip by; that I need not feel uneasy, Agnes would get well." Her anxiety at one time, about three weeks before the death of the child, was so great that she sent for Dr. Dunham for the purpose of expressing her feelings to him, because she thought that she saw alarm in Dr. Wells's countenance, though he said nothing. But he told her that there was no cause for solicitude. It seems that Dr. Wells put his ear to the chest twice during this long illness, probably suspecting from the breathing that there might be difficulty there. The second time that he did this, Mrs. Lottimer says that she asked Dr. Dunham "if there was any congestion about Agnes's heart or lungs, or any other vital part; and he said I was nervous and unnecessarily alarmed; that there was nothing of the kind; she was all right. *He did not make any examination whatever previous to answering me.*" Propositions were made to call in some other physicians, but Drs. Wells and Dunham told the parents and friends that this was wholly unnecessary. At one time the parents and Mrs. Lottimer's brother decided to call in another physician, and fixed upon Dr. Gray. It was proposed to Dr. Dunham, but he had the old story, that there was no need of further advice—"she is all right." And he added, that if further counsel were needed, he certainly would not select Dr. Gray, because he did not practice *this system* (how many systems are there?) of homœopathy.

And so, although the disease was going on week after week, it was *all right, all right*, till at length the patient was suddenly worse. The congestion which the anxious mother suspected, but which Dr. Dunham, without any examination, assured her did not exist, was suddenly largely increased, convulsions and hemorrhage ensued, and in a few hours the patient died.

Of course, the death must be accounted for in such a way as to satisfy the friends of the patient, and save the reputation of the quacks who had treated the case, saying all right, all right, up to the very last day of her life. The common, we may say the old, trick of quackery was resorted to for this purpose. The death was attributed not at all to the disease treated, but to another disease that "set in," as it is vulgarly expressed. This disease was the mumps, which, according to the homœopathic doctors, destroyed life in this way. It was unfortunately *translated to the brain*, and so produced the convulsions; and then, either the convulsions caused a congestion of the lungs, or the mumps experienced a second translation—from the brain to these organs.

These, with various other attendant explanations, would probably have answered the purpose very well, if they had merely been talked over in the usual indefinite way by the homœopathic doctors and their believers. Now and then, it is true, they would have come to the ears of some "allopathic" doctors, as these homœopaths are pleased to style rational physicians, who would be so uncanid as to pick some flaws in the anatomy and pathology exhibited in these explanations. But after all, the quacks (for by no other name can we properly designate them) would have had it very much their own way, and the Lottimer family, and all the believers in homœopathy, would probably have been satisfied with the seemingly learned and plausible views of the case presented by Drs. Wells and Dunham. Unfortunately for them, however, the case was not to be disposed of in this quiet way. Their plausibilities were not to be left to go unexamined. An examination after

death was made by several physicians. And then a coroner's inquest was held, and all the facts of the case, together with the explanations of the homœopaths, were fully brought out. Not only were Drs. Wells and Dunham thoroughly exposed, but the false and nonsensical character of homœopathy, entitling it to the appellation of a system of quackery, was exposed also.

In order to prevent this exposure from having its legitimate effect on the community, a garbled report was prepared by a homœopathist, and was published, which is just such an expedient as we should expect from such a source. To say nothing of other omissions and misrepresentations in this report, a monstrous blunder in pathology committed by Dr. Wells, in accounting for the hemorrhage in the case, was *entirely omitted*, although it was really a prominent point in Dr. Wells's testimony, as the reader will soon see.

In the pamphlet before us, we have a full report of the testimony in the case, without note or comment, as it was taken down at the time. There are some parts of this testimony which we propose now to examine.

Dr. Wells's testimony is very full. He utters his nonsense about the pathology of the case and its treatment with a grave and learned air. He comments *in extenso* upon the effects of the remedies that he used, which were many, viz: *aconite, belladonna, rhus toxicodendron, pulsatilla, sulphur, arsenicum, carbo vegetalis, veratrum album, ipecacuanha, nux vomica, cinchona, calcarea carbonica, cedron, opium, cicuta*. All these he gave in infinitesimal doses, and besides, he gave coffee and lemon-juice in "allopathic" doses; why, he does not inform us. If the jury were enlightened at all by his comments on these remedies, they were more fortunate than we have been. There is a *show* of wisdom in them, which was intended to have its effect on the jury, and probably had. We suppose that they really were impressed with the great and discriminating knowledge of Dr. Parkhurst P. Wells in the *Materia Medica Pura* of Hahnemann. We give an extract from this part of the testimony.

"The ague of *veratrum* is made up more of chill than fever, the chill also accompanied by a cold perspiration—this she got after the night of the 10th. The ague of arsenic has so many varieties that I could not repeat them without getting the book and reading. *Ipecac* produces vomiting, perspiration, cramps in the stomach, &c. Ague and fever without thirst, *ipecac* will very likely cure. Probably at that time her fever and ague was unaccompanied with thirst. I judged it to be the very best thing she could take when I gave it, because the symptoms she had corresponded with the symptoms the *ipecac* would produce. I do not remember them now. All the symptoms produced by *ipecac* are not found in the same person; they are modified by age, temperament, sex, &c.

Nux Vomica. Thirst from the beginning of the chill, continuing through the chill, also through the fever, pains in the head through both chill and fever; it is also applicable to paroxysms occurring any time within the twenty-four hours; thirst ceases at the cessation of the fever. It is probable that this was the form of fever at the time this was prescribed; this was given in the one-third centesimal, and in the same form, every two hours, repeated during the intermission.

Pulsatilla produces fever, leucorrhœa, amenorrhœa, dysmenorrhœa, nausea and vomiting, diarrhœa, &c.

I do not remember exactly why I gave this, further than the general statement already given. The same dose as the others.

Cinchona, the same dose. I do not remember the form of ague in this particular case.

Calcareo Carbonica was given in the same dose, and for some other form of the fever which I do not recollect.

Cedron is a kind of bean that grows in tropical regions; the properties are not proved, or fully understood. I took it myself in health, and got chills affecting the left side of the body, and in the afternoon, pains in the stomach and in the forehead and over the eyes; darting pains in the right arm first, and in the left the second, along the ulnar, and also in the right hand. I do not remember the particular state of the patient for which this was given; her paroxysms were very much mitigated by its effects, so much so that I expected every day she would pass her chill. She continued its use perhaps ten days; nothing else given at the same time."

The reader will observe some strange failures of memory here. Although Dr. Wells gave *cedron*, for example, for ten days, he does "not remember the particular state of the patient for which this was given." Neither does he remember why he gave *ipecacuanha*, *pulsatilla*, *cinchona*, and *calcareo carbonica*. And he is entirely in the fog about *arsenic*, or *arsenicum*, as all the lady-homœopathists are careful to call it. He informs the jury that he could not enlighten them about this "without getting the book and reading." If he had been requested to do this, we should like to have looked at that jury while he read to them the description of the effects of arsenic from that standard work, *Jahr's Manual*, filling over four large closely printed pages, with such ridiculous minutiae as these: "Muddled and heavy head in the house, relieved in the open air. Headache alleviated by putting on cold water, but aggravated when removing it. Religious pensiveness; fear of spectres and thieves; warts; attacks of rheumatic pains continuing six days, with intervals of four days. When lying, one feels strong and wishes to get up, but upon doing so sinks together. Desire for acids, or whiskey, or coffee. Great anxiety in the evening after lying down, or at 3 o'clock in the morning when awaking," &c. &c. But we pass on to graver matters.

Dr. Wells's idea of the case was in the beginning that it was inflammation of the membranes of the brain. But in about eight days under his treatment she was cured, or, as he would say, *healed* of this. Then came on suddenly a state of things thus described: "On the night of the 10th of August, about 10 o'clock, I was called by a messenger, who said she was much worse, and they thought she must die. I found the surface cold, pale, and covered with perspiration. Pulse 160 in a minute, small and weak; great thirst, restlessness and anxiety, and much tossing about the bed, and anxious respiration. She was unconscious." He does not explain at all this state of things, but he says, that for ten days after this, the case "was ague and fever without intermission; there was not sufficient vital power to bring about an intermission. After that ten days it became a regular intermittent, and so continued uncomplicated up to three days before her death, and then continued up to her death *complicated with mumps, which was translated to the membranes of the brain.*" This translation of the mumps, he supposes, caused a "congestion of the brain," and this produced spasms. Then, to crown all, there was another metastasis of the mumps, it going this time to the lungs, and death was the result.

Before commenting on these and other opinions of Dr. Wells, let us look at the result of the *post-mortem* examination. It was made by Dr. James R. Wood, of New York, in presence of Professors Willard Parker, and Chandler R. Gilman, of New York, and Dr. Dudley, of Brooklyn. The account of it, as given by Dr. Wood, is as follows:—

"Upon an external view of the body there was nothing peculiar, excepting an extraordinary quantity of adipose matter or fat. The skin was of a dirty

white hue. The eye only peculiar from its life-like appearance. Its membranes appeared natural. The eye was normal. I made a section of the scalp; found nothing peculiar, except that not a drop of blood followed my scalpel. On removing the calvarium, the external membrane of the brain, the dura mater was pale and healthy. Upon removing the superior portion of this membrane, the second or arachnoid membrane was brought into view; it was opaque, and thickened at points in the course of the longitudinal sinus; beneath it was an effusion of serum, to the physician known as subarachnoid effusion. This membrane was healthy in other parts of the brain. A third or vascular membrane, the pia mater, was thickened, congested, and could be removed in considerable portions by slight traction with the forceps in the neighbourhood of the opaque and thickened arachnoid. This tissue in other regions of the brain was healthy. Upon making a section of each hemisphere of the brain, the parts were found healthy. Upon cutting into the ventricles, the organs there contained were healthy. There was a small quantity of serum in each ventricle, which, in my opinion, was post mortem; other parts of the cerebrum healthy. The cerebellum was healthy. Upon dividing the medulla spinalis, and removing the brain, there was discovered a considerable quantity of serum in the spinal canal, also a considerable quantity in the fossa of the occipital bone. This was placed in a tumbler, and estimated to be about three ounces in quantity. On making a section of the integuments from the upper portion of the sternum to the pubis, nothing peculiar, except the enormous quantity of fat, by measurement from the integuments to the tendon of the external abdominalis muscle, one and an eighth inch. On raising the sternum, and looking into the cavity of the left pleura, a considerable quantity of bloody serum was discovered; it was removed and estimated at from three to four ounces in quantity. In the right pleura there was also bloody serum, estimated at about or little more than an ounce. The pleura covering the lungs, and that lining the thorax, intensely congested; the lung barely crepitating throughout. An old adhesion between the middle and inferior lobe of the right lung. Upon examining the heart and its envelops, they were found healthy; very little or no blood in the heart; a small quantity of serum in the pericardium. Upon removing the lungs from the cavity of the thorax, and making an incision at different points, they were found to be intensely congested with blood. So heavy were they, that they would barely float in water. Upon examining or laying open the large bronchial tubes, they were found empty, with their lining, or mucous membrane, intensely congested, of the colour of mahogany. In tracing down the smaller tubes, they were also congested, but contained bloody serum. In other respects, the thorax healthy. Upon opening the abdomen, parts of the viscera, which presented themselves to view, presented no other peculiarity except that of being almost bloodless, with the exception of the liver, which appeared much darker than natural. This organ, upon being cut into, was found to be almost bloodless. It was healthy, and its appendages, including the gall-bladder, also healthy. The spleen was found to be many times its natural size by measurement; eight inches the longest diameter, and four the transverse; by weight, one pound; very little blood, if any, followed the scalpel. In making a section of it, its colour was the colour of the ordinary blue plum. Requested by the coroner, I applied ligatures to the two orifices of the stomach, which was taken by him for chemical analysis. The kidneys examined externally, the colour very much like that of the spleen. Upon making a section, they were found to be very much congested, and presented to view a pathological condition of this organ known as Bright's disease, or granular kidney, in its first stage. The two weighed eight ounces. The pancreas, large and small intestines, the bladder, and other organs of the abdominal and pelvic cavity, were examined and found healthy."

The following are some of the opinions which Dr. Wood gives upon these appearances.

"It was my opinion that the opaque and thickened condition of the arachnoid membrane was the result of inflammation, and that inflammation not of a

recent date—perhaps of some weeks' standing. I also account for the pathological change of the pia mater in the same way. The serum was the result of prior disease; much of it from the congested condition of the vessels of the pia mater. Some of it was post mortem.

"The lungs were congested. From the evidence of Dr. Wells, there can be no doubt that this patient was suffering from the poison of marsh miasma. From the post mortem appearances, it is my opinion that they were produced by the effects of that poison on the system. The enormous size of the spleen, the colour of the liver, the intense congestion of the lungs, and the history of the case, warrant me in giving this as my opinion.

"The blood thrown up undoubtedly came from the lungs. It was in this instance, in my opinion, an effort of nature to relieve the lungs of the superabundance of blood which they contained. From experience we know that when large quantities of blood are thrown into the lungs, and that they are not able to rid themselves of it, the respiration of course is interfered with; the patient dies either of congestion of the lungs, or of what is called pulmonary apoplexy. Here, as I have said before, I believe that the hemorrhage was an effort of nature to rid the lungs of the blood they contained, and that the patient died of the loss of blood."

These opinions were coincided in by the physicians who attended the examination, and by Professors Alonzo Clark and J. M. Smith, and Dr. John Watson, who gave their opinion upon the testimony as they heard it.

It will be seen that upon one point in the case there is an agreement between Dr. Wood and Dr. Wells, viz: the existence of inflammation of the membranes of the brain at the beginning of the sickness. We have but a single remark to make in regard to this. If this inflammation had been met with the proper remedies, instead of being left to the recuperative efforts of nature alone, the probability is, that the inflammation would have been wholly overcome, and no such results would have been left in the brain as were found in the post-mortem examination. Giving the infinitesimals of homœopathy in such a state of things is not merely ridiculous trifling; it is a trifling with life, which cannot but stir one with indignation who knows how utterly without effect such doses are. As it was, a chronic disease was left in the brain, which undoubtedly had a share in bringing about the fatal result. Such a chronic disease, although generally manifesting itself more or less by characteristic symptoms, physicians well know sometimes exists without exhibiting these symptoms with any clearness. And yet we cannot help suspecting that Dr. Wells, who shows in his testimony such looseness of views, and such random, and fairly nonsensical reasoning, really failed to see symptoms that ought to have revealed to him this difficulty. But this we will not dwell upon. There are other points in the case in which he more palpably failed than in this. Some of these relate to the treatment, and some to the pathology of the case. A few of these we will notice.

Here is a case, which, according to Dr. Wells' own admission, was uncomplicated intermittent fever for over six weeks, and which was probably essentially this disease from the beginning of the case, that is, for over two months. And yet all this time he administered his high dilutions except for two days. And then he directed "a cup of black coffee and a tablespoonful of lemon-juice." A strange overturn this in his practice! Why he did not give these remedies in infinitesimal doses he is not pleased to inform the jury. We may show an ignorance at which Dr. Wells and his learned compeers may laugh, but really we cannot see why coffee and lemon-juice should be given in doses millions upon millions of times larger than other remedies. According to Dr. Wells' common mode of dosing, here was coffee and lemon-juice enough to suffice for all the cases of intermittent fever that have been

or will be in all time. And yet it must all be poured down the throat of this little girl twelve years old at one time!

But ridiculous and supremely trifling as is this infinitesimal dosing with the interlude of large dosing, it is not the worst of the case. No fact is better established in medicine, by an abundant and long-continued experience, than that *intermittent fever may in almost all cases be arrested by medicine both speedily and safely*. And yet, in face of this established fact, Dr. Wells let this little patient go on suffering week after week from the congestion produced by the paroxysms, till at length the fatal congestion was fastened upon the lungs. Whatever other causes may have helped to insure the fatal result, this congestion thus produced by the intermittent fever was clearly the chief cause of death.

Homœopaths admit that cinchona, or its active principle, quinine, is capable of *arresting* the paroxysms; but they claim that it does not cure or *heal* (their favorite word) the disease. The malady, they say, reappears in the same or another form. And they pretend that the homœopathic remedies do actually *heal* the disease—that they *eradicate* it wholly from the system by a gradual process, so that it cannot reappear. The truth is, that in all those cases in which recovery takes place under their system of high dilutions the disease simply wears out. That this will occur in some, perhaps we may say a considerable, proportion of cases, is well known from what is observed in some communities, where there is much prejudice against taking anything to arrest the disease. But in how large a proportion of cases this recovery by the recuperative energies of nature will take place has not been ascertained. We do not care to ascertain it, so long as we have a remedy which so safely arrests it, and saves our patients from the suffering and the risk that attend a continuance of the disease.

In the practice of homœopaths there is often much cheating in the treatment of this disease. Many cases are actually *healed* by quinine, and yet are reported to be cured by homœopathy. A patient who had not been inducted into the mysteries of homœopathic management, and who knew little of homœopathy but its name, was boasting of being cured of intermittent fever by this system of practice. On being questioned as to what medicines he took, he said that he did not know, but the powders which the doctor gave him were awfully bitter. Such cases of relinquishment of true homœopathy are, we apprehend, by no means uncommon. Indeed, some homœopathic doctors use quinine in large doses uniformly in the treatment of intermittent fever. This is the case with Drs. Gray and Hull, two homœopaths of some celebrity in New York, who testified in regard to this case. Dr. Gray says: "My dose of quinine varies from an eighth of a grain to *two grains*." Dr. Hull says: "I vary my dose from the $\frac{1}{100}$ th of a grain to a grain and a half or *two grains*." We do not admire the honesty of such a course. To call themselves homœopathic physicians, and yet give such doses, is certainly setting up false pretensions.

In the case before us, there was surely time enough given to the trial of the boasted eradicating process; and as it did not succeed, common prudence should have dictated a resort to a process in which the great body of the medical profession, however variant their views on other points, have had confidence more than two hundred years. It seems that the failure of the eradicating process did produce in Dr. Wells' mind a willingness to employ some other measure. Why, then, did he resort to the comparatively doubtful remedies, black coffee and lemon-juice, when the sure remedy, quinine, was at hand? The most charitable construction we can put upon this, is, that he

was so strongly wedded to his exclusive notions that he would not consent to adopt a remedy so highly approved of by the hated "allopaths." It was slavery to a system that forbade his using the quinine—a system that has but one doctrine, and ignores all facts that militate against this sole law, though they may be established by the experience of ages.

It is in the point just referred to that rational medicine and homœopathy are at antipodes to each other. Rational medicine has no one law of cure. It has many. And not only so, but it is not confined to these many laws, but goes beyond them, and adopts remedies that it cannot bring under any general law of cure. It is ready to adopt anything which is curative, whether it can explain its *modus operandi* or not. The mode in which quinine cures intermittent fever it cannot explain, and therefore cannot classify it under any law of cure. It knows the bare fact that it will cure, and so adopts it; and it is ready to adopt anything, even infinitesimal dosing, if it only be *proved* to cure. It has no exclusive system of doctrine or practice, as the term allopathy, so gratuitously and falsely bestowed upon it by the homœopaths, would imply. But it is otherwise with homœopathy. This rejects everything that manifestly disagrees with its "sole law of cure." It is for this reason that it rejects quinine in the treatment of intermittent fever, although the proof that it cures this disease is beyond all question.

In looking at the symptoms of the case before us, its treatment, and the revelations made to us in the post-mortem examination, we are fully warranted in saying that if the appropriate means had been used, this patient would probably have recovered. If the proper means had been used at the outset to overcome the inflammation of the brain, probably no chronic disease would have been left there to complicate the intermittent fever. And then, if the quinine had been used, it would have readily overcome the malady thus relieved of its complication, and the little patient would have been saved from the fatal congestion, which this long continued endurance of the paroxysms at length produced.

But it is claimed by Dr. Wells that the intermittent fever did not cause death, and that this result was produced by the mumps. This disease all the homœopaths testified was translated to the brain. Dr. Wells goes a little further than his brethren, and supposes a second translation, to the lungs. This he supposes to be the cause of the congestion there. For he says: "The congestion of the lungs might have arisen from one of two causes; first, the mechanical effect of the embarrassed respiration (I attach no particular importance to this, the probability is the other way); and the other, metastasis of the mumps from the brain to the lungs." His brethren seem to lay great stress upon this "mechanical effect of the embarrassed respiration" produced by the convulsions as the great cause of the congestion; and they speak of it as if convulsions ordinarily produce congestion in those organs. And yet all experience is against such an assumption. There may be convulsions long continued, and yet, if the patient recover, there is ordinarily no congestion of the lungs as a result; or if he die, there is ordinarily none found there. If it occur, it is the result of some other cause, and not the consequence of the convulsions. The homœopaths have put the consequent in place of the antecedent, the effect in place of the cause, as they are very wont to do. The congestion was undoubtedly the cause and not the result of the convulsions, the chronic disease in the brain helping of course to produce the result. This is a common sense rational conclusion drawn from the history of the case in view of the pathological experience of the profession. But the common

sense rational view is not the view to be taken of anything by the dreaming and exclusive homœopathists.

There is another point in the homœopathic pathology of the case that deserves a passing notice. There was blood discharged from the mouth and nose of the patient just before death in quite a large quantity, so large that she was supposed to die from suffocation by the accumulation of the blood in the throat. The homœopathic doctors speak of this as resulting from a rupture of some bloodvessel caused by the convulsions. They speak of it as if rupture were common in hemorrhage. They do not seem to be aware of the well established fact, that in hemorrhage from the lungs the blood is poured out by innumerable little vessels on the surface of the mucous membrane, and that very seldom does any rupture of a bloodvessel occur in those organs. An ignorance of this fact is pardonable in non-medical men. They may talk of vessels being ruptured or "uncapped," in bleeding from the lungs; but for one who professes to be a physician to talk thus betokens a ridiculous and unpardonable ignorance. But this, perhaps, is to be expected of a sect that discard pathology as being useless.

Rupture does often occur in the brain, and Dr. Wells treats us to a somewhat remarkable supposition in regard to hemorrhage in that organ. He says, that "*the hemorrhage might have come from rupture of the longitudinal sinus, and coming through the ethmoid plate; and it is only conjecture that it came from the lungs.*" His idea is, that the sinus having been ruptured, the blood came through the little openings in the cribriform plate of this bone into the nose, just as water or any fluid would run through a sieve. This was probably a clear idea to the jury, though it be an idea of an impossibility, or rather of a series of impossibilities.

Let us see how much this pathological supposition of the learned and ingenious Dr. Wells implies. First, there must be a rupture of the longitudinal sinus. This can only result from two causes, mechanical violence and ulceration. As to the first, nothing short of a breaking in of the skull could do it; the pressure of blood induced by convulsions never could. As to ulceration of the sinus, if it ever do occur, it must be exceedingly rare. And if it should occur, the blood would be diffused in every direction over the surface of the brain, unless indeed the march of the ulceration should be towards the ethmoid bone. And then, in order to have the blood discharge from the nose, there must be actual ulceration of the bone itself, and of the membrane in the nose that covers it.

Dr. Wells evidently contemplates no such series of events as this in his supposition, for he says: "I see no difficulty of a certain amount of pressure forcing the blood through the openings in that bone." He forgets, if he ever knew, that these openings are wholly closed by the branches of the olfactory nerves and their accompaniments, and are covered tightly by the membrane lining the nasal cavities; so that no blood can come through them till all these soft parts be removed, for the same reason precisely that a fluid cannot be poured from a bottle till the cork be drawn.

Dr. Wells heaps blunder upon blunder. He states that "softening of the brain" is necessary for this discharge of blood through the ethmoid plate. How this can have any connection with such a result we leave it to homœopathic pathology to show. One might as well talk of blood being discharged from the lungs through the skin that covers the chest, and of a softening of the lungs as being necessary to its discharge in this way. Well does Dr. Gilman remark: "I cannot conceive of the hemorrhage coming from the brain in the manner spoken of any more than I can conceive its coming from the

brain to the ends of the fingers." These homœopaths are familiar with conceptions of impossibilities. They are to be found here and there in their standard works. Jahr states in his *Manual*, for example, as one of the effects of stramonium, that "air passes out of the ears." How this can be, unless when the drum of the ear is deficient or absent, we of course cannot understand, but we suppose it is plain enough to one who can conceive of blood being forced through the ethmoid bone.

But it would seem that Dr. Wells had, on reflection, some idea of the absurdity of his supposition, for in testifying afterward, on being recalled, he says of it: "This was not intended to explain the hemorrhage in this case, but only as a possible occurrence. *My opinion now is that the hemorrhage comes from the lungs,*" an opinion in direct opposition to that which he gave when he testified first, for he then laid great stress upon his idea of a flow of the blood through the ethmoid bone, and said that "it is only *conjecture* that it came from the lungs." The homœopathic reporter, it seems, had so lively an idea of the ridiculousness of Dr. Wells' supposition, that not the most distant allusion to it is given in his report of the doctor's testimony.

Though Dr. Wells was so ready with his supposition in regard to the hemorrhage, he seems unaccountably reluctant to explain another phenomenon in the case, the appearance of bloody serum in the chest. He says on this point:—

"I decline accounting for the appearance of this bloody serum in the chest, on account of the uncertainty which attends its origin. Whether it took place before death or after, the origin of the serum is all uncertain. I have made post-mortem examinations of persons who have died from diseases of the lungs, and have found serum in the cavity of the chest. I have seen it of all amounts, from as much as the chest was capable of holding down to a very little. Some died of pleurisy, some of phthisis. I have opened persons who have died of other diseases, such as dropsy of the brain, jaundice, with considerable serum in both pleural cavities, and no disease of lungs or pleura. I am not aware that I ever saw bloody serum in any case that I remember."

Now what mystery there can be about the "origin" of this bloody serum we cannot see. Where else could it come from but the pleura? It being a close sac, there is no way for the blood to get into it but by exhalation with the serum from this membrane. The membrane itself and the lungs which it covered were highly congested, as appeared from the post-mortem examination, and with this congestion there is no mystery about the origin of the bloody serum. The merest tyro in medicine would be at no loss on this point. And yet the erudite Dr. Wells, who could suppose a hemorrhage through the cribriform plate, all at once becomes cautious, and declines accounting for a phenomenon, the cause of which is palpable. We do not wonder that he prefaced such a declinature with the remark, that "post-mortem examinations are of much less value than is commonly supposed." He certainly had not profited much by them.

We come now to look at the testimony about the mumps, which the homœopathic doctors all considered as the undoubted cause of the death in the case. Dr. Wells remarks thus on this point:—

"The reason why I supposed she had mumps, and they were translated to the membranes of the brain, is because, 1st, for the two or three weeks preceding her death she had no brain symptoms whatever, not even a pain in her head, and I questioned her every day on that point; 2d, the attack came on just at the time mumps will come if she had taken it from her mother; 3d, the slight stiffness and tenderness of the neck, as mentioned; 4th, the character of the convulsions; 5th, the termination by hemorrhage. *The hemorrhage might have come from rupture of the longitudinal sinus, and coming through the ethmoid plate; and it is only conjecture that it came from the lungs.*"

Of course, if the hemorrhage came from the lungs, as he afterwards said was his opinion, he would dismiss his fifth reason. The force of the other four reasons which he gives so systematically, will appear to the reader as we proceed.

The translation of mumps to the brain, which the physicians who testified deemed to be very rare, none of them ever having seen it, the quacks talked about as if it were a common thing. Dr. Dunham said he had seen a case which terminated just as the case of Agnes Lottimer did, by hemorrhage from the nose and mouth. Dr. Hull had a remarkable case of metastasis of this disease in his own family, which he relates in his testimony. He says: "In the case of a child in my own family, the mumps apparently commenced in the bowels, and the application of a poultice of *angle-worms* to the parotid gland caused a metastasis of the disease from the bowels to the parotid. The brain was somewhat affected, but not a complete metastasis. The child recovered." We suppose that the metastasis to the brain would have been complete if the angle-worm poultice had not drawn the mumps right to the parotid gland when it left the bowels. How the poultice acted in consonance with the great Hahnemannian law of cure, we are at a loss to understand. The explanation of this could probably be achieved only by one who knows how blood can get through the cribriform plate.

Though the homœopathists all agreed that the mumps killed the patient, we fail to see, and the physicians who testified failed to see, that there was any evidence of the existence of mumps in the case. It was supposed that the patient took the mumps from its mother. But the description which the mother gave of her own case is certainly not a description of mumps, but of common inflammation of the jaw, or *ague* in the face, as it is commonly called. It is as follows:—

"On the 14th of September, about 11 o'clock at night, I sent for Dr. Dunham, with my tooth paining; it seemed to have a great deal of inflammation; I had leeches on the gum in the afternoon, and had applied hops and vinegar to my face about eight o'clock in the evening; my gum and the side of my cheek were very much swollen. It pained me all underneath my jaw-bone, more the jaw-bone than the glands; the swelling was more under the jaw-bone; the glands underneath the ear seemed *slightly*, but not much, swollen. It was a day or two before my child died that I got entirely free from all pain and soreness about my face and neck; I never got entirely well of the swelling and pain in my face, and then the swelling commenced under the ear; it was all together. My sister, Mrs. Large, told me that, when a child, I had mumps on one side. My sister could not recollect, and I do not know, on which side of my neck I had mumps. About the third day after I complained of this tooth, Dr. Wells told me I had mumps. The next morning Dr. Wells prescribed for my face, and attended me for it constantly. My face was still swelled, and Dr. Wells told me there was matter on the tooth; at the time he told me I had mumps. I am certain this was all together, and not two different attacks. I have frequently before had trouble about my teeth and gums, with swelled face frequently; at the times before, my jaws have been stiff and a great deal of swelling of the face, though not so much as this time. I have before had to call in medical advice; Dr. Wells several times attended me with it."

From this account it appears that the first sign of trouble was in or about a tooth. Dr. Hull, we suppose, would say that in this case the mumps began in the tooth, just as in the case of his child it began in the bowels. Perhaps the reason that it did not get more fully into the parotid gland was, that the true homœopathic remedy, an angle-worm poultice, was not applied. It was not till the third day of her suffering that Dr. Wells told Mrs. Lottimer that she had mumps. Matter was soon found, as he told her, and yet he still said

the case was mumps. This positive opinion thus given, certainly indicates a blundering looseness in opinionating. The utmost that could be claimed in endeavouring to make out a case of mumps would be, that possibly this disease might be present at the same time with the inflammation of the jaw. And the fact that she had once had the mumps is fatal to this supposition.

The evidence that the child had the mumps is even less than that the mother had it. Dr. Wells' testimony about this is as follows: "The third day before her death, she complained that she did not feel as well; complaining of nothing specific, however, except a slight stiffness of one side of the neck, with slight tenderness on pressure over the upper and anterior part or edge of the cleido-mastoid muscle. The domestic attendants thought she was getting mumps, as her mother had mumps about one week before. There was no swelling of the parotid gland." I will simply quote in regard to this a remark of Dr. Gilman's in his testimony: "Here are mumps spoken of without any swelling or tenderness of the parotid gland. Now I take it, this is like speaking of smallpox without any eruption."

But it was this strange concealed kind of mumps that went to the brain and destroyed life! And what is most wonderful of all is, that the disease appeared one day in the shape of pain and soreness of the neck, the next day disappeared altogether, and on the third reappeared in the brain! For the order of events and of the doctor's opinionating is this, as appears from Mrs. Lottimer's testimony. She says:—

"On the Thursday previous to her death, Agnes first complained of pain and soreness in the neck, and on that day I called Dr. Wells' attention to it; he did not examine her neck on Thursday; I did two or three times; he did not account for the stiffness, &c. On Friday I next called his attention to it; he then examined the neck; he said there was no appearance of mumps now at all. On Saturday, about 12 o'clock, Agnes was taken with convulsions. The time Agnes was in a convulsion, he said he thought that the mumps had flown to the brain."

Where the mumps were on Friday, when the doctor said there was now no appearance of mumps, we are not told. The author of the ingenious supposition of hemorrhage through the ethmoid bone probably supposed that the mumps were secretly hatching a disease which appeared in convulsions the next day.

The verdict of the jury in this case was of a non-committal character. In regard to this the following remarks are made in the preface to the report of the testimony:—

"With their 'finding' we have no concern. The public health demanded of the coroner the investigation, and for the fearless performance of his duty he deserves the thanks of the community. Homœopathic physicians of the vicinity have chosen to stigmatize the proceedings as persecution. If we are to believe them and their advocates, this inquest was something strange, and till now unheard of. The patient, say they, was attended by a physician, and he prescribed 'according to his system; surely, he is not to be held accountable.' How has it been in similar cases? It is not very many years ago that a young man named French was attended by a Dr. Frost, who '*prescribed for him according to his system.*' French died, and not only did the coroner hold an inquest, but the grand jury found a bill; Frost was tried, and, we believe, convicted of manslaughter! As in the case of Wells, physicians (so called) from all quarters gave their testimony that the treatment had been judicious '*according to their system.*' Yet Frost was convicted. To be sure, Frost's system was Thompsonianism; but is that worse than homœopathy? In the one system we have Thompson, Lobelia, and Number 7; in the other, Hahnemann, Belladonna, and the Thirtieth Dilution. Who shall decide between the Dutch

and the Yankee quack? Who will venture *tantas componere lites*? One credit we will allow these Homœopathists; if their knowledge of anatomy is as infinitesimal as their doses, their *esprit de corps* is as high as their dilutions. During the trial they made common cause—high dilutionists and low dilutionists united to assure the jury that the treatment was ‘judicious;’ though it will be seen that they by no means agree that they would have treated it in the same way.”

We felt some reluctance to notice the pamphlet before us on account of the feelings of the family and the friends of the patient. The loss to parents of a loved child is an affliction sufficiently hard to be borne, without having added to it the reflection, that their child was a victim of outrageous quackery in the shape of a vaunted system. We would like to relieve their hearts from such a festering wound as this reflection must create. It gives us pain to open this wound afresh; but duty has required of us, as public journalists, the exposure we have made of the gross quackery that produced the fatal result in this case. The exposure is painful to the afflicted family, but it may save by its warning many other families from experiencing a similar affliction.

Before dismissing this case, we have a few remarks to make upon some of the mental and moral developments of homœopathy, brought out in the testimony.

There is a striking contrast to be observed between the character of the testimony of the physicians in this case, and that of the testimony of the homœopathic quacks. The testimony of the former is straightforward, clear, common sense testimony, without any useless or irrelevant matter; while that of the quacks is confused, inconsistent, full of assumptions and blunders, and cumbered with a false display of learning and of minute observation in regard to the effects of remedies. The physicians who testified are evidently men that are accustomed to definite thinking and to accurate examination of evidence. The quacks who testified, are, on the other hand, men who are accustomed to talk loosely and think loosely, although they make large pretensions to great exactness in their observations of disease, and of the effects of remedies.

It is this loose habit of thinking which appears as a very prominent characteristic of the practitioners of homœopathy. That this is a legitimate result of their system, any one can see who will take the trouble to look into any of their standard works, especially the larger ones, like *Jahr's Manual*, for example. The descriptions given of the effects of remedies are the most random and confused collections of everything that can be conjured up by the most exuberant fancy, as the reader may see from the extract from *Jahr's Manual*, given in another part of this article. There are over four hundred closely printed pages of such discrepant nonsense in this book; and this is but one among many such books. Those who conjured up these collections must have resigned all definiteness of thought, and those who take them as authority must do the same.

Adherence to any exclusive system has a tendency to obscure as well as to narrow the mind. It does so because the mind acquires the habit of slurring over all facts that are inconsistent with its favourite system. This is especially true of homœopathy, not only because it is an exceedingly exclusive system, having but one law of cure; but also, because it has gathered such a mass of loose and bewildering details, mixtures of entities and nonentities, chiefly the latter.

There is, from these causes, a manifest lack of straightforward honesty of mind in homœopathists as a class. They are habitually familiar with plausibilities and glosses and misrepresentations; and the transition from this to

wilful misrepresentation is, to most men, an easy one. Even one who goes into homœopathy, from strictly honest conviction of its truth (which we believe to be exceedingly rare), experiences this corrupting influence upon his mind and heart. He is subjected to the temptation to be dishonest continually. No homœopathic doctor can get along with his patients and the public without making constant misrepresentations. He acquires a loose habit in this respect necessarily. A giving up of the habit would involve a giving up of homœopathy. No homœopathist can, in the strictest sense of the word, be honest. Many of them are dishonest in the full sense of the word. They are wilful deceivers. Our experience would lead us to say that this is true of the great majority of them.

We have already alluded, in the first part of this article, to a dishonest expedient very commonly resorted to by homœopathists. We refer to their attributing death to some other disease than the one which the patient had in the beginning. This is occasionally the true view of the cause of death; and, because this is so occasionally, they take the liberty of considering it as a common occurrence, for the purpose of deceiving the public in regard to the results of their practice. For example, if an epidemic be prevalent, although the homœopathists may be abundantly unsuccessful, they commonly make out a good story of their success, to be retailed about by their friends, by resorting to the expedient mentioned. We knew a homœopathic doctor to claim that he had lost no cases of dysentery when it was epidemic, although it was well known that he had lost many. How did he make out his case? Why, this one died of typhoid fever that "set in," that of hemorrhage from the bowels, and so on. He even went so far as to insert these as causes of death in his certificates for the registrar. He pursued a course of wilful and bold falsehood. And yet this man has a good standing in the community, and he had a prominent clergyman among the believers and busy retailers of his falsehoods.

One of the most common practices among homœopathists is to cheat their patients with the idea that they are under homœopathic treatment, when they are in reality taking the full doses of "the old practice." If nature will cure while the show of medicine is kept up with the high dilutions, it is very well. The cure, to be sure, is sometimes slow, but the patient is quieted with the idea that the mysteriously powerful pellets are gradually *eradicating* the disease, and then the slowness of the case insures a good long bill. But, if this course does not succeed, the high dilutions are forsaken, and full doses are given, after the manner of the "allopaths." This is done, when it can be, in secrecy; in other words, downright cheating is practised upon the patient. And when, from the bitterness of the medicine, or some other cause, this secrecy cannot be practised, there is nothing said, unless the patient notices the change. If he does, the common excuse is, that the great law of cure applies to all sorts of doses, which is as ridiculous as it would be to say that a large spike would do the same thing as the finest needle, and in the same manner. Indeed, this comparison fails to come up to the ridiculousness of the position of these homœopathists, for the difference between a high dilution and a common dose of "the old practice" is vastly greater than that between the spike and the needle; it is greater even than the difference between an atom and a world. Surely, if both infinitesimal and common doses cure, they do so clearly in different ways, and not by one and the same law of cure.

So common has this administration of ordinary doses become among homœopathists, that it has come to be the prevalent doctrine of this sect of quacks, that Hahnemann's law of cure applies equally to doses of all sizes. It is true,

that some of them resist this as an unholy mixture; but the practice is all the while gaining ground, and homœopathy, like every other form of quackery before it, is fast losing its distinctive characteristics, and will ere long be merged in something else, as new novelties arise in the quackish world.

Among the remedies most commonly used in ordinary doses by homœopaths is quinine for the cure of intermittent fever. Drs. Hull and Gray testified in this case that they used it thus. The truth is, that the proof of the efficacy of quinine in ordinary doses in this disease is so well established, that homœopaths are obliged to use it thus in spite of its being inconsistent with their system. If they refuse to use it, they are in danger of meeting with such disagreeable circumstances as have fallen to the lot of Dr. Wells. The losing of two cases of intermittent fever in one family, and the storm of public reprobation which has fallen upon him in consequence, we think, must cure him of adhering to infinitesimals, unless all this may filter through his mind and conscience as easily as he fancies blood to go through that cribriform plate.

The looseness of ideas, of which we have spoken as being even more characteristic of homœopathic quacks, than of quacks of any other class, is exhibited in this case more by Dr. Wells than the other homœopaths, because his testimony is more full than theirs. It is evident that he has been so much in the habit of talking in a random and confused manner among his patients, that this habit clung to him when testifying under the solemnities of an oath. Confused ideas of symptoms and of remedies, groundless assumptions, and palpable blunders, appear throughout his testimony. His loose habit of thinking was plainly the cause of his blunder about the hemorrhage. A moment's honest, sober reflection would have showed him the ridiculousness of his supposition and the impossibilities that it involved. But the power of sober, honest reflection he had destroyed by the long cultivation of his loose habit of thinking and talking, and so he plunged into this blunder, as he had into many a blunder before it. The same can be said about his testimony in regard to the mumps, showing an utter want of discrimination; and so of other points in this remarkable testimony.

Tried by the common principles by which men try the character of testimony on all other subjects, this testimony of Dr. Wells is wholly unreliable. We can feel no certainty at any time that he is making correct statements. What he says may be true, or may not be true. There is no confidence to be put in a mind that is so befogged. He is not to be credited, for the same reason that any one would not be credited that should talk as loosely and nonsensically on any other subject. Suppose any question of practical science were involved in a legal investigation before a jury; for example, some question in regard to the explosion of steam-engines. If any one should talk as much at random and blunder as egregiously on this subject as Dr. Wells did in regard to anatomy, pathology, and the effects of medicines, his testimony would not be considered as having the weight of a feather. If he should say, for example, that steam could come through the pores of a boiler in great quantities, and that, therefore, a rent of the boiler was not at all necessary to the explosion; he would be no farther out of the way than Dr. Wells was in saying that blood could come through the openings in the ethmoid bone. And if he should say that he declined accounting for the moving of the piston of the engine, he would not show greater ignorance than Dr. Wells did when he said, "I decline accounting for the appearance of this bloody serum in the chest, on account of the uncertainty which attends its origin." To such a witness neither judge nor jury would give any credit. But nonsense in

medicine seems to meet with special tolerance. In the case before us, it would appear by the non-committal character of the verdict, that the jury gave nearly as much weight to the testimony of Dr. Wells, although he made a fool of himself, and to that of his brother quacks, as to the clear and sensible and conclusive testimony of the eminent physicians who testified in the case.

It is time that men of standing and influence in the community should apply the same rules of judging to medicine that they do to other subjects, and should cease to consider sensible and rational physicians as upon a level with the irrational practitioners of a system of quackery. Folly in the science or the practice of medicine is just as bad as it is in any other science or practice, and should be treated with the same contempt; but it is not, commonly, thus treated. A large portion of even the men of standing in the community tolerate, in medicine, folly which they would abominate in anything else. Many clergymen, we regret to say, do not treat the medical profession in this respect as they would wish to be treated themselves. Mormonism is to theology, both in its falseness and its folly, what homœopathy is to rational medicine; and yet a clergyman began a long notice of a book that exposed the fallacies of homœopathy by saying, who shall decide when doctors disagree? If this clergyman should write a book, exposing the follies of Mormonism, it would be quite as proper for us to begin a notice of it with the taunt, who shall decide when divines disagree? Little do clergymen and others think how galling it is to an honorable and rational physician, to be put in the same category with men that utter such blundering nonsense, as appears in the testimony of the homœopaths in the pamphlet before us.

We have thus commented on the testimony in this case, not for the purpose of convincing our readers of the falseness and folly of homœopathy; this would be unnecessary, for they have long ago seen enough of this system of quackery to know what it is. We have made our comments for a different purpose. We wish to supply our readers with the means of convincing non-medical men, as they have opportunity from time to time, that homœopathy is unworthy of their belief and confidence. The facts developed in this article, and in our review of Simpson's *Facts and Tendencies of Homœopathy*, in the January number of last year of this Journal, furnish considerable material for this purpose. Physicians have a duty to perform to the community in the exposure of quackery, which we pointed out at some length in our review of Simpson. We will, therefore, refer our readers to the views there expressed, instead of again dwelling on the subject here.

W. H.

BIBLIOGRAPHICAL NOTICES.

ART. XV.—*Transactions of American Medical Societies:—*

1. *Transactions of the Ninth Annual Meeting of the Ohio State Medical Society, held in the City of Cincinnati, June 6th, 1854, 8vo. p. 172.*
 2. *The Proceedings and Reports of the Medical and Chirurgical Faculty of Maryland, Incorporated in 1799, at its Annual Session, in June, and Special Session in September, 1854, in the City of Baltimore, 8vo. p. 82.*
 3. *The Transactions of the New York Academy of Medicine. Vol. I. Part II. Containing Hospital Hygiene, Illustrated, by JOHN H. GRISCOM, M. D. 8vo. pp. 12.*
- The Transactions of the New York Academy of Medicine. Vol. I. Part III. Containing A Report on Solidified Milk, by the Standing Committee on Public Health and Legal Medicine. 8vo. pp. 12.*

1. THE volume of *Transactions* of the Ohio State Medical Society at its session of 1854, comprises a number of reports and papers of a highly interesting character. One of them, the Prize Essay of Dr. M. B. Wright, of Cincinnati, on Difficult Labours and their Treatment, we have considered worthy of a special notice, and one more extended than it will be convenient for us to bestow upon the other portions of the *Transactions*—we have accordingly assigned it a separate place in our Bibliographs.

The valedictory address of Julius S. Taylor, M. D., after retiring from the chair, as President of the session of 1854, though of a somewhat discursive character, is replete with correct and noble sentiments. Its leading topic is the necessity of union of action, and perfect organization among physicians for the promotion of the character, interests, standing, and usefulness of the profession—the cultivation and advancement of medical science and literature, and the elevation of the standard of professional education. This topic is urged with an earnestness and zeal becoming its importance.

The Ohio State Medical Society, Dr. Taylor informs us, was organized in the year 1846, by the association of twenty-five physicians.

“From that date,” he remarks, “the labours of its members have been devoted and zealous, and their efforts have been crowned with the happiest results, in the production of many reports and papers relating to our science, which are of great importance to the profession generally.

“Since its organization, numerous truly educated physicians have become members, so that at our last annual meeting we found enrolled three hundred and nineteen. This, however, is but a fraction of the many truly and correctly educated physicians of the State of Ohio. Some may, perhaps, object to becoming members of this Society from the fact, that the results of our labours are yet so small. We would say unto such, ‘Come in and assist us, as the harvest is yet large and the labourers few.’

“The results that will follow this organization, if it shall continue to increase in numbers until it has enrolled among its members the name of every physician within the State, that has the true Science of Medicine at heart, and who will be willing to lend his individual aid to the promotion of a general science of Medicine, will soon be found to present an interest so great, to create an emulating influence so profound, an education so perfect, a friendly intercourse so extended, a professional harmony so complete, and so general a desire to aid each other in all that shall tend in the least to advance our cause—the cause of legitimate medicine—that the most skeptical mind will soon be obliged to acknowledge that our cause is just and our objects attainable.

“To accomplish a result so desirable—will require, certainly, a continued

labour; a correct deportment in all professional etiquette; a fixed determination in every member to look to the interest of all as to that of his own. These, however, are but small requirements, and I have no doubt but they can and will be lived up to by all of us with cheerfulness and fidelity. If such shall be the case, as a Society, we have nothing to fear; our cause will be carried onward and upward, until it shall be crowned with the most perfect triumph."

The importance and necessity of organization among all the members of the medical profession, in district, county, and State societies; and the concentration of these local organizations, by a justly apportioned and properly appointed delegation in the National Association—having the same great objects in view as are set forth in the constitution of the State Medical Society of Ohio; mutual recognition and fellowship, the maintenance of union, harmony, and good government among its members, the cultivation and advancement of medical science, and the elevation of the standard of medical education—cannot be too frequently, nor too earnestly urged upon physicians. They who stand aloof from these organizations, or view them with a distrustful and jealous eye, if they have no sinister ends to answer by keeping themselves apart from their brethren, no selfish purposes to be accomplished that would not comport with the requirements of the code of ethics of the profession—if their desire is to base their success only on their characters as educated, intelligent, and honourable practitioners of the healing art, do a wrong to themselves as well as to the entire body of the profession of which they claim to be legitimate members, by refusing to identify themselves with "the common-weal," and lending their personal aid and influence towards its maintenance and promotion. With the elevation and aggrandizement of the entire profession must necessarily be connected the promotion of their individual influence and interests.

A report is presented by the Committee on Medical Ethics through its chairman, Dr. John Dawson. This is a pertinent, and well drawn up report. The committee, as its name imports, has imposed upon it the duty of making known to the society the moral standing of the profession throughout the State. This delicate task the committee have performed with a good deal of tact. By confining themselves, for the most part, to generalities, they have avoided offence to individuals, while by the judicious remarks and suggestions they have thrown out as to the most prominent derelictions of the code of ethics of which the physicians of Ohio have been guilty, and the evils to the profession thence resulting, they will have effected more good, probably, than if they had made more specific charges and brought them home in direct terms to individuals.

After quoting the 1st section of Article I., of the second chapter of the Code of Ethics, Dr. Dawson very pertinently remarks:—

"A doctrine very clearly set forth in this section, is, that the Profession of Medicine has a *character*, which is the property, not of a single member, or of the members of any particular region, but of the Faculty of the whole country. This character is the result of the labours of the upright in heart and the self-sacrificing for centuries; and as a consequence, it is no wonder that there are many to be found who venerate it, and exert their best abilities to maintain its dignity and honour.

"The introduction of an individual into the Medical Profession consists in nothing more nor less than the clothing of him with the character of the profession, and in imparting to him the right to exercise everything pertaining to it, for the good of society and of himself. The possession of such a treasure, together with the rights pertaining to it, must necessarily imply on the part of the individual thus invested, certain obligations, for without the existence of these, the relation would be imperfect. These obligations, among other things, require of him that he shall exert himself in every reasonable manner, and on all proper occasions to maintain the portion of professional reputation intrusted to his care; and that he shall also add to the common stock. For, it has been in this way, and in this way alone, that our common character has been built up—has been formed."

Among the derelictions of the Code of Ethics by certain of the physicians of Ohio, noticed in the report before us, is that of "speaking evil of each other."

"Our conversation," says Dr. Dawson, "about each other, when surrounded by the circumstances of rivalry in practice or in teaching, too often wants candour, liberality, and dignity. Old men are denied the veneration and respect due to their years and services; younger ones, who happen to be prominent, are criticized, persecuted, and occasionally slandered."

Another violation of ethics referred to in the report, is the mean attempt on the part of physicians, when called into consultation with younger and less prominent members of the profession, to weaken confidence in the medical knowledge and skill of the attending physician, with the view of attracting to themselves the future patronage of the family and friends of the patient.

"Impressed," says Dr. Dawson, "with the belief that the value of consultations most frequently consists in inspiring confidence in the physician having charge of the case, and assisting him as one mariner would another that had been tossed about by contrary winds, to keep his latitude and longitude right, until securely anchored in the port of safety, we cannot avoid suggesting the propriety of a review and revision of the whole subject. Consultations cannot be dispensed with without incalculable detriment to the sick, saying nothing about their value, when honourably conducted, to the profession. But we repeat, that it is the testimony of physicians all over the State, that they are diminishing in frequency; that many of the older physicians are avoiding them as useless, while the younger ones, are complaining that they seldom ever get through with them without being injured. To us, the existence of such facts is everything else than agreeable, and of course our only object in alluding to them, is, that our Society may look them in the face, and make an effort at their correction."

It is but proper to state that while the Committee on Ethics have considered it their duty to point out the existence of such deviations from the path of professional rectitude as have been just referred to, they bear cheerful testimony to the honourable character and conduct of "the great mass of the quiet and working portion" of the physicians of Ohio.

"In looking over our State," they remark, "and taking an imperfect survey of our physicians, and the manner in which they are demeaning themselves to the profession and to the people, we see much to excite admiration and gratitude. Labouring for less pecuniary reward, and at greater sacrifices of leisure and all the conveniences and comforts of life, than what in these times pertains to any other kind of business, they seem, nevertheless, as a general rule, to be conducting themselves very creditably, so far as an honest and faithful observance of the essential rules of ethics are concerned, while, at the same time, they are performing their duties to the people, frequently under circumstances of danger, with alacrity."

As a subject following within the range of ethics, and certainly of no slight importance, the committee notice the insufficient compensation too generally accorded to the physician for his professional services.

"To some extent," they observe, "this has always been the case, but at the present, it is eminently so. Every department of labour or service has exacted from the community increased rates of remuneration, while the services of the physician, his deprivations and sacrifices, have experienced, so far as increase of compensation is concerned, no change. Indeed, we are not sure but that the reverse is true. Physician's bills, from all that we can learn, are now paid off with less money, and more reluctance than they were a quarter of a century since. Many of our best educated young physicians find out, on entering the profession, that the compensation for their services, even if they succeed to a reasonable amount of business, is less than what can be obtained in many of the ordinary pursuits of life, and, as a consequence, they abandon the profession. Many, too, who have laboured in the ranks of the profession for years, find out at last that they are failing to secure the means necessary to sustain the infirmities of life, and in disgust turn their attention to other pursuits."

The small pecuniary return which physicians generally receive for the services they render—the impossibility, excepting under circumstances of rare occurrence, to lay by from the legitimate income of the profession any provision for the declining years of life, have induced more than one, who might

otherwise never have been tempted to do wrong, to resort to sinister and unworthy means of increasing their practice and augmenting their fees; or even boldly to break faith with their brethren by entering openly the regular ranks of empiricism and imposture.

If the public would secure for their service a well educated and experienced body of medical practitioners, upon whom they can depend, with full confidence, when disease assails them and their lives are endangered, they must be prepared to afford a compensation more in proportion to the value of the services they require of the physician; one better calculated to place him beyond the fear of want, and to relieve his mind from the harassing cares thence engendered, than they are now willing to do.

Have the profession within their reach any means to raise their compensation to a more equitable footing?

"Heretofore," says Dr. Dawson, "reliance has been placed upon the getting up of fee bills. Badly these have worked in most places where they have been tried. Too frequently the people have looked upon them as combinations to extort exorbitant charges, and while it is humiliating to acknowledge it, it is a fact that, too often, they have had their prejudices strengthened by the testimony of some unworthy member of the profession, one such being, unfortunately, to be found in almost every neighbourhood, who, by uniting in the clamour, is in hopes of obtaining popular favour and increased patronage. In view of this state of things, and of the propriety of doing something, no measure seems to us more likely to be attended with good results than a general movement on the part of the older physicians, those well established in the confidence of the community. Let them, by concert of action, take the proper ground in relation to the matter and maintain it, and the work is done. What they do is generally regarded as right, should it even interfere with the usages of the purse. They owe it to themselves, and to the profession to lead off in a measure so necessary at the present time, and one that unless carried out will require that our profession shall frequently be united with some other occupation in order that the means of a reasonable support shall be secured."

In the conclusion of their report, the committee briefly animadvert on the general standing of the profession, at the present moment, in the estimation of the public.

Like many of the things to which they previously allude, the public estimate of the medical profession is, they confess, not what most of us desire.

The cunning subterfuge, the impudent pretension, the total disregard to truth, the mean truckling to public prejudice and to popular errors which distinguish the bold and industrious opposition to scientific medicine of the vast host that muster in the ranks of the irregulars, whose only aim is gain, has had the effect of destroying, to a very great extent, the confidence in it and its legitimate practitioners, of large portions of different communities.

"It were well," remark the committee, "if this injurious influence was exerted only by the unwary and ignorant. Then we could look forward with some hope to the changes which the schoolmaster is making upon the race, as the era of deliverance from the trouble and annoyance. But how frequent and how common is it to see those communities, that we have been wont to regard as the most intelligent, taking the foremost rank in the patronage of the most transparent humbugs of the day. And, what is still worse, many of our distinguished men, those intrusted with the care of church or of State, and who enjoy a large portion of public confidence, not only seem an easy prey to the mountebank, but, when occasion requires it, are ready to aid, by their personal or official influence, in almost anything that shall detract from the character and claims of legitimate medicine, scientifically studied and scientifically practised.

"Besides the operation of causes *external*, we have those of an *internal* character, the influences of which are decidedly more fatal. To some of these we have already alluded in another connection, and only notice them here to increase attention to them, as our worst enemy. It is an old maxim that teaches the doctrine, that if we would have others respect us, we must respect ourselves. We have seen enough to convince us all, that, although our existence

as a faculty is indispensable to the welfare of humanity, still we cannot rely on the claim of utility alone, as the means of securing confidence, and of giving us popularity. To amenity of manners, and straightforward, upright conduct; to a disposition to treat each other, under all the circumstances of rivalry, as gentlemen, as brethren; to the habit, in all our connections, of avoiding contemptuous or sarcastic remarks in relation to each other; to union among ourselves, and to industry in cultivating every department of nature and art available in the management of physical suffering and infirmity, we may look with confidence as being the only means of placing us in a position to command the respect and influence to which we are so justly entitled."

The prize essay of Dr. Wright on Difficult Labours, which follows the report on medical ethics we shall notice in another place, in the present number of our Journal.

The paper next in order is by Dr. L. M. Lawson, formerly of Cincinnati. Its subject is Practical Observations on the Diagnosis of Phthisis Pulmonalis.

The author presents a very fair, succinct view of the subject, which may be read with profit, especially by the younger members of the profession. In its more advanced stages, tuberculosis of the lungs presents no difficulty in its diagnosis to the well-instructed practitioner. In the very commencement of the disease, however, in what may be termed its formative stage—a period when a correct diagnosis is of the greatest importance—to arrive at a certain judgment requires a careful consideration of all the vital or rational symptoms, and of the local physical signs indicative of the deposition of tubercular matter in the pulmonary tissues. The one without the other is insufficient to render the diagnosis clear and positive.

Dr. Lawson, from his own observations and experience, and the experience of physicians generally throughout the Western States, is irresistibly led to the conclusion that consumption is decidedly on the increase throughout that portion of the United States. "It has been observed," he remarks, "in many localities, that tubercular disease of the lungs, although formerly rare, is now becoming quite common, and those observations apply to so many districts of country, that no doubt can remain on the subject."

Dr. Lawson also notices, as an interesting fact, that there is a relationship, in point of time at least, between the increase of *typhoid fever* and consumption. He is not prepared to assert that there exists any direct and immediate connection between the causes which produce these two affections. He admits that their simultaneous increase may be accidental, or produced by different causes.

"But," he adds, "while this may be true, we cannot altogether disregard several important facts, which appear to favour the affirmative view of the question. At first, it is a pretty well established law, that there exists some degree of antagonism between *miasm* and pulmonary consumption, and that tubercular disease of the lungs is comparatively rare in paludal districts. And the same remark is true in reference to typhoid fever. So long as our regular miasmatic fevers prevail, typhoid fever is seldom witnessed; but as the former subside, the latter increases. It appears evident, therefore, that in proportion as the miasmatic influence subsides, typhoid fever and pulmonary consumption increase; and the suggestion that there may be some possible connection between their causes, is somewhat strengthened by the observation that the development of tubercle not unfrequently follows a protracted attack of typhoid fever. Louis met with such a result in a number of cases, and Dr. Copland regards consumption as one of the sequelæ of typhus. It has been alleged, also, that the product of typhus and tubercle are analogous, though of this there is no decisive evidence. It is proper to add, however, that Andral came to a different conclusion, and supposed typhus fever to be seldom followed by consumption. But, whatever may be the relationship of these affections, we know at least one important and interesting fact, namely, that they have increased in the same districts, and apparently under similar influences, and that they are both kept in abeyance by miasmatic influence."

In our investigation of the etiology of tuberculosis, which has been somewhat close and extended, we have not been able to satisfy ourselves that the

supposed antagonism between a miasmatic influence and the production of tubercle has any foundation in truth. It is very certain that tubercular disease of the lungs is by no means of infrequent occurrence in what are termed miasmatic districts of country, and where diseases universally esteemed to be of miasmatic origin prevail as endemics. From among a very large amount of direct testimony in support of this fact, which might be adduced, we quote the following. It occurs in the report on the Epidemics of Tennessee and Kentucky, made to the American Medical Association at its session of 1853. The statement is by Dr. George R. Grant, of Memphis, Tenn.

"It is believed, and asserted by high authority, that phthisis pulmonalis is not only not so prevalent in malarial as in non-malarial regions, but also that, by a residence in localities confessedly abounding with malaria, the consumptive invalid might even hope and expect a radical cure. An inspection of the table herewith sent will show, as far as it goes, that facts are not in accordance with this opinion; that the disease is undeniably on the increase in Memphis, where malaria is almost as abundant as in the 'Pontine Marshes.' Nor is this all. An examination of the mortuary table not only shows that consumption is the only disease which has consigned victims to our cemeteries in every month of the past year, but it likewise informs us that precisely *one-half* of the 54 deaths reported to have died of it occurred from July to October, inclusive, the very season of the year when malaria is in most abundance, and is exerting its power to the fullest extent.

"No one will run the risk of making himself so justly subject to ridicule as to say that these are *strangers*, who have come hither to spend their *summers* on account of the healthfulness of the place."

The same gentleman, in the report on the epidemics of the same localities, made to the Association in 1854, uses the following emphatic language:—

"Will it not appear almost incredible that, as far south as the 35th degree of latitude, in a region notoriously subject to bowel complaints and to the prevalence of the paroxysmal fevers, two diseases of the lungs—*pneumonia* and *consumption*—should prove to be the cause of nearly one-fourth, or twenty-five per cent. of the mortality? It would scarcely be credited did not figures demonstrate the truth of the statement."

Add to this the testimony of Dr. Drake, who, in his work on the *Diseases of the Interior Valley of North America*, after a careful review of the whole subject, makes the general statement: "In those regions where periodical fevers prevail, consumption is, also, a prevalent disease."

It is not our intention to enter into a review of the practical diagnosis of pulmonary tuberculosis as laid down in the paper before us. It presents no prominent points for notice or criticism, and little that is not familiar to every well-instructed and experienced practitioner.

The general phenomena indicative of the first stage of tubercular deposit in the lungs described by Dr. Lawson are: 1. Emaciation; 2. Persistent dry cough; 3. Infra-clavicular depression; 4. Diminished expansion; 5. Dulness on percussion; 6. Increased vocal fremitus; 7. Altered respiratory murmur—weak, jerking, harsh, bronchial, prolonged expiration; 8. Dry crackling. The existence, he remarks, of these signs, or a fair proportion of them, may be regarded as positively indicative of tubercular deposits.

"The preceding observations refer," says Dr. L., "to ordinary crude tubercles, as they are deposited in masses; but there is another form, denominated *miliary tubercles*, which presents different and less distinct phenomena.

"Auscultators are by no means agreed as to the signs which indicate the presence of miliary or solid tubercles, and it is generally admitted that no fixed and distinctive phenomena exist. Dr. Skoda declares, indeed, 'that there are no distinct signs by which we can with certainty diagnose the existence of acute miliary tubercles.' Dr. Walshe admits the difficulty of diagnosing miliary tubercles, but adds, that the real difficulty consists in distinguishing miliary phthisis from *typhoid fever*.

"My own observations on this subject have been made with much care, and I have been able, in a number of instances, to diagnose the existence of

miliary tubercles, which, unfortunately, was confirmed by *post-mortem* examinations.

"The characteristic signs, or those on which I chiefly rely, are, persistent dry cough; quick, but usually feeble pulse, with general febrile action; slight incompleteness of pulmonary crepitant or finely sub-crepitant rhonchus. Dulness is not appreciable, except in very advanced stages. The existence of the crepitus, extending to the base of the lungs, without the accompanying signs and symptoms of either pneumonia or bronchitis, is as characteristic of acute miliary tuberculosis as dry crackling is of tubercles in masses. The degree of the febrile movement, which is an exact measure of the acuteness of the disease, will vary with different cases, but it is nearly always greater than in the ordinary forms of consumption.

"It is altogether possible that there is a stage of incipency in this variety, in which there will be neither sufficient fever, embarrassed breathing, nor altered murmur, to enable us to make a diagnosis; but, when a little more advanced, I am satisfied that in the phenomena which I have mentioned will usually be found their concomitants."

There is much truth in the following remarks of Dr. Lawson—they deserve the serious consideration of every practitioner. It is in the removal of the diathesis, the morbid condition of the organism upon which the tendency to the deposition of tubercular matter in the tissues of the body depends—the *tubercular predisposition*, as it has been termed—that our success in the cure of consumption will depend. We have, heretofore, too much neglected this "beginning of the evil," and have almost invariably directed our efforts to arrest the results of the disease, the morbid deposit and the consequences resulting from its presence in important organs, rather than the disease itself.

"It is a melancholy fact," observes Dr. L., "that, thus far, consumption has almost defied the skill of the physician and the potency of medicines. No climate, age, or condition is exempt from its insidious ravages; the robust and athletic man, the mere infant, young and old, though not equally liable, are, nevertheless, but too often victims of the disease. Bred in the very composition of the tissues, bone and muscle, membrane and blood, all become contaminated, and finally decay and die, as the inscrutable disease spreads throughout the system. There is a period, however, when the disease consists, not in a morbid deposit, but in what has been and still is called a *diathesis*, and this is the period when our remedies may be most successfully employed, before a fatal disorganization takes place. This constitutional predisposition has been most erroneously and fatally called a *functional* condition, and not actual *disease*; and until that error is corrected and a different doctrine embraced, I greatly fear we shall have but little more success in the cure of phthisis than our predecessors. That the condition which has been called the tuberculous constitution or diathesis is something more than a *functional* disorder, is clearly proven by the analysis of the blood, the examination of the tissues, and the general physical conformation. Long anterior to the deposit of tubercles, the blood is found deficient in globules, hæmatin, and iron, with an excess of albumen and water, and altered or depraved fibrin. This, and but little more, exists during the first stage of tubercular deposits; and yet it is averred that this is *not disease*, but a mere *tendency* to it. The truth is, the tuberculous constitution is a positive disease, which is exhibited in a changed condition of the blood, and of many of the solid tissues.

"I do not deny the curability of consumption, especially in an early stage; on the contrary, there is, doubtless, a fair proportion of cases, treated judiciously and at an early period, and even pretty far advanced, radically cured, and others, for a time, suspended. But while this is true, we neglect too often the golden opportunity of doing good, by failing to recognize the constitutional derangement as positive disease, and thus wait for fatal organic lesions of a secondary character, before our agents are applied. The hope of the profession still seems to be that a great *specific* may yet be found capable of curing all cases, and in all stages; but the hope is as vain as the assumption is practically mischievous and fatal. Our country is now deluged with cod-liver oil, and the professional man is apt to conclude that he has done his whole duty

when this nauseous drug has been freely given. But I fear that experience is here again at fault. No modern practitioner can have more confidence in the cure of consumption with cod-liver oil than had Beddoes in the use of digitalis in that affection. But whatever may be the true merits of cod-liver oil, it is to be greatly feared that the present indiscriminate mode of employing it can hardly fail to be productive of as much harm as good, and that the mortality of consumption will not, in the slightest degree, be diminished by its introduction."

The paper of Dr. Lawson is followed by an interesting report on Surgery, by Dr. G. V. Dorsey, of Piqua. After some very judicious introductory remarks in reference to the causes which led to a separation between medicine and surgery, and on the importance of the study and practice of the two being combined, the reporter considers briefly the management of fractures and dislocations, with the treatment of the diseases of the bones and joints; the subject of amputations; injuries of the head; extirpation of tumours; wounds of the throat; permanent cure of hernia; the use of anæsthetics in surgical operations, etc. etc. On each of the subjects indicated, although all are treated with great conciseness, the remarks of the reporter are occasionally pertinent and of value, though not unfrequently of too loose and general a character to lead to any important practical results. The cases interspersed throughout the report will be read with interest.

We cannot agree with Dr. Dorsey in his defence of the propriety of inventors of surgical instruments securing to themselves the *profit* of their inventions by a patent, and we confess we have experienced both pain and mortification that so important and influential a body as the State Medical Society of Ohio, should, by a formal vote, declare "that it is not derogatory to medical dignity, or inconsistent with medical honour, for medical gentlemen to take out a patent-right for surgical or medical instruments."

The framers of our national code of ethics have, with great propriety, declared it to be "derogatory to professional character for a physician to hold a patent for any surgical instrument or medicine." There is no identity whatever between the copyright of a book and the patenting of a surgical instrument or medicine. In the first case, there is no restriction upon our use of the knowledge, the facts, and information contained in a copyright work; we have a right to make use of it in any manner we may think proper; we are at perfect liberty to apply it for our own advantage, or to communicate it to others, who are equally unrestrained in their employment of it. We are only prohibited from printing the work in which that information is contained. But not so with a patented instrument or medicine; we may be convinced of its usefulness, we may fully understand its construction, but under no emergency can we avail ourselves of it, or recommend others to do so, unless we have purchased the instrument or medicine of the proprietor, or, at least, the right to use, manufacture, and vend it. Independently, therefore, of circumscribing within certain limits a useful invention in surgery, or an important discovery in therapeutics, the *profits* demanded as a remuneration by the inventor will often prove a barrier to the true value of the instrument or medicine being tested by repeated experimental trials. We have said nothing of the inconsistency of a private appropriation, solely for self-aggrandizement, of any improvement in surgery or medicine calculated to relieve human suffering or save human life, with that beneficence and liberality which should ever be the distinguishing characteristic of our profession.

The report concludes with some pertinent remarks on the necessity of a combined effort to remove the impediments to practical anatomy originating in the fears and prejudices of the community.

A communication from Dr. G. Mendenhall, of Cincinnati, calls attention to some facts presented in an inaugural dissertation, presented to the Miami Medical College for the Degree of Doctor of Medicine, by E. T. Baily, of Emmettsville, Indiana, on nitric acid as a remedy for intermittent fever.

Dr. Baily states that he has treated over ninety cases of intermittent fever with the nitric acid, with remarkable success; all recovering promptly except ten; and, in all of these unsuccessful cases, the remedy was discontinued

contrary to directions. Of the whole number of cases, fifteen were of the tertian type, and seventy-five of the quotidian. In fifty cases, there was no return of the chill after commencing the use of the acid. In the others, rarely more than one paroxysm occurred, and in no case a third. When a paroxysm occurred after taking the medicine, it was in every case diminished in intensity and duration.

Dr. B.'s mode of proceeding is to give from five to eight drops of the commercial nitric acid, properly diluted, once in six hours, without regard to intermissions or exacerbations. Cathartics and alteratives may be necessary, for the purpose of changing certain conditions of the system; but, so far as the arrest of the paroxysms is concerned, the acid may be given without any preparatory treatment.

The next paper is on the Etiology and Curability of Phthisis Pulmonalis, by Dr. C. G. Comegys, of Cincinnati. Though a mere outline, in which the several questions embraced in the general subjects of which it treats are rather hinted at than discussed, and containing little, if anything, strictly original, the paper is nevertheless a deeply interesting one. The views of the author are in the main correct, and although opposed, in many particulars, to what have been esteemed well-established facts, they will be found to be, very generally, borne out by recent and more accurate observations.

"Phthisis pulmonalis," Dr. C. remarks, "is now known to be a common disease in all civilized nations, and the remark may be extended, almost as fully, to embrace the people, whether savage or civilized, in all parts of the habitable globe; but it does not prevail to the same extent in all countries; and the most secure residences seem to be those so far north as to escape the greatest vicissitudes in the seasons. Thus in Stockholm, the deaths from this disease only amount to one-fifteenth of all; while in London, Paris, and Berlin, they amount to almost one-fifth. In some of the German cities, the proportion is also much larger than in St. Petersburg. Many writers still hold out the idea that warm climates are comparatively exempt from this disease; but since statistical research has been made, the deaths from phthisis have been found to be almost as common as in higher latitudes. Thus, for the eastern continent, the rate is almost as great at Rome, Naples, Madrid, Lisbon, Marseilles, Malta, the Ionian Isles, and the north of Africa, as it is in England.

"From the researches made on our own continent, the same result is established; in fact, according to the statistics collected by the late Professor Drake from the reports of the British army, and the sanitary publications of some of the cities on our seaboard, the proportion of deaths from phthisis is greater in the south than in the north. These statistics relate to the West Indies, as well as to the British possessions further north. Statistics collected in Brazil and Martinique, by Drs. Rufz, Jobins, and Sigaud, show that it is as murderous in those places as in Europe or North America."

In the Island of Cuba, we may add, the deaths from phthisis for the city of Havana constitute considerably more than one-fourth of the entire mortality, and for the rural districts upwards of twelve per cent. In the city of New Orleans, La., the deaths from phthisis constitute 9.51 per cent. of the entire mortality. The deaths from the disease in neither the Island of Cuba nor in New Orleans are chiefly confined to strangers who visit these places for the recovery of their health, but occur in a very large proportion among the native population.

Inherited tendency to tuberculosis has been supposed the chief, almost the only, predisposing cause of the disease. But, while the hereditary character of the disease is well established, we nevertheless admit, with Dr. C., that the children of tubercular parents are by no means necessarily the subjects of phthisis. We have in numerous instances known them to entirely escape an attack; while, on the other hand, very many have been its victims in whose families, for many generations back, on the side of neither the male nor female progenitors has the disease ever occurred.

Lebert gives the result of his observations in 132 diseased families, and shows that in 87, or two-thirds, no hereditary influence could be traced. In

only about one-tenth of the cases occurring in his practice could Louis trace it to inheritance, Piorry in about one twenty-third, and Rilliet and Barthez in one-seventh. With these positive evidences before us, we are almost inclined to subscribe to the opinion of Dr. Comegys, that "death from phthisis, as an inherited disease, is the exception and not the rule."

Dr. C. very correctly remarks that "a single bad hygienic condition cannot be regarded as sufficient to the production of tubercles; it is only an association of several conditions that exerts a real influence." We may remark that the production of the tubercular dyscrasy is not the result of any rapid action of morbid causes, but of the slow and continued action of those influences which have a tendency to gradually impair the vital energies of the system—impair digestion and impede regular and healthy hematosis and nutrition.

Dr. Comegys states that "the most favourable external hygienic circumstances" will not prevent the development of tubercles. Not always, it is true; moral causes, long-continued and exhausting diseases, may slowly impair the organic functions and give rise to tubercles; but the influence of even these causes in the production of tubercular disease may be, to a very considerable extent, averted by the most-favourable external hygienic circumstances.

"The influence of the seasons on the production of tubercles," Dr. C. remarks, and with great truth, "has been exaggerated more by hypothesis than by observation."

"The special influence of professions or occupations in the production of tubercles," says Dr. C., "has not been well established."

We believe this assertion will not be borne out by accurate statistics. Indoor occupations of a purely sedentary character, especially those demanding a constant constrained position of the body; occupations, also, which, besides being sedentary, call for prolonged and intense mental application, are confessedly more liable to produce tubercular disease than such as are carried on in the open air, and which allow the free and unconstrained exercise of the muscles generally; or which occupy and interest the mind without overtaking it—even when every other hygienic condition is, in all of them, equally favourable.

After endeavouring to show, what nearly all pathologists now admit, that the etiology of phthisis pulmonalis is to be sought in "the imperfect digestion and elaboration of the elements of the food consumed;" in other words, in disordered digestion, assimilation, and nutrition—Dr. C. infers, and very correctly, "that the number of cases that may be successfully treated, and the diatheses that may be combated, will depend largely upon the period at which the treatment is commenced, and the amount of control obtained in the direction of the functions of digestion and assimilation, and of the external conditions that surround our patients."

The paper next in order is on the Pathology, Symptomatology, &c. of Latent Pneumonia, by Dr. S. G. Armor, of Cincinnati.

This form of pneumonia is asserted to be of frequent occurrence in the malarious districts of the West, and being unattended in its early stages by the more prominent symptoms of pneumonic inflammation, making its approaches silently and gradually, its presence is often unsuspected by either patient or physician until the work of destruction is accomplished. There is little constitutional disturbance, so little that the patient may often pursue his usual occupations.

"If the disease has made considerable progress," says Dr. A., "on being *interrogated*, the patient may complain of a troublesome, but not painful cough, with a somewhat viscid and scanty expectoration, the tongue moist and slightly furred, appetite unimpaired, respiration hurried, inability to take exercise, and previously to *hectic* symptoms, there will be found a marked disparity between the respiration and the circulation."

"In latent pneumonia, *congestion* always predominates over the determination of blood. Hence its marked asthenic character." "There is little or no pain; fever and other signs of increased action are comparatively slight; nor does it tend to such speedy results as the acute variety."

Latent pneumonia has the same stages as the ordinary disease; they occur, however, more slowly, and are attended with less prominent symptoms. The

true crepitant râle, so constantly present in the stage of engorgement of active pneumonia, and even heard in the local or lobular variety, is rarely detected; if heard at all, it is as a muco-crepitant râle.

Latent pneumonia is usually seated in the inferior portion of the left lung, extending from below upwards.

Gangrene of the lungs is an occasional termination of the disease, especially when it occurs in connection with an alteration of the blood. Derangement of the liver, followed by jaundice, is a frequent complication.

We present, in his own words, Dr. Armor's account of the treatment of latent pneumonia.

"Caution has to be exercised in the use of all remedies which are calculated to depress the vital forces. Beef tea and quinia are oftener indicated than bloodletting and antimony. The patient's strength must be supported, the local hyperæmia must be overcome; and, although to accomplish this it may be necessary, in some cases, to abstract blood locally by means of cups, it may be equally necessary, at the same moment, to husband the patient's strength by the administration of tonics."

In latent pneumonia, it is all-important to guard against the occurrence of bronchitis by placing the patient in an apartment of a moderately warm and uniform temperature, and guarding against sudden exposure to cold or drafts of cool air.

"In the more advanced stages, the moist, coarse râles are frequently heard over a large extent of surface. If, in connection with this physical symptom, the pulse be feeble, the skin dusky, cold, and relaxed, we should lose no time before putting the patient on the use of some of the stimulating expectorants, and, perhaps, none equal in importance the senega and sesqui-carbonate of ammonia. This combination appears to be peculiarly adapted to those cases in which the secretion into the tubes is considerable, and the patient expectorates with difficulty."

"The sesqui-carbonate of ammonia, wine, bark, and the nutritious animal broths, well seasoned with the chloride of sodium, often exercise a marked control over the local disease."

Dr. Armor considers the effects of blisters to the chest as more certain in latent than in ordinary pneumonia; and that their use may be resorted to more early. They should, he thinks, be small in size, and frequently repeated; never being kept on long enough to produce exhausting suppuration. Counter-irritation, from the use of strong acetic acid and turpentine, he notices as also an important remedy.

"Any indication of periodicity should be promptly met by the use of the *sulphate of quinia*. In malarious districts, the powers of this remedy are often singularly displayed in the treatment of this form of pneumonia."

Dr. R. Thompson presents a drawing and description of two instruments, a "gouge bone cutting forceps," and "a hand-drill or bone perforator." The first he considers of great convenience and advantage in that class of operations on bones which has hitherto required the employment of the mallet and chisel, gouge, trephine, chain-saw, etc.; and the second he offers as very useful in many cases. The peculiarity of these instruments cannot be understood without a reference to the drawings by which the descriptions of them are accompanied.

2. The reports of the Medical and Chirurgical Faculty of Maryland are only two in number—one on surgery, by a committee, of which Dr. Christopher Johnston is chairman; and one on medical chemistry, by a committee, of which Dr. Charles Frick is chairman. They are both of a very high order, presenting a very full and very able digest of the present state of knowledge in reference to several important questions in the respective departments of medical science of which they treat.

The report on Surgery comprises observations on the *microscope* in relation to *diagnosis* and to *classification* in certain diseases; the *union of fractured bone*, and the treatment of *fractures*; *aneurism* and its treatment; and the

pathology and curative indications in certain *ophthalmic diseases*, together with some modern instruments to assist in their detection, etc.

The report is an extremely interesting one, and may be read with profit by every surgical practitioner; especially by those whose locations are remote from the great centres where the progress in each department of chirurgical medicine becomes first known and duly appreciated. The interest and value of the report in question do not consist in its embodying a series of original observations in reference to either of the topics embraced in it, but in the faithful, clear, and, at the same time, concise summary it presents of the recorded facts and deductions of the authorities of the profession.

It would be impossible to present any useful analysis of either of the reports presented at the last session of the Medical and Chirurgical Faculty of Maryland; and from that on surgery we do not think it necessary to make any extracts.

The report on Medical Chemistry is restricted to the *chemistry* and *pathology* of the urine.

The important physiological fact of the manufacture of sugar by the kidneys, as shown by the beautiful experiments of M. Bernard, and its relation to the pathology of diabetes is first considered. Reference is also made to the experiments of Valentin, of Bern, in which sugar was detected in the liver, blood, and watery contents of the stomach of marmots in whom hybernation had existed over two months; as well as to the experiments of Henle, to show that "the nervous reflex action, upon which depends the secretion of sugar by the liver, is generated in the liver itself, by the stimulating power of the blood of the portal vein."

The report, in noticing Regnoso's assertion that, from his observations, he has found the urine of most aged persons, as well as those suffering under imperfect respiration, to contain sugar, remarks, "this statement requires confirmation; we ourselves have not been able to verify it, and, with one exception (*Chemist*, April, 1853, p. 304), we are not aware that any one else has done so." He has lately declared, in addition, that the urine of epileptics, after the attack, also contains sugar. The report before us says, that in two cases in which its authors have examined the urine by the means indicated by Regnoso, "they failed both times to detect it, so that, however general the fact may be, it certainly is not universal, as he concludes."

The report before us presents a very able summary of the recent facts developed in relation to the leading subjects embraced under the general head of medical chemistry, including in that term both physiological and pathological chemistry. The report concludes with an account of some interesting experiments, "undertaken with a view of showing the influence of different remedies upon the urinary secretion."

The experiments were performed on the inmates of the Maryland Penitentiary Hospital, during a period of nearly ten months.

"Each inmate admitted to the hospital was subjected to the treatment that seemed appropriate to his disease, and without reference to the investigations that were being pursued. Each was provided, at his bedside, with a clean glass vessel of sufficient size, in which he was directed to pass all the urine he voided. Daily measurements of these amounts were taken, the specific quantity ascertained, and, after recording them, the particular remedy, or combination of remedies, with the doses, was set opposite to each name. Subsequently, from these tables the amount of solids was calculated, and the deductions then made which are herein embodied. In this way nearly 2,000 separate observations were collected with very little trouble; but as, in many instances, the treatment was of too complicated a nature, or of too short duration, to allow of any proper conclusions being drawn, a large number of these were rejected, and the tables here presented are made up of about 1,200 separate observations. We might remark here that the largest quantity of urine and solids the tables record, is 100 ounces, containing 2,800 grs., more than three times the average amount. And that in no single instance of the 2,000 did we ever detect sugar, although a very large number were examined with this view—a result entirely opposed to the statements of Regnoso.

"The conclusions here presented are average results obtained from the examination of a large number of cases taking the same remedy, without reference to the disease under which they were labouring. In but one or two of the averages were a less number than 50 taken, and in one more than 100. It may be objected that the influence of disease, a most important consideration, is here omitted. But this omission is almost unavoidable, and from the fact that the different diseases bear nearly the same proportion to each individual remedy, this element becomes in a measure equalized, and does not interfere with the correctness of the conclusions, as might at first be anticipated.

"We might also remark that the amount of solids, as deduced from the specific gravity, is not exactly correct, but still sufficiently so for comparison; and that the amount of solids, as well as of fluid, is less than it ought to be in every instance, as no account could be taken of the quantity passed while at stool.

"The quantity of urine in a healthy man varies from 20 to 60 ounces, containing from 400 to 1,600 grains in the 24 hours, and often much beyond these limits. The cause of these variations are partly internal, and partly external—some transitory, others of a longer duration—some simple, and others complicated. Of these, the influence of drink is most remarkable. It may increase the secretion, within four hours, to three or even ten times its normal amount. The quantity voided at night is generally the least, while in the morning it increases, and in the afternoon or evening attains its maximum. As regards the quantity secreted in diseases, it is found to be normal in most chronic affections unattended with fever. It does not often exceed the usual quantity, and is diminished in most diseases. This diminution, to the greatest degree, is found on an average in the acute fevers."

We present, in tabular form, the result of the experiments referred to—

Substances employed.	Quantity given.	Number of analyses.	Urine in ounces.	Solids in grains.
Sulph. quiniæ and sulph. ferri,	{ 3 grs. of first, and 1 of second,	48	57	1248
Juniper tea,	{ 4 oz. berries to a quart of water in the 24 hours. }	39	56	1134
Prussiate of iron,	4 grs. 3 times daily,	52	52	1095
Sulphate of iron,	1 gr. 3 times daily,	53	53	992
Phosphat. ammoniæ and wine of colchicum,	{ 20 grs. of first, 15 drops of second. }	42	42	936
Acetate of potass.	{ from $\frac{1}{2}$ drachm to $\frac{1}{2}$ ounce in one quart water daily. }	110	40	926
Decoction of Peruvian bark,	{ 2 drachms to a quart of water, $\frac{1}{2}$ pint daily, }	93	49	925
Wine of colchicum,	15 drops 3 times daily.	47	39	893
Gin,	{ 2 oz. in water, in divided doses, daily, }	17	51	853
Whiskey,	in the same quantity.	22	47	842
Phosphate of ammonia,	10 grs. 4 times daily,	43	38 $\frac{1}{2}$	834
Mur. tinct. ferri,	45 drops daily,	48	52	810
Iodide of potass.	15 grs. daily,	63	41	809
Sulph. quiniæ,	12 grs. daily,	57	39	801
No medicine,	{ persons labouring under disease requiring no internal medication, }	71	34	795
Phosphate of lime,	12 grs. daily,	19	36	792
Spir. nit. ether,	half drachm daily,	51	37	782
Strychnine and mur. tinct. ferri,	{ $\frac{1}{20}$ gr. of first and 15 drops of second, 3 times daily, }	48	33	725
Cod-liver oil,	2 oz. daily,	63	38	720
Morphia,	$\frac{1}{2}$ gr. daily,	48	32	627
Strychnine,	$\frac{1}{20}$ gr. three times daily,	53	29	500

3. In the report on Hospital Hygiene, Dr. Griscom presents a striking illustration of the virulence of the morbid exhalation from the bodies of patients labouring under low forms of fevers, when such patients are congregated in hospitals, even when the wards of these are apparently not deficient in respect to size and elevation, nor absolutely defective in means for ventilation, as well as of the tenacity with which the poison thus generated adheres to the atmosphere and walls of the apartments where it was generated.

In the building attached to the New York Hospital, up to January, 1852, immigrants labouring under typhus fever had been received in considerable numbers, some to spend merely a night previously to their removal to the fever hospital on Staten Island, and others as permanent patients. This building would be regarded by almost any person who should visit it as possessing, in elevation, general situation, and internal arrangements, advantages superior to a great majority of buildings devoted to hospital purposes. In January, 1852, however, the atmosphere of the whole house seemed to have become thoroughly infected; patients with rheumatism, or other benign complaints, began to succumb to the power of the pervading miasm, even in some of the wards into which no typhus cases had been admitted.

"Notwithstanding that thorough cleanliness, and as free ventilation as possible, were maintained, and the immigrant cases were rigidly confined to their particular apartments, the difficulty increased to such a degree as at last to render necessary an entire change in the economy of the establishment with respect to this disease. At the suggestion of the visiting physicians, the Board of Governors, first, prohibited the reception of any more immigrant night cases; and secondly, a thorough purification of all the typhus wards was ordered. The number of patients was reduced, so as to allow one or two wards to be vacated, and successively the infected rooms were disinfected, and exposed to the external air night and day for several weeks, until the whole was renovated."

In this work of purification, four men, three masons and a laborer, were employed to cleanse the walls and ceilings, and whitewash them where necessary. "They wrought only in the wards which had been vacated, and then only after each had been thoroughly aired by open doors and windows. These men did not enter any of the wards occupied by the sick; they had no occasion, in going to and fro, to see any part of the premises but the large halls, the stairways, and the apartments they were at work in. They were engaged there one week. The labourer passed a great deal of his time in the open air, preparing and carrying materials for the others, and hence visited the building only occasionally, remaining but a short time in it, except when, being otherwise unoccupied, he aided the others a little at their work."

Within six days from the period when they finished their work at the hospital, three of these men were attacked with typhus fever, of whom two died. They were treated at their own homes, which are represented as having been comfortable residences, and they were all temperate and respectable persons.

In contrast to the foregoing, and as an evidence of the beneficial influence of free ventilation in disarming typhus fever of its infectious and malignant character, the following is related by Dr. G.:—

In the month of August, 1837, a number of ships with immigrant passengers arrived at Perth Amboy, from Liverpool and other ports, on board of which ship fever prevailed. There was no hospital or other accommodation in the town, in which the sick could be placed, and no person would admit them into private dwellings, fearing the infection of the fever. They could not be left on board the ships. An arrangement was made to land the sick passengers, and place them in an open wood, adjacent to a large spring of water, about a mile and a half from the town. Rough shanties were erected, two in number, thirty feet long, twenty feet wide, boarded on three sides about four feet up, covered with sails, and floored. Thirty-six patients were taken from on board ship with boats, landed as near the spring as they could be got, and then carried in wagons to the shanties, under the influence of a hot sun, in the month of August. Of the thirty-six landed, twelve were insensible, apparently in the last stage of ship fever, and not expected to survive twenty-four hours. The

day after landing, there was a heavy rain, and the shanties affording no protection with their "sail roofs," the sick were found the next morning wet, and their bedding, such as it was, drenched with the rain. It was replaced with such articles as could be collected from the charity of the inhabitants. The number at the encampment was increased by new subjects to the amount of eighty-two in all. On board of one of the ships, which was cleansed after landing the passengers, *four* of the crew were attacked with fever, and two died. Some of the nurses at the encampment were taken sick, but recovered. Of the whole number of the eighty-two passengers removed from the ships, *not one died*. Pure air, good water, and perhaps the rain—though only the first thirty-six were exposed to it—seem to have effected the cure. "The medical treatment," remarks Dr. C. M. Smith, who had charge of the patients, "was exceedingly simple, consisting, in the main, of an occasional laxative or enema, vegetable acids and bitters; wine was liberally administered, together with the free use of cold water, buttermilk, and animal broths." The four sailors, who sickened after the arrival of their vessel, were removed to the room of an ordinary dwelling-house; the medical treatment in their case was precisely similar, yet two of them died. Two of the patients suffered from carbuncle while convalescing. Dr. S. adds: "My opinion is that, had the eighty-two treated at the encampment been placed in a common hospital, many of them would also have fallen victims. I do not attribute their recovery so much to the remedies administered as to the circumstances in which they were placed; in other words, a good washing to begin with, and an abundance of fresh air."

Dr. Griscom regards the first of the cases just related as "presenting a type of the average hygienic character of hospitals in general *as they are*; the last a type of what they *should be*, in this respect, excepting, of course, the materials and style of structure.

"Regarded in its general aspect as a source of light and health," says Dr. G., "an ample supply of pure air, in conjunction with the immediate removal of secreted and exhaled impurities, beyond the possibility of a re-inhalation, is a subject of profound interest to all humanity; but to the practitioner of medicine it presents itself with increased force. There is imposed upon us a double obligation. The question should be constantly before our minds, whether we shall deny, or allow to be denied, to our patients, the use of oxygen in the fullest measure in which it can be found in the atmosphere? Whether, while searching our *materia medica* for the most appropriate remedies, according to our theories of disease and treatment, we will continue to overlook the most potent of all restoratives, that derived from Nature's own laboratory?"

"Let it never be forgotten for a moment that this agent—to procure which we have neither to dig into the earth, nor transport from foreign climes, nor distil from the alembic, nor refine in the crucible, but which is pressed upon us with a force and in a measure equalled only by the supreme benevolence which furnishes and unceasingly renews it; this agent, when left free to act its part, removes the effete poison from the blood, and imbues it with continual health and freshness, but when stifled and confined, whether intentionally or by accident, turns, like a viper, upon the arm that nourished it, and plants a deadly venom in its veins."

The report on Solidified Milk, though ostensibly emanating from the Committee on Public Health and Legal Medicine, is, we suspect, from the pen of the same gentleman to whom we are indebted for the preceding one on Hospital Hygiene.

Every means adapted to preserve for a length of time, and in a portable form, those alimentary substances necessary for the support of human life, or which add materially to his real comforts, especially such substances as quickly deteriorate, either by the action upon them of external atmospherical influences or by the spontaneous reaction of their own elements, is a subject of deep interest. In the various circumstances in which man may be placed, it is possible for him to be removed for months and even years beyond those sources from which a supply can be obtained of the food necessary for his sustenance, in that unaltered condition upon which its palatableness and wholesome qualities mainly depend. It is conferring upon him, therefore, an essential benefit to supply

him with necessary food and alimentary luxuries in a form of concentration and durability in which their natural flavour and wholesome nutritious properties remain unimpaired.

"As an article of food, under all circumstances of life, civilized and barbarian, in infancy and in age, in sickness and in health, *milk* is at once a luxury and a necessity; and it is remarkable that, being such, it, of all the sources of man's sustenance, should be the most susceptible of spontaneous decomposition, and this not so much from the influence of the atmosphere or any other external agent upon it, as from its own innate tendency, arising from its complicated and peculiar organization; a few hours, and, in warm weather, even a few minutes, being sufficient to change its constituent character. Any method to preserve the integrity and nutritive qualities of milk, so that it may be available at any time as diet, must therefore, if successful, be regarded as among the most interesting and valuable of the gifts of science to humanity."

The committee, whose report is before us, state their belief, derived from a very thorough examination of the whole subject, that this most desirable object has been attained by a simple and ingenious process devised by Mr. S. J. Blatchford, of Dutchess County, N. Y., who manufactures and supplies for sale solidified milk, which the committee have ascertained to be the solid constituents of pure milk, combined with a little less, by weight, of white sugar.

"It contains," they assure us, "no other foreign substance." The various solids of the original fluid are preserved intact, even the butter globules being unbroken. It is readily and perfectly soluble in water, and when so dissolved in proper proportion, it is, in fact, milk, as it was secreted by the cow, with the sole exception of the sugar which accompanies it; and the only medicinal or culinary operations in which ordinary milk is required, and the solidified article cannot be used, are those in which sugar is inadmissible; on the other hand, whenever sugar is required in connection with milk, they are here found together.

"Blatchford's Solidified Milk comes to us mainly in the form of a tablet, covered with tin foil. It has a light yellow, slightly mottled appearance, is of a very firm texture, but yields readily to the knife or grater. Each tablet weighs a pound, and will make in solution five pints of rich milk. When pulverized, it is readily soluble in hot or tepid water, while cold water requires rather longer time, but the solution is perfect in either. The tepid solution approaches nearer the standard of natural milk. The only objection, besides that of the presence of sugar, which can be made to it, is an empyreumatic flavour somewhat similar to that of boiled milk, which it receives in the process of manufacture. This varies in degree, but it is much less distinct when the solution is made with cold than with hot water; and in the preparation of custards, puddings, arrowroot, wine whey, ice creams, &c., in all of which the committee have practically tested, it disappears. To many persons, this flavour is not objectionable, and with many others the palate soon becomes reconciled to it, and in a short time fails to recognize it. By contemplated improvements in the apparatus for manufacturing it, this objection is expected to be entirely obviated.

"A solution in cold water, allowed to stand sufficiently long, will exhibit a surface of rich cream, much more abundant than is found in the same quantity of milk carefully selected in the city; from this cream your committee have caused good butter to be made, from which all traces of sugar are easily washed away, showing that this substance is only mechanically, and not chemically, incorporated with the milk constituents. In solution, it does not acidify so soon as the milk of city consumption.

"Besides the hard tablet, the solidified milk is also furnished in a granular form, inclosed in cans; it is thus more convenient for use, and will doubtless keep sweet for many months, though probably more liable to change than in the other form."

D. F. C.

ART. XVI.—*Reports of the American Institutions for the Insane.*

1. *Of the New Hampshire State Asylum*, for 1852 and 1853.
2. *Of the McLean Asylum*, for 1853 and 1854.
3. *Of the Retreat at Hartford*, for 1853.
4. *Of the Asylum for the Poor, Blackwell's Island, N. Y. City*, for 1853.
5. *Of the Indiana State Hospital*, for 1853 and 1854.
6. *Of the Illinois State Hospital*, biennial for 1853-54.
7. *Of the Missouri State Asylum*, biennial for 1852-53.

1. IN July, 1852, Dr. Andrew McFarland resigned the office of Superintendent of the *New Hampshire Asylum for the Insane*, having, in the language of the Trustees of the Institution, "with signal ability and devotion, discharged the duties for about seven years." He was succeeded by Dr. JOHN E. TYLER, in whom the Trustees believe that they have "a judicious, efficient, and devoted Superintendent."

The report of Dr. Tyler for the fiscal year ending on the 31st of May, 1853, seven months after he became connected with the Asylum, is limited to about half a dozen pages, and contains but little matter of general interest to medical men. It is a very prudent and sensible production for a beginner.

	Men.	Women.	Total.
Patients in the Asylum May 31, 1852	63	55	118
Admitted in course of the year	68	64	132
Whole number	131	119	250
Discharged, including deaths	61	46	107
Remaining May 31, 1853	70	73	143
Of those discharged, there were cured	41	22	63
Died	5	3	8

Dr. Tyler complains that the number of patients is so great as to prevent a proper classification of them.

The report for the year ending May 31, 1854, is somewhat more extended than its immediate predecessor, but is, in a great measure, confined in its subjects to the *matériel* of the establishment, and to topics already familiar to our readers. The doctor complains of the pressure from without of patients, and proposes that an additional wing and a building for the violent shall be erected. "The house is now lighted with gas, and we not only find its use more convenient, comfortable, and cleanly than oil, but its brilliant light a curative means in making our previously half-lighted halls cheerful and pleasant." He says nothing of the comparative expense.

	Men.	Women.	Total.
Patients May 31, 1853	70	73	143
Admitted in course of the year	72	69	141
Whole number	142	142	284
Discharged, including deaths	67	56	123
Remaining May 31, 1854	77 ¹	84 ¹	161
Of those discharged, there were cured	34	29	63
Died	7	7	14

The whole number of patients exceeds that of any previous year by 64.

"During the whole year our household has enjoyed remarkable physical health. We have been entirely exempt from epidemics of all sorts, and acute disease has been almost unknown. Cleanliness, regularity of life, and a most healthful location, have been the chief causes of this desirable state of things. The deaths which have occurred, with a single exception, were of those who

¹ According to the previous figures, these should be 75 and 86.

for a long time had been considered incurably insane, and who at last were literally worn out by the continued and unremitting force of their malady."

Patients admitted from 1843 to 31st May, 1854	1,199
Cured	497
Died	106

2. Dr. BELL, of the *McLean Asylum*, has written but few reports of any length, and in the two which are now before us he is even unusually brief, both of them occupying but about a dozen pages. One reason of this brevity is mentioned in the extract which we subjoin, merely remarking that, although it may be good and sufficient for the district from which the *McLean Asylum* is principally supplied with patients, it is hardly equally so for many other sections of the country.

"There was a period in the history of the institutions for the insane of this country when their annual reports were looked for with an interest natural to a new topic, and when so many communities were about engaging in the great work of providing for the insane, that all information which could throw light upon the path of duty was eagerly and gratefully accepted. That time is now passed, for the demand has been essentially met, and good taste and propriety are no longer in antagonism with philanthropy, as to spreading to the world the often painful incidents connected with a sad disease and its victims. It would ever be an easy service to furnish a prolonged and interesting narrative of the cases of an asylum, were the motives now allowable which formerly justified such communications with the public."

	Men.	Women.	Total.
Patients at the commencement of 1853			201
Admitted in course of the year	61	53	114
Whole number			315
Discharged, recovered	30	28	58
Died	7	10	17
Remaining at the close of 1853			195

"From a minute, kept during a portion of the year," says the report, "it is probable that we have been obliged to refuse three times as many patients as have been received."

It will be remembered by those who have read our previous notices that Mr. Appleton gave a large fund to this institution, for the purpose of constructing apartments specially intended for persons able and willing to pay a liberal remuneration for their accommodations. The object of the donor has been partially accomplished. The "Appleton ward" for men has been completed, and in reference to its operation we find the subjoined remarks:—

"The patients themselves, in these rich and spacious quarters, can draw no unfavourable comparisons with their situations at home, and are spared one pang in the distress incident to their disease. The only drawback suggested or anticipated—that the patients who did not enjoy the new accommodations might indulge a certain sentiment of jealousy towards their more fortunate associates—has never been manifested."

Report for 1854:—

	Men.	Women.	Total.
Patients at the commencement of the year	94	101	195
Admitted in course of the year	70	50	120
Whole number	164	151	315
Discharged, including deaths	67	53	120
Remaining at the close of the year	97	98	195
Of those discharged, there were cured	32	27	59
Died	5	11	16

The *McLean Asylum* was opened on the 6th of October, 1828. It was under the superintendence of Dr. Wyman about sixteen years, and of his successor, Dr. Lee, two years. Dr. Bell has been the incumbent of the office since the

commencement of 1837, a period of eighteen years. We believe this to be the longest term of service of any officer who has occupied an office of the kind in this country.

Patients admitted under Dr. Wyman	. . .	1,122
“ “ Dr. Lee	. . .	189
“ “ Dr. Bell	. . .	2,572
Whole number admitted	. . .	3,783
Discharged, including deaths	. . .	3,588
Cured	. . .	1,802
Died	. . .	322

“I cannot but believe,” remarks Dr. Bell, “that the time is near when the necessity of dividing this asylum, and establishing a department for one sex elsewhere in the vicinity, will result in action. The financial experience of this establishment, for many years past, would seem to demonstrate that the first outlay for such an addition to the means of treatment of those classes of the insane who are now mainly received here, would be all the demand needful upon the philanthropic and liberal of our community.”

3. The last report, from the *Retreat* at Hartford, Connecticut, which we passed under review, was issued during the absence of the Superintendent, Dr. BUTLER, upon a European tour. The one now before us bears his signature.

	Men.	Women.	Total.
Patients in the Retreat March 31, 1853	80	90	170
Admitted in course of the fiscal year	74	103	177
Whole number	154	193	347
Discharged, including deaths	65	96	161
Remaining March 31, 1854	89	97	186
Of those discharged, there were cured	22	42	64
Died	13	9	22

The Retreat was opened on the 1st of April, 1824. For ten years it was under the superintendence of Dr. Todd, six years under Dr. Fuller, and three years under Dr. Brigham. At the date of this report it had been eleven years under Dr. Butler.

Patients admitted in course of the term of Dr. Todd	. . .	520
“ “ “ Dr. Fuller	. . .	481
“ “ “ Dr. Brigham	. . .	246
“ “ “ Dr. Butler	. . .	1,388

	Men.	Women.	Total.
Whole number admitted	1,266	1,369	2,635
Cured	1,331
Died	265

Thus the deaths of *cases*, during thirty years, was 10.05 per cent. But, upon reference to another table, we find that the whole number of *persons* who made up these 2,635 *cases* was but 1,798. Of these 1,798 *persons*, 248 were admitted twice each, 65 three times, 17 four times, 10 five times, 4 six times, 1 seven times, and 1 nine times. Of 1,798 *persons*, 265 died, which is 14.73 per cent. The proportion of cures, upon admissions, was 50.05 per cent. But the same *person* may, in many instances, have been cured two or three times, and, in some instances, four, five, or six times. The report throws no light, even by comparison of tables, upon the number of *persons* cured. This is an imperfection which, as we have heretofore remarked, pervades nearly all the American statistics of insanity.

One further illustration, which we overlooked until the last preceding sentence was written. Dr. Butler states that the percentage of deaths on the whole number discharged since the opening of the institution is 10.82. This is correct, if calculated upon the number of *cases*. But what is the result, if

otherwise regarded? Of 1,798 *persons* admitted, 186 remain in the asylum. Hence 1,612 have been discharged. Of 1,612 *persons* discharged, 265 died, equal to 16.43 per cent.

Of 1,203 cases admitted since March 31, 1845, the age, at the time of first attack, was between 20 and 30 years in 402; between 30 and 40 years in 240. The excess of the former is equal to 66 per cent.

The causes of death in 196 cases, which have occurred since the 31st of March, 1841, were as follows: Exhaustion 36, dysentery 21, general debility 19, phthisis 14, apoplexy 12, general paralysis 12, paralysis 10, erysipelas 10, disease of the brain 9, old age 7, marasmus 7, suicide 7, "disease of lung" 5, epilepsy 4, inflammation of bowels 4, fever 3, internal hemorrhage 3, chronic diarrhœa 3, "injury" 2, disease of heart 2, psoas abscess 1, inflammation of liver 1, disease of uterus 1, acute diarrhœa 1, dropsy 1, cancer 1.

After stating that "neither order of court, certificate of physicians, nor written application of friends or relatives" is required for getting a patient into the Retreat, and that "the admission rests solely upon the judgment of the superintendent," Dr. Butler very properly appeals to the Board of Directors to remove the responsibility from him, and place it upon the friends and the attending physician of the patient. Where are the Connecticut lawyers, judges, and legislators, that such a weak point in the barriers of the rights and liberties of the people has thus long remained unguarded?

The remarks of Dr. Butler upon the condition in which patients come to the Retreat close with this passage, which we earnestly commend to the notice and the memory of every physician in general practice: "Others, worse than all, have been brought here by the ill-judged and most pernicious means of deception, the effect of which has been, in every case that ever came under my observation, both annoying to ourselves and detrimental to the poor sufferer. '*How can I believe you, sir,*' said a gentleman to me, while trying to soothe him, '*when these, MY FRIENDS, have lied to me every mile of my way here?*'"

No apology is required for making the subjoined extract, albeit somewhat longer than we are wont.

"During the six months' vacation which was so kindly granted me by the liberality of your Board, I had the pleasure of being able to visit many of the most prominent lunatic hospitals in England and Scotland. I embrace this opportunity to express my grateful sense of the cordiality and courtesy with which, as the superintendent of one of the oldest lunatic hospitals in the United States, I was everywhere received, and of the frankness and promptitude with which the details of the different institutions were shown. Every door was opened, and every department freely exhibited, evidently giving me the credit of coming to learn the advantages of their institutions, and not to seek for demerits or matters of cavil.

"My reception at some of them was more like that due to an old friend than to a stranger, and was a pleasant recognition of that kindly community of feeling which springs up in every liberalized mind towards those who are fellow-labourers in the same great commonwealth of philanthropy.

"It is evident that, from a variety of causes, a spirit of improvement is pervading these hospitals. A great impetus has of late years been given to this department of human effort, and the most beneficial and gratifying results have been attained.

"It is not expedient, in the narrow limits to which I desire to restrict this report, to go into a consideration of these causes. It is sufficient for my purpose to say that, notwithstanding a few years since our leading institutions were not surpassed by the best of theirs, it is very evident to me that we have now none which will compare with some of those lately erected there. In the older hospitals, there was manifest improvement in the buildings where original defects could never be wholly remedied. In the new institutions, those erected within a very few years, or just now going into operation, I found a beauty of structure, with a thoroughness and perfection of arrangement which I have never seen equalled elsewhere. Among these it will not, I hope, be invidious to mention the asylums at Prestwich and Cheadle, near Manchester;

at Mickleover, near Derby; at Clifton, near York; and the new asylum at Stafford.

"It was evident that in these new asylums no pains nor needful expense had been spared to obtain, in the first place, the most unexceptionable plans. The highest authorities were consulted, and their conclusions referred to the scrutiny of other practical men; the errors of preceding structures were avoided, and every improvement as readily adopted, with the single desire to obtain the best. It is evident that, generally, each succeeding structure contains improvements upon its predecessors. Once adopted, the plans have been carried out without that curtailment and distortion which sometimes, in this country, has produced such unfortunate results. In some instances, it is evident that undue expenditure has been incurred to produce external effect; but in the internal arrangements, especially, it is clear that, while in county asylums everything is plain and simple and unpretending, *that* is deemed in all the best and wisest economy which, in the long run, shall best effect the desired object."

"The chief points of excellence are extensive, well laid-out, and carefully-planted airing-courts and pleasure-grounds, and sufficiency of cultivated land for out-of-door employment; spacious, airy, and well ventilated apartments; the extensive application of steam to every available purpose, cooking, pumping, heating, ventilating, &c., and open fireplaces in every admissible room. The most important of all are the extensive arrangements made for the manual employment of the inmates both within doors and without. There are workshops for the different trades, in some of which these trades had been successfully taught, and in many the amount of work performed showed that the shops were sources of profit to the institution, as well as of beneficial employment to the patients."

"Another feature which struck me most pleasantly was the construction, in several hospitals, of a large and handsome room, especially for the social gatherings and amusements of the patients. My attendance at some of these festival occasions is among the most pleasant reminiscences of my visit. A large amount of profitable out-of-door labour is insisted upon in many, and the amount accomplished in some instances is highly creditable. It is very evident that, if the American institutions are to maintain the comparatively high rank to which they have justly heretofore had claim, a more liberal expenditure than has been adopted in most, in regard to occupation both of body and mind, amusement, &c., must be adopted."

4. The movement of the population of the Lunatic Asylum for the Poor of New York City, for 1853, as given in the report by Dr. RANNEY, was as follows:

	Men.	Women.	Total.
Number of patients January 1st	226	301	527
Admitted in course of the year	226	261	487
Whole number	452	562	1,014
Discharged, including deaths	220	252	472
Remaining Dec. 31, 1853	232	310	542
Of those discharged, there were cured			271
Died	56	59	115

Of those cured 14 were cases of delirium tremens, 2 of febrile delirium, and 7 who were discharged, "recovered," twice each, in course of the year. These being subtracted, the number of cures is 248.

"The ratio of recoveries," remarks Dr. Ranney, "is a little more than 50 per cent. The proportion must depend much upon the length of time the disease has existed previous to admission. Usually, the indigent are placed in an asylum earlier than the wealthy. For this, as well as other reasons, the percentage of recoveries in a hospital of this character should be larger than in institutions devoted to the use of the higher classes, provided the means for effective treatment be furnished."

Causes of death.—Consumption 45, chronic diarrhœa 14, paralysie générale
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13, congestion of the brain 7, marasmus 6, typhus fever 4, typho-mania 4, paralysis 3, apoplexy 3, dysentery 3, delirium tremens 2, epilepsy 2, chronic pleurisy 2, dropsy 2, suicide 1, accidental drowning 1, pericarditis 1, hæmoptysis 1, inflammation of spinal marrow 1.

"It will be seen that consumption is the most common (cause of death). The prominent symptoms of this disease are usually absent where insanity exists. The patient will frequently walk until near the day of his death, and, if there be any cough, it is often so slight as to escape observation."

Of 3,160 patients who have been received since January 1, 1847, no less than 2,381 were foreigners, and but 779 native Americans. The largest number of natives received in any year was 149, in 1847; the smallest number, 94, in 1853. Of foreigners, the number has increased from 280 in 1847, to 393 in 1853. This is accounted for by the increase of immigration. The leading numbers in the table of nativity for 1853 are as follows: Ireland 241, Germany 94, England 19, Scotland 10, Switzerland 5, France 4.

"Very few of the indigent insane of this city are sent to the State Asylum at Utica, and none to Flushing, Hudson, or the Bloomingdale Asylum. Either the ratio of insane is very much less among the natives, or they are kept at their homes. Probably the first supposition is true, and this may arise in part from peculiar causes incident to emigration, and in part from the shipment of the insane from Europe during a lucid interval."

Dr. Ranney, as he intimates, has had uncommon advantages for studying chronic dementia, and he asserts his belief that "by constant training, very many who, if left to themselves, fall into the most miserable condition, would become valuable aids in the asylum, even if perfect recovery did not follow. *

* * If there were an important organic lesion of the brain, no great improvement could be expected; but, *from my examination of this organ in a great number of cases, the proportion in which important lesions were found has not been large. The enfeeblement of the mind depends, in many cases, upon the loss of tone, from inaction. * * * After some acute disease has existed, as mania, this organ becomes exhausted, i. e. loses its tone, and can only be restored by nourishment and the proper mental stimulus.*"

These remarks remind us of the case of a man of more than ordinary intellectual capacity, who was more than fifteen years a patient at Bloomingdale, a large part of the time demented. Attacked with typhoid fever, and removed to the New York Hospital, he died. The late Dr. Swett made a *post-mortem* examination, and found, in the brain, no lesion of importance—absolutely *nothing* whereby to explain the patient's long-continued mental incapacity.

5. In their report for 1853, the Commissioners of the Indiana Hospital for the Insane refer to the resignation of Dr. R. J. Paterson, who had held the office of superintendent from the opening of the institution, and remark that "in his departure he carried with him not only the high esteem of every other officer and attendant of the hospital, but, we trust, of every friend of the unfortunate lunatic in the State." He left on the 1st of June, 1853, and was succeeded by Dr. JAMES S. ATTON.

	Men.	Women.	Total.
Patients in the hospital October 31, 1852	81	78	159
Admitted in course of the year	74	82	156
Whole number	155	160	315
Discharged, including deaths	77	75	152
Remaining October 31, 1853	78	85	163
Of those discharged, there were cured	47	39	86
Died	7	7	14

"While portions of our country have suffered from disease, the Hospital for the Insane has escaped everything like an epidemic. This immunity from intermittents, remittents, and dysenteries, may be attributed to the favourable location of the institution, and to the prompt and rigid enforcement of the sanitary laws for the government of the establishment."

"Already over two hundred insane are knocking at the door of the hospital for admission, and cannot be received for want of room. The institution is crowded to its utmost capacity." The erection of an additional wing is in prospect.

Two suicides, the first which have occurred in the hospital, took place in the course of the year.

From the remarks by Dr. Athon upon the medical treatment of the insane, we make the following extract:—

"Emetics and purgatives are useful in expelling vitiated matters from the stomach and bowels. Conjoined with proper diet and exercise, they may be made subservient in restoring the natural secretions of the alimentary canal. To attempt to make a lasting and beneficial impression on the system by repeating these remedies beyond their aperient or gently evacuant effect, is irrational and highly injurious to the patient. There are too many cases brought to this hospital, exhausted to mere skeletons by the use of the lancet, blisters, and purgatives, to deny, for one moment, this position. By the administration of tonics, and the use of a nutritious diet, a large proportion are restored to physical health. These remarks are made with the hope that our professional brethren who may have charge of patients before sent to the hospital, will keep in view that the *hypercynosis* system can very rarely, if at all, do good in cases of insanity."

We now come to the report for 1854:—

	Men.	Women.	Total.
Patients in the hospital October 31, 1853	78	85	163
Admitted in course of the year	83	86	169
Whole number	161	171	332
Discharged, including deaths	88	84	172
Remaining October 31, 1854	73	87	160
Of those discharged, there were cured	59	55	114
Died	5	8	13

Causes of death.—Typho-mania 3, general paralysis 2, maniacal exhaustion 2, pulmonary consumption 2, scrofula 2, tabes mesenterica 1, erysipelas 1.

"Scarcely any other disease," remarks Dr. Athon, "than what is consequent to insanity, has had a place in our wards, although the adjacent country has suffered much from summer and autumnal afflictions."

It is stated in the report of the commissioners that the great percentage of cures "is, in part, attributable to the selection of patients, receiving those whose insanity is of the most recent date—not filling up the hospital with chronic cases that are hopelessly incurable."

Two festivals were enjoyed by the patients in the course of the year. On May-day "a banquet was given, and the day celebrated with as much parade as is usually manifested upon such an occasion;" and "the second annual celebration, at the hospital, of American independence, was attended, like the first, with the happiest results. Above a hundred patients participated in the festivities of the occasion."

	Men.	Women.	Total.
Patients admitted from Nov. 1, 1849, to Oct. 31, 1854	372	367	739
Discharged, recovered	192	178	370
Died			63

Religious excitement and anxieties is alleged as the cause of the mental alienation in 61 cases, Millerism in 2, and "spiritual rappings" in 29.

"Millerism, in its day," says Dr. Athon, "startled the minds of men and turned them into religious bigots, and thence the transition was easy, in many instances, to insanity. But the spiritual rapping mania eclipses Millerism, or any other *ism*, in its agency to produce aberration of mind. The spiritualists profess to commune with departed spirits, and through their omniscience learn the condition of the dead, look into the future, and do other ridiculous things.

This delusion prevails, in some parts of the country, to a most unaccountable extent, and has been prolific, beyond any other one cause, of insanity."

6. The reports from the Illinois State Hospital for the Insane are *biennial*, that which is about to occupy our attention being for the fiscal years ending on the 30th of November, 1853 and 1854.

The former superintendent having left the institution, he was succeeded, in June, 1854, by Dr. ANDREW McFARLAND, for some years favourably known to our readers as the principal officer of the State Asylum of New Hampshire. The Trustees remark of him that their high expectations in regard to his superior qualifications for the office "have been fully realized."

Patients in the asylum Dec. 1, 1852	82
Admitted in the two years	265
Whole number	347
Discharged, including deaths	181
Remaining Dec. 1, 1854	166

"As the hospital affords accommodation for the two sexes equally, and the whole has been occupied the most of the time recently, the number of males and females has been nearly the same."

Of the patients discharged, there were cured	114
Died	21

"The bodily health of the inmates of the hospital has generally been good. No epidemic has at any time prevailed, although during the past summer (1854) the danger seemed somewhat imminent from the frequency of cases of cholera in the vicinity. The instances of mortality have mainly occurred among those exhausted by long-continued mental disease. The fact cannot be questioned that most forms of mental disease shorten the period of human life. In the exceptions to the rule, in all cases of high excitement, it will be found that lucid intervals occur which allow nature to recover itself before the point of exhaustion is reached.

"Epilepsy, palsy, and consumption will annually claim a class of victims in whose temperament certain predispositions exist. But the natural termination of mania, as it usually occurs, is in a form of disease which has eluded the nomenclature of medical writers. It consists in the failure, *seriatim*, of those physical functions whose healthy performance depends upon a normal innervation. [What functions do *not* require normal innervation for normal performance?] The hesitating step and a difficulty of utterance are among the earliest indications that the brain is losing its supremacy. Calorification fails; the extremities become cold, and the individual is found hovering about the registers or other sources of artificial heat. Soon the circulation becomes enfeebled; the face assumes a swollen and stolid appearance; the extremities swell and become purple, especially if in a dependent position. Digestion becomes involved, and emaciation quickly follows. The legs break out with ulcers which soon become the nucleus of extensive sphacelation, and death is welcomed to close the scene. The individual usually sinks before all the stages in this succession of physical decay have been taken. Science has given no name to this disease, whose aspect is familiar to all who treat the associated insane. It is neither palsy, dropsy, nor marasmus, and yet it combines something of each."

Of the 406 patients who have been received at the institution since it was opened, on the 3d of November, 1851, *only forty-six were natives of Illinois*. The others, so far as known, were immigrants from various States and countries, in the following proportions: New York 47, Kentucky 36, Pennsylvania 35, Ohio 25, Indiana 16, Tennessee 15, N. Carolina 13, Virginia 11, Vermont 7, N. Hampshire 7, N. Jersey 7, Massachusetts 5, Maryland 5, Connecticut 4, Maine 3, Delaware 3, Missouri 2, R. Island 1, Wisconsin 1, Georgia 1, Germany 30, Ireland 25, England 12, Scotland 4, France 3, Sweden 1, Russia 1, Poland 1.

Dr. McFarland thinks that, considering the great proportion of foreigners among the inhabitants of the State, the number of them in the hospital is small. "The Germans," he observes, "are the best, as they are the most numerous, of our foreign patients. They possess a healthy and elastic mental constitution; they are docile and affectionate under treatment, and grateful when they recover."

We know that for many years there has been some discrepancy of opinion among the superintending physicians of our institutions for the insane, in regard to the utility of a committee such as is mentioned in the subjoined extract; and we rejoice that Dr. M. has found it to work so favourably:—

"Another feature in the experience of the hospital for the past year, too interesting and too important to be passed without notice, is the formation, on the part of the ladies of Jacksonville, of a benevolent association, having for its express object a regular and stated visit to the institution on the Saturday of each week, by a committee assigned in rotation. This society, appropriately styled 'The Dix Association,' we regard as a conception of the most happy kind, and its operations have been peculiarly promotive of the welfare and happiness of the unfortunate persons for whose benefit it was instituted. We earnestly hope that the zeal and faith of the society may be sustained, fully believing that its formation is an important era in the history of the institution."

The original plan of the buildings of the hospital at Jacksonville consisted of a central building and four wings, with accommodations for about four hundred patients. But two of the wings have been erected. These furnish apartments for but 168 patients, and the hospital is so much crowded that parlors are being converted into dormitories, undoubtedly to the detriment of the establishment. The question now is, whether the primary design shall be carried out, or a new hospital established in another portion of the State. The Association of Physicians to American Institutions for the Insane have deprecated hospitals intended for more than 250 patients, yet Dr. McFarland advocates the enlargement of that at Jacksonville, by the addition of the formerly contemplated wings, and alleges the reasons therefor, expressing his belief that the special circumstances render this institution an exception to the general rule.

7. The second biennial report of the Trustees and Superintendent of the Missouri State Lunatic Asylum, is the first which has reached us from that institution. We have no information in regard to the precise time at which the establishment was opened, nor any detailed description of it. It appears that it contains seventy-two rooms, each designed for one patient; that it is being enlarged, so as to accommodate about seventy more; and that it is under the superintendence of Dr. T. R. H. SMITH. During most of the period embraced by this report it has been over-crowded with patients, the "usual average" number having been "about one hundred;" and over seventy applications for admission have been rejected.

	Men.	Women.	Total.
Patients in the Asylum Nov. 29, 1852	34	28	62
Admitted in the course of two years	68	55	123
Whole number	102	83	185
Discharged, including deaths	49	42	91
Remaining Nov. 27, 1854	53	41	94
Of those discharged, there were cured	20	23	43
Died	22	16	38

Causes of death.—Epilepsy 11, consumption 6, chronic diarrhœa 4, typhoid fever 4, "ulceration of bowels" 3, paralysis 2, "disease of heart" 2, inflammation of bowels 2, ascites 1, accidental burn 1, "abscesses and gangrenous ulcers at time of admission" 1, exhaustion 1.

"The general health of our household has been very good, excepting during the past season (1854). The intensely warm weather which continued during

the summer months, in connection with the unparalleled drouth, produced an unusual amount of sickness among our patients. The citizens of Fulton, whose healthfulness, heretofore, has been proverbial throughout the State, also suffered greatly from dysentery. The prevailing disease with us was diarrhœa, with a few cases of dysentery and typhoid fever. The attacks were generally very violent in their character, and all of a typhoid type. The fatality, the number of cases considered, could not be regarded otherwise than small."

Dr. Smith, in explaining the apparently, the *really* large mortality, says: "The patients received into this institution, when first opened, and until filled to its entire capacity, with a few exceptions, were those of long standing, who had been accumulating for many years in consequence of the want of provision for their proper treatment. A large number of these were also labouring under incurable diseases associated with their insanity." Such, or similar, has been the experience at a very considerable number of our institutions, and Dr. Smith is not the first who, at the opening of an asylum, has seen a large number of his patients succumb within the first few months.

Of the epileptics, "the majority died during convulsions, and the remainder gradually sunk under the exhausting influence of repeated attacks." The death from "accidental burn," was that of Theodore McGready, "an idiotic boy, admitted in accordance with a special act of the last Legislature. * * * A few weeks before the accident, the weather becoming cold, and in consequence of the unfinished condition of our steam heating apparatus, we were forced to take all our patients from the halls into the stove-rooms in the centre building, to prevent them from suffering. * * * Early in the morning of the day of this sad occurrence, the attendant took Theodore into the stove-room first, and returned for other patients, expecting to be absent only a minute or two. After reaching the hall, he heard him crying, ran immediately to the room, and, upon entering, to his great astonishment, found his clothes on fire. He made every effort to extinguish it, but before it could be done the burn was very severe, extending over his abdomen, his sides, between the lower extremities, the forearms, and hands. * * * He received every attention in our power, through the day and night, but the constitutional irritation was so great that he died the next morning, about twenty hours after receiving the burn. * * * * The manner in which our building is now heated (by steam) precludes the possibility of another such accident."

Whole number of patients since opening of Asylum	193
Married 81, single 100, widows 9, widowers 3	193

Insanity commenced between 20 and 30 years of age in 81; between 30 and 40 in 51. Among the assigned causes of the insanity of the patients are: Miasmatic fevers 25, spiritual rappings 4.

The moral treatment pursued at this asylum is similar to that of other American institutions of the kind, and is already so fully understood as to require no further description in this place. A chapel has been constructed, and a library of 500 volumes collected by gratuitous contribution.

In his discussion of the causes of mental disorders, Dr. Smith makes the following observations:—

"It has been a source of astonishment to many that insanity should prevail to so great an extent in this highly favoured land of ours, and seems to be increasing even in a greater ratio than our population, and is, perhaps, of more frequent occurrence in this than in most other countries of the world. The general impression is that our happy form of government * * * would be incompatible with its prevalence, at least to any great extent. It is true, the elements which enter into the composition of our government, in the abstract, seem well calculated to contribute to man's highest and best interests; yet the freedom of thought and action possessed by every individual connected with this highly-favoured state of things, the high degree of excitement incident to the different pursuits of life; the spirit of emulation; the hopes, the fears, the joys, the sorrows brought into exercise in quick succession—all tend, in a

striking manner, to disturb the equilibrium so essential to the healthy action of the mental faculties; and, by a repetition of the same excesses of feeling, this governing and protecting principle is lost, disease developed, and the mind in ruins one of the sad and fearful results. Is not the conclusion, therefore, justifiable that our form of government, with the habits of our people, is calculated to increase rather than diminish the frequency of insanity, especially when we reflect that the causes referred to are acting upon ill-balanced minds and misproportioned characters, the effects of inefficiency of the intellectual and moral powers with those not favoured with good opportunities in early life, and their misdirection with those who enjoyed better advantages.

"The great practical question, then, is, What must be done in this, our happy country, already the hope and admiration of the world, to prevent, in the midst of so many exciting causes, the most terrible of all afflictions? The answer is, ample provision for, and a radical reform in, the early education of the rising generation; or, in other words, the more careful and philosophical cultivation of the intellectual and moral faculties and propensities of all, in harmony with a correct physical education. Inefficient and misdirected early education constitutes the great predisposing cause to crime as well as insanity; sad combination, truly. The statistics of all hospitals for the insane prove that the great majority of the cases of insanity occur between the ages of fifteen and thirty, and the statistics of crime give us nearly the same results."

We are but little disposed to differ from our friend, Dr. Smith, in the really essential parts of this extract; but we must venture to propose some of the thoughts suggested by the perusal of it. The island of Malta was, by nature, a single mass of rock, almost wholly destitute of vegetation and of soil. Yet the Maltese boasts of his home as "The flower of the world." The Neapolitan exclaims, "See Naples, and die!" (there being nothing more beautiful to be seen.) They of the country of Confucius, who claim that theirs is the "Celestial Empire," say, "We have two eyes, the Europeans have one, and all the other inhabitants of the world are blind." We smile at the simplicity of the Maltese, we do not esteem Naples so much of a paradise as to be willing to die the moment we have seen it, and our people are not so much enamoured with the ocular advantages of the Chinese as to prevent a disposition to expel them from the country. Now let the impartial statesman, or jurist, or philanthropist, read the first of the two paragraphs quoted, and would he be so thoroughly convinced of our happiness, as a people, that, in the fulness of his heart, he would reiterate our expression to that effect, almost at the very beginning of the second? or would he rather pause to reflect whether, after all, we are so *truly* happy as we claim to be? We will not anticipate the decision of the question; for, most certainly, if we are not a people among the happiest in the world, it is not in default of as great a proportion of the means or elements of happiness as has ever fallen to the lot of any nation. Do we employ those means wisely? Do we combine those elements with the skill which is suggested and produced by a profound and just philosophy? Lord Morpeth, now the Earl of Carlisle, after his tour through the United States, declared it as his opinion that no other people on earth possess so many of the comforts of life as the Americans, and among none is there so little happiness. P. E.

ART. XVII.—*A Practical Treatise on the Diseases, Injuries, and Malformations of the Urinary Bladder, the Prostate Gland, and the Urethra.* By S. D. Gross, M. D., Prof. of Surgery in the University of Louisville, &c. &c. &c. Second edition, revised and much enlarged, with one hundred and eighty-four illustrations. Philadelphia, Blanchard & Lea, 1855. Pp. 925, including Appendix and Index.

THE appearance, within four years, of a second edition of this admirable work, sufficiently proves that, notwithstanding its bulk and elaborate character,

a monograph of the kind was wanted and has been properly appreciated by the medical profession.

As the first edition was reviewed at length soon after publication (see No. of this Journal for October, 1851, p. 415, *et seq.*), it will answer our purpose on this occasion merely to call attention to some of the additions and other improvements of the present issue.

The whole work has been thoroughly revised, and thus rendered, as its indefatigable author justly hopes, still more worthy of the patronage and confidence of his medical brethren. His "object has been to afford at least satisfactory, if not full, information upon every subject treated of in its pages;" a task which he has found it impossible to accomplish in a smaller compass than that of the unusually thick octavo volume in which he has presented the results of his extended observations and researches. Large as it is, however, it is entirely too ample in its scope and rich in practical detail, as well as too concise in style, to be regarded with apprehension by the genuine student. No intelligent reader can estimate our author's book as anything but a great good instead of a great evil, however unwieldy in dimensions, while it continues to embrace so little that the earnest inquirer could afford to lose, and so much to instruct and interest every one who may refer to its clear and comprehensive expositions.

Two hundred pages and seventy-eight illustrations have been added in the present volume. These additions "are dispersed through the work in the form of new paragraphs, sections, and chapters, which impart to it somewhat of the aspect of a new treatise."

We have taken a good deal of trouble, in looking over the new edition, to compare it with the old, for the purpose of briefly noting the principal changes which impressed us as most worthy of attention. But the alterations are so numerous and important, and are distributed to such an extent throughout the work, that the attempt to point out even a small portion of them would involve us in a lengthened review, which at this time would be out of place.

It will suffice to say that Prof. Gross appears, if possible, to have exhausted his theme, at least so far as principles and their general applications are concerned; and that he has missed no opportunity, that we can discover, to bring his treatise completely to the level of the present state of progress in respect to everything which he has undertaken to discuss.

Perhaps the most important feature of the new edition, which distinguishes it from its predecessor, is an extremely interesting appendix of some twenty-nine closely printed pages, occupied with a statistical inquiry into the prevalence and etiology of urinary calculi and calculous disorders in the United States and Canada. This is believed by the author to be "the first attempt that has ever been made to collect and systematize our information upon that subject."

"The facts," says Dr. Gross, "which form the basis of this division of the subject have been collected with great care, and comprise, it is believed, an accurate outline of our knowledge upon the subject. However this may be, it is hoped that they will not be without value, and that they will have the effect of awakening further and more extended inquiry into the locality and etiology of a class of affections of so much interest and importance." We are strongly tempted to lay before our readers some of the most curious of the statements which embody the results of this investigation, but we must perforce content ourselves with recommending them, as well as all the other matters so attractively dwelt upon and illustrated in the different chapters, to the serious attention of every medical practitioner who has truly at heart his own best interests, as well as those of his patients, to say nothing of his calling.

"When it is recollected," remarks Dr. Gross, in relation to the topics of his work, "that these affections are of daily occurrence in every community, that they entail a vast amount of suffering, and that they are a frequent source of mortality, it is surprising, nay, positively shocking, to find how much they are neglected by systematic writers and public teachers." We most cordially sympathize with him, in the hope that his contribution, "in its present improved form, will have the effect of arousing the attention of the profession to

a more full and just appreciation of the important and interesting affections of which it treats, and of elevating them, in their estimation, to the same rank as the maladies of the eye, ear, and other organs, concerning which so much has been published in the various languages of Europe." E. H.

ART. XVIII.—*Surgical Reports and Miscellaneous Papers on Medical Subjects.*

By GEORGE HAYWARD, M. D., President of the Massachusetts Medical Society; Fellow of the American Academy of Arts and Sciences; late Professor of Surgery in Harvard University; and Consulting Surgeon to the Massachusetts General Hospital. Boston: Phillips, Sampson, & Co. 1855. 12mo. pp. 452, including index.

OUR readers will recognize, in the neat volume with the above unpretending title, a number of old acquaintances, which they will be glad to meet again in a more permanent and accessible form. Nearly all the papers of which it is composed have been already published in some shape or other, many of them having made their first appearance in this Journal, while most of the remainder have been noticed in our pages. "They are now collected," according to their author, "from a belief that some of them would be useful from the facts and tables they contain, and in the hope that all might be read with advantage by students and the younger members of the profession. It was even thought that they would perhaps be occasionally consulted by those somewhat advanced in practice, whose time was too much occupied to allow an examination of more extended works on the subject of which they treat."

No one who is at all familiar with the personal and professional character of Dr. Hayward and his writings, can hesitate to admit that he has conferred a great favour on his medical brethren, in thus presenting a revised and uniform edition of his previously scattered contributions.

These various essays are especially interesting as the memorials of a well-spent and unusually honoured professional career of nearly half a century's duration. They are still more valuable, however, for their intrinsic merits as the legitimate fruits of the practical good sense and extended reading and experience which so eminently distinguish their veteran and able author, whether as a teacher or practitioner.

The first eighty pages are occupied with an instructive series of clinical reports and comments on surgery and surgical operations in the Massachusetts General Hospital. Then we have an important report on the "Permanent Cure of Reducible Hernia." This paper is particularly deserving of attention, on account of its conclusions. They accord so entirely with the views which a good deal of special inquiry as well as positive experience have impressed upon us, that we are glad to reproduce them here.

"1. That there is no surgical operation at present known, which can be relied on, with confidence, to produce in all instances, or even in a large proportion of cases, a radical cure of reducible hernia."

"2. That they regard the operation of injection by the subcutaneous method as the safest and the best. This will probably, in some cases, produce a permanent cure, and in many others will afford great relief."

"3. That compression, when properly employed, is, in the present state of our knowledge, the most likely means of effecting a radical cure in the greatest number of cases."—Pp. 116, 117.

After this comes a succession of chapters on different subjects, all of more or less interest to the professional inquirer, and some of them of a nature to command the attention of the general reader. Among them, those which struck us as perhaps most worthy of note, were the well known Statistics of Amputations, the article on Vesico-Vaginal Fistula, those on Anæsthetic Agents, on Statistics of Consumption, and on the Contagion of Cholera. We shall not proceed with our selections, because a farther perusal of the book only adds to the difficulty of choice, whilst it tempts us largely to extend our list.

This is not the time or the place for a discussion of the only question broached in relation to the Statistics of Consumption, which is the comparative mortality from that disease, in the cities of Boston, New York, and Philadelphia. It is too serious and difficult a topic to be touched upon in the only manner within the compass of a passing notice. Still, we may venture to suggest one matter of consideration, as likely to have a decided effect upon the relative proportions of fatal cases, although it does not appear to have entered into Dr. Hayward's calculations. How far it ought to be considered, we are not prepared to show; but we are satisfied that it should not be forgotten. We refer to the fact that numbers of phthisis patients annually leave Boston and New York for more favourable climates and localities; and that many of these unfortunates not only withdraw themselves permanently from their original homes, but remain to live or die in Philadelphia. For the same reason, but to a more limited extent, Philadelphia, of course, annually parts with her share of invalids, who go to increase the burthen of mortality in districts farther south.

We are glad to find that, up to the present day, Dr. Hayward, one of the first and most influential pioneers of anæsthesia, continues an advocate of the use of *pure sulphuric ether*, in preference to all other known pain-suppressing agents; whilst he uncompromisingly condemns the resort to chloroform in any of its combinations, and especially in the delusive mixture which has been employed as chloric ether. One of the latest authorities in behalf of chloroform—and perhaps one of the best as to expertness, experience, and entire immunity from fatal mischief in its use—is Mr. Syme, of Edinburgh. This sagacious but rather eccentric operator tells his pupils, in a recent clinical lecture, that the better fortune of himself and colleagues, in the employment of this potent instrument, is due to a more rational mode of administration of it. That they give it “according to principle,” and not “according to rule;” that there are “considerable differences between our practice and that which prevails more or less elsewhere,” and that in this he is only following the example of the discoverer, Dr. Simpson. (*London Lancet* for 1855, p. 200.) We must refer to the lecture itself for the various and doubtless excellent instructions given with regard to the points to be observed in carrying out the plan he recommends. We would feel much more at ease, however, if, in preparing a patient for operation, we could produce a sufficiently analogous result without the necessity of so many minute precautions in order to preserve our patient from constantly impending sudden death. We take leave of our most excellent friend and collaborator with the best wishes for his long continued health and prosperity. We heartily commend his little book to all our readers, and sincerely entertain the hope that we may yet enjoy the benefit of his instructions on the many other topics, still untouched, which he is so well fitted to discuss. E. H.

ART. XIX.—*Difficult Labours and their Treatment*. By M. B. WRIGHT, M.D., of Cincinnati. For which a Gold Medal was awarded by the Ohio State Medical Society. 8vo. pp. 32.

In this essay Dr. Wright confines himself exclusively to a consideration of the proper management of that form of difficult labour in which there is a presentation of one or other of the shoulders of the fœtus at the brim of the pelvis.

Although it is possible, in these cases, for delivery to be effected by the unaided efforts of the uterus, still, in the great majority of instances, the safety of both mother and child requires a resort to manual interference at as early a period as possible after the malposition of the fœtus has been detected.

By the leading authorities in obstetrics of Great Britain, and Continental Europe, as well as of this country, it is taught that the proper management of these cases is to bring down the feet at once, while the uterus is in the most favourable condition to enable us thus to turn the fœtus; but when, from any

circumstance, this cannot be accomplished, we are directed to open the chest of the child, remove the viscera, and extract by the crotchet.

The attempt to bring down the head in a favourable position in cases of shoulder-presentations has, at the present day, but few advocates, excepting, perhaps, in certain exceptional cases of very rare occurrence.

In the essay before us, Dr. Wright has endeavoured to prove that cephalic version, as it has been termed—the changing of a shoulder into a favourable vertex presentation, while it is a preferable procedure to turning by the feet, is applicable to a much larger number of cases than is generally supposed; the opposition to it, on the part of the leading obstetricians of the present day, originating in the improper manner by which it has been attempted to be accomplished, rather than in any difficulty or danger attendant upon the operation when properly conducted.

After referring to a few cases in which the head was very promptly substituted for the shoulder as the presenting part, Dr. Wright considers himself justified in declaring—

“1. That at an early period in labour, and especially if called before the uterus has been deprived of its liquid contents, a shoulder may be converted into a vertex presentation more easily than turning by the feet is ordinarily performed.

“2. That although the membranes may have been long ruptured, turning by the head can be accomplished with great facility.

“3. That delivery by *cephalic version* may be speedily effected, after repeated and ineffectual efforts have been made to turn by the feet.

“4. That *cephalic version* should receive a prominent, nay, leading place, as a means of expediting delivery in shoulder-presentations.”

In regard to the question as to which mode of procedure, turning by the feet or cephalic version, will prove most favourable to the mother, after noticing the disadvantages of podalic version as pointed out by Churchill; namely, the fearful risk incurred of injury to the mother, in consequence of the distance the head has to traverse, and the difficulty of seizing the feet, and of turning the child in utero; after pointing out the fact, as shown by the tables prepared by Dr. Lee, that out of seventy-one cases of shoulder-presentations, in which turning by the feet was resorted to, “seven women died from rupture, and three from inflammation:” three of the cases of rupture occurring in patients under the care of Dr. Lee, where no great difficulty was experienced or force employed in turning, Dr. Wright remarks, that—

“In *cephalic version* the hand does not enter the cavity of the uterus, and, consequently, neither its walls, nor any portion of them, are forcibly pushed out. The fœtus is moved comparatively little within the uterus, the head being already near the superior strait; while in *podalic version*, the part to be first delivered, is most remote from the canal through which it must pass. In the former, the injury to the mother cannot result without great awkwardness on the part of the obstetrician, while in the other we have reason to feel surprised at the escape from injury. In turning by the feet, the hand must necessarily be moved considerably within the uterus, and often while it is contracting violently. In turning by the head there is but little, if any, direct contact of the hand within the uterus. In the one case, contusion of the uterus by the hand is to be expected; in the other case, there is no injury, because there is no contact. Turning by the feet may occasion a severe nervous shock. Not so changing the shoulder for the head.”

Dr. Wright examines next the influence of the two modes of procedure upon the life of the child.

“In describing,” he remarks, “the disadvantages of turning by the feet in all cases, Churchill says: ‘The mortality amongst the infants thus brought into the world is very great. As far as our statistics extend, they yield 174, out of 518 delivered, or 1 in 3.’

“The mortality in shoulder-presentations is, doubtless, greater than this. In the first place, the position of the fœtus weakens its hold upon life. In the second place, the hand is more difficult of introduction into the uterus in shoulder than in head-presentations, and whatever force is required is sensibly

felt by the foetus, and upon that part of the body where pressure is made with least impunity.

"A timely resort to *cephalic* version gives to the foetus almost as much certainty of life as if the presentation had been originally of the head. Why not? The manoeuvre amounts to but little more than in rectification of deviated head positions.

"We are informed by Churchill, that, 'Bush gave an account in 1826, of fifteen cases, in which *fourteen* were born living. In 1827, Ritgen collected forty-five successful cases. Riecke has had sixteen cases.' In all the cases treated by myself from the beginning, the children were born alive. The liability to compression of the chord and consequent death of the foetus, is in proportion to the length of the labour, or rather to the descent of the foetus in the cavity of the pelvis. Hence, to be wholly successful, cephalic version should be performed a short time before, or soon after the commencement of the second stage of labour."

To the question, can any one mode of treating shoulder-presentations be relied on exclusively? Dr. Wright replies in the negative.

"We are disposed," he says, "to adopt the language of Cazeaux, 'that at the present day it would be improper to embrace either practice exclusively; some cases are better suited to cephalic version, while there are others, on the other hand, where pelvic version is alone practicable; consequently both operations should be retained in practice, leaving the judgment of the accoucheur to determine the cases where the one or the other should be preferred.'

"Turning by the feet," according to Dr. Wright, "is to be preferred in cases of inefficient uterine action, or in exhaustion from long continuance of labour; in hemorrhage, convulsions, or in any case in which there may be a demand for speedy delivery.

"Turning by the head should be selected in all cases where difficulty arises from mal-position merely; or in convulsions, hemorrhage, or prolapsus of the funis, if the uterus should be engaged in vigorous expulsive efforts. In rupture of the uterus our great reliance is in artificial delivery; and the question naturally suggested would be, which would guarantee the greatest safety, podalic version, or cephalic version, aided by the forceps? And we should be guided in our action by the answer we give to the question."

Dr. Wright, of course, lays no claim to the introduction of cephalic version as a mode of treating wrong presentations, and expediting delivery; he thinks, however, that the credit of originality may be awarded to him in respect to the means he has suggested for effecting readily and successfully, a change of shoulder-presentations into a favourable one of the vertex.

His mode of accomplishing cephalic version is thus described:—

"Suppose the patient to have been placed upon her back, across the bed, and with her hips near its edge—the presentation to be the right shoulder, with the head in the left iliac fossa—the right hand to have been introduced into the vagina, and the arm, if prolapsed, having been placed, as near as may be, in its original position across the breast. We now apply our fingers upon the top of the shoulder, and our thumb in the opposite axilla, or on such part as will give us command of the chest and enable us to apply a degree of lateral force. Our left hand is applied to the abdomen of the patient, over the breech of the foetus. Lateral pressure is made upon the shoulders in such a way as to give to the body of the foetus a curvilinear movement. At the same time, the left hand, applied as above, makes pressure so as to dislodge the breech, as it were, and move it towards the centre of the uterine cavity. The body is thus made to assume its original bent position, the points of contact with the uterus are loosened, and perhaps diminished, and the force of adhesion is in a good degree overcome. Without any direct action upon the head it gradually approaches the superior strait, falls into the opening, and will, in all probability, adjust itself as a favourable vertex-presentation. If not, the head may be acted upon as in deviated positions of the vertex, or it may be grasped, brought into the strait, and placed in correspondence with one of the oblique diameters.

"It will be observed, that we do not act upon the shoulders by raising them. Perhaps a slight elevation would facilitate the movement already described—

or it might be better to depress them—and, again, by lateral pressure, without either elevation or depression, our object might be accomplished. *Pushing up the shoulders*, therefore, does not constitute a prominent part of turning, if by pushing up is meant the mere raising of the shoulders above the brim of the pelvis.

“As the body of the fœtus makes its curved movement under the hand of the operator, it advances upward, as well as laterally, by a combined rather than a single action, which would give it only one direction.

“The back of the hand, with which we have been acting upon the shoulder, is toward the head of the fœtus—consequently, its hold upon the head would be, apparently, slight—yet, after the shoulders have reached the iliac fossa, the vertex may fall upon the palm of the hand in occupying the strait, and its adjustment become easy. If, however, there should seem to be a necessity for grasping the occiput, there could be no reasonable objection to a speedy change of hands.

“The entire process of cephalic version is to be adopted in the absence of uterine contraction; or, rather, during the intervals of expulsive force. And, as it is now a vertex-presentation, we must be governed, as to the time and manner of delivery, by those general rules applicable to such cases.”

Dr. Wright informs us that in all his cases, excepting one which terminated as a face-presentation, the occiput assumed a position corresponding with the first or second of the vertex. In the exceptional case the occiput was before one of the sacro-iliac symphyses, and to this fact Dr. W. attributes the tendency of the occiput to slide above the brim of the pelvis, and the difficulty of keeping it in place. He thinks, however, that if there had been the usual degree of uterine contraction, the head would in all probability, have become fixed, and the presentation would have continued as one of the vertex, instead of changing for the face.

“In all the obstetrical works we have examined,” remarks Dr. W., “in which cephalic version is recommended by internal manœuvre, it is directed to *raise* the shoulder as the first necessary impression upon the fœtus. Viewed anatomically or mechanically, men have not been persuaded into the belief, that raising the shoulder can facilitate the permanent descent of the head into the superior strait. We maintain, what is apparent to the eye in viewing a proper engraving, and as can be demonstrated with the manikin, that the elevation of the shoulder at the brim of the pelvis, tends to increase the long diameter of the fœtus, and the transverse diameter of the uterus, and without any favourable adjustment of the head after pressure upon the shoulder has been withdrawn.

“Suppose we follow out the directions given by some, and, after the elevation of the shoulder, attempt to force the body of the fœtus in a lateral direction, will not the breech impinge against the walls of the uterus transversely? To enable the head to engage in the superior strait, the body must be entirely moved from it, and this can only be done by raising the breech towards the fundus of the uterus. Raising the shoulder, therefore, is very naturally considered a means to prevent cephalic version. Hence we are not surprised that podalic version is almost universally adopted in the treatment of shoulder-presentations.”

If experience shall prove the views of Dr. Wright, as set forth in his essay, to be correct; if it shall be found that cephalic version, in the manner practised by him, can be as readily effected as he contends, he has certainly made a most important contribution to the practice of obstetrics.

Shoulder-presentations are, happily, of comparatively rare occurrence. Seldom, however, as they are met with, every obstetrician will, nevertheless, rejoice, shall it be found that the risk incurred by both mother and child in the usual mode of managing such cases, by turning so as to deliver by the feet, or by evisceration and extraction by the crotchet, can be obviated by so easy and simple a manœuvre as cephalic version is described to be by Dr. Wright.

D. F. C.

ART. XX.—*Lectures in reply to the Croonian Lectures for 1854, of Charles West, of London, on the Pathological Importance of Ulceration of the Os Uteri.* By HENRY MILLER, M. D., Professor of Obstetric Medicine in the University of Louisville, etc. etc. From the Western Journal of Medicine. 8vo. pp. 71. Louisville, Ky., 1855.

IN his review of Dr. West's lectures, Professor Miller appears to us to have overlooked the leading question discussed in them—the pathological importance of *ulceration of the os uteri*—and he has, consequently, brought a large amount of heavy artillery to bear upon a fortress which it does not appear that Dr. West had attempted to garrison or defend.

Dr. Miller must be aware of the very high rank that has of late years been assigned, in female pathology, to simple ulceration of the neck and mouth of the uterus. There are, in fact, physicians who would seem to refer to it nearly every uneasy sensation, every indication of disturbed health met with in the female, from puberty to the close of life, and whose therapeutics, in the major portion of her ailments, is circumscribed almost exclusively to the local application of nitrate of silver and other caustics, for the purpose of healing the ulcers which, with them, have, in a great measure, been constituted *fontes et origo morborum fœminæ*. While we have another class of physicians who consider these ulcers of comparatively unfrequent occurrence, or, when present, of secondary importance to the more grave diseases of which they are the result. The inquiry, therefore, into what is the actual pathological importance of ulceration of the os uteri would appear to be a very natural and proper one. This inquiry has been undertaken by Dr. West, and the mode in which he has conducted it, and the conclusions at which he has arrived having been presented to the profession, are fair subjects for criticism. If his facts, his arguments, or his deductions are erroneous, the cause of truth and of humanity requires that this should be made known, and a currency given to the exposition of his error or errors co-extensive with that which has been given to the latter. It is important, however, in testing the accuracy of the conclusions at which the inquirer has arrived, and the correctness of the process pursued by him in his investigation of the question at issue, that the real object and scope of his labours should be kept constantly in view, and all impeachment of improper motives and unfairness of conduct avoided. We are sorry to say that this course has not always been strictly observed by Professor Miller in his reply to the lectures of Dr. West.

One would suppose, from the general tenor of the criticism before us, that the inquiry undertaken by the Croonian lecturer for 1854, was into the general pathology of the uterus, instead of being restricted to the pathological importance of *ulceration of the os uteri*.

Professor Miller admits that the manner in which Dr. West has conducted the inquiry which constitutes the subject of his lectures "is courteous and dignified;" and yet he has not hesitated to accuse him of unfairness; of resorting to dishonourable means to disparage, and, "to the extent of his abilities," render contemptible the doctrine and practice of those from whom he differs; of the artifice of mutilating statements to suit his views; of the "vice of dissimulation;" of "an apparent ingenuousness," designed to entrap the unwary reader; of dealing, "throughout his lectures," in logical tricks, etc. etc.

Noticing the different results arrived at by Drs. Lee and West, from their respective examinations of the bodies of females, with the view of determining the frequency of the occurrence of lesions of the os and cervix uteri, Professor Miller asks, "How are we to reconcile this discrepancy between Dr. Lee and Dr. West? How, but by supposing that each found what he sought, and what he judged would be most effective in arresting the progress of the speculum. Prejudice is proverbially blinding in its influence, and under its dominion Dr. Lee, and those who saw for him, may not have seen what was plainly before their eyes; but it may likewise sharpen the sight, as in the case of Dr. West, and enable it to see what is hid from others."

We regret to find such a charge made by a gentleman holding so high and responsible a position in our profession as Professor Miller, and one who, by his talents and industry, has won for himself so fair a reputation. Admit the position he has assumed as correct, and we at once destroy the value of statistics in the settlement of any disputed medical question. And we would suggest that, if there is any weight in the accusation brought against Drs. Lee and West as to the "*einseitig und unrechtig*" manner in which their observations have been made and reported, the same accusation may with equal force be brought against the authority of Professor Miller's statements in regard to the result of his own examinations of the os and cervix uteri, in females labouring under sexual complaints; for we have no more evidence of prejudice on the part of either Dr. Lee or Dr. West, in regard to their views of uterine pathology, than we have of prejudice on his part in regard to his.

Let us have some faith in medical testimony. Though we may be forced to point out, in the statistics furnished us in support of one or other side of a disputed question, their inconclusiveness, in consequence of their limited character; their deficiency in various important points; their irrelevance to the question at issue; or their entire disagreement with others of a more extended, accurate, detailed, or relevant character, let us not doubt the honesty and good faith of those from whom they have emanated—especially when we know them to be distinguished and reputable members of the profession.

We confess that we have failed to detect any evidence of a partisan character in the Croonian lectures of Dr. West; they certainly exhibit much less of a partisan spirit than the lectures in reply of Professor Miller. Dr. West's mode of conducting the investigation of the pathological importance of ulceration of the os uteri may be shown to be erroneous, his reasoning defective, and all his conclusions false; but, from neither the language nor general tone of his lectures, have we any ground for inferring that he undertook the inquiry they are intended to elucidate with any bias for or against the views at which he has arrived, or for any other object than to arrive at truth.

It is not our intention to take any part in the controversy which has given rise to the lectures of Drs. West and Miller. We have made, it is true, quite a number of examinations, with the speculum, of the os and cervix uteri in females labouring under sexual diseases, and in others in whom we were led to suspect some affection of the lower portion of the womb; and in these examinations we have often detected unquestionable inflammation and ulceration, or hypertrophy of its neck and mouth; nevertheless, judging from our own examinations, and those at which we have assisted in patients under the care of other physicians, we have arrived at the conclusion that the frequency of inflammation and ulceration of these parts has been greatly exaggerated. Mere epithelial abrasions have unquestionably been taken for ulcerations, and a relaxed and engorged condition of the cervix for a state of chronic inflammation and enlargement—of the non-dependence of these abrasions and engorgements upon inflammation, we feel fully satisfied. Although of themselves of secondary importance, yet we cannot but view the condition of the cervix uteri under which these most commonly occur, as one demanding the closest attention on the part of the practitioner. We are far, however, from considering it to be one in which the application of the nitrate of silver, or any other caustic, will be found generally beneficial or curative. D. F. C.

ART. XXI.—*On the Construction, Organization, and General Arrangements of Hospitals for the Insane.* By THOMAS S. KIRKBRIDE, M. D., Physician to the Pennsylvania Hospital for the Insane. Philadelphia, Lindsay & Blakiston, 1854. 8vo., pp. 80.

No other class of men in our country, not even professional architects, have so generally made the structure of buildings for the accommodation and treat-

ment of the insane a subject of study, as the superintendents of the several hospitals of that description; and, of these, no one has investigated it with more assiduity, or more thoroughly, than Dr. Kirkbride. A "residence of sixteen years among the insane, in three different institutions, the last thirteen of the time being in immediate superintendence of that with which he is connected, and the care of more than 2,700 patients under very varied circumstances, joined to a familiarity with the defects as well as the advantages of a majority of the American Hospitals," have given him opportunities enjoyed by but few to make himself familiar with all the necessities, both material and moral, to the proper treatment of the subjects of mental alienation. He has not neglected those opportunities, and the results of his researches, observations, and reflections are now before us, in what we venture to pronounce the best and most nearly perfect work of the kind which has ever been produced.

The subject is treated as if this proposition had been laid before the author for demonstration: *GIVEN; two hundred and fifty unhoused insane persons, one-half of each sex, and all of such classes and conditions as are admitted into our State hospitals; that is, part pay patients and part paupers, part curable and part incurable, and including among them all forms and degrees of the disease; that combination of the means and facilities for their treatment REQUIRED which shall be most effective towards the attainment of the ends, primarily and paramountly, of restoration to health of the curable, and the greatest comfort, and bodily and mental improvement of the incurable; and secondarily, and so far subordinately as not to infringe upon the former, the best economy of pecuniary expenditure.*

He has answered it by giving a plan of a building, with illustrations of the architecture, both external and internal; remarks upon the site, the amount of land, supply of water, drainage and inclosures; descriptions of the general arrangements of the buildings, the cellar, materials of the walls, plastering, roof, floors, doors, locks, stairs, windows, bath-rooms, water-closets, sinks, dust-flues, dumb-waiters, speaking-tubes, baking, washing, drying, and ironing arrangements, and many other things which need not here be mentioned in detail.

No subject of importance in the construction of the buildings is left unnoticed in the first part of the essay. The second part is devoted to "Organization and General Arrangements." Here is an exposition of the duties of the Trustees, the Treasurer, the Physician and Assistant Physicians, the Steward, Matron, Chaplain, and Consulting Physicians, as well as of all the subordinate employées properly appertaining to the establishment. Among the other subjects discussed are the following, viz. residence of the physician; hospital furniture; classification of patients; should curables and incurables be separated? separation of the sexes; restraint and seclusion; labour, out-door exercise, and amusements; means of extinguishing fire; supervision of hospitals for the insane; provision for insane criminals; visitors; admission of patients; importance of a correct nomenclature.

An appendix contains the "Propositions relative to the Construction and Organization of Hospitals for the Insane," which have been adopted and issued by "The Association of Medical Superintendents of American Institutions for the Insane."

From what has been said, it may readily and correctly be inferred that, for all who are in any way connected with institutions contemplated in it, this book is full of interesting matter, and that to all who are about to be engaged in the construction of a hospital for the insane, it will be indispensable.

Nor is its scope thus limited. The trustees, managers, architects, and officers of general hospitals, prisons, institutions for the blind, for deaf-mutes, or for orphans—in short, of any large public establishment intended for the accommodation of numerous inmates, may find much in it which will not only interest, but instruct.

The very nature of the essay precludes the possibility of utility from the garbled extracts which might be embodied in so brief a notice as that to which we are limited. An elaborate review alone could do it justice. We commend the work itself to all who might be interested in such a review. P. E.

- ART. XXII.—1. *The Progressive Changes which have taken place, since the time of Pinel, in the Moral Management of the Insane; and the various Contrivances which have been adopted instead of Mechanical Restraint.* By DANIEL H. TUKE, M. D., Assistant Medical Officer of the York Retreat, &c., &c. London; John Churchill, 1854.
2. *The Asylums of Holland; their Past and Present Condition.* By DANIEL H. TUKE, M. D. From the *Psychological Journal*, July 1, 1854.

IN our notice of the report of the Trustees of the Massachusetts State Lunatic Hospital, in the issue for April, 1855, of this Journal, it was asserted that "no man named Tuke was ever Superintendent" of the York Retreat, and that "we never heard, in England, of any Doctor Tuke." The sheets of the Journal were hardly dry, when we received, from their author, the book and the pamphlet, the titles of which are hereto superposed. There is, therefore, a Doctor Tuke in England, and he is connected with the York Retreat.¹ These facts, however, do not militate against the truth of the assertions above quoted. Doctor Tuke is the great-grandson of him whom the Trustees aforesaid "dubbed a doctor;" and when, for several days, we sojourned in the hospitable house of his father, and took notes of our visit to the Retreat, sitting in the invalid chair made for, and, for many years, occupied by the "father of English grammar," Lindley Murray, he was a lad whose greatest cares and griefs were undoubtedly in his satchel, and whose thoughts were less upon a prospective profession than upon the trundling of a hoop, or playing hide-and-seek among the ivy, the laurels, and the other shrubbery of his father's garden.

The "Society for Improving the Condition of the Insane," which was instituted in 1842, by the late Earl of Shaftesbury, annually offers premiums for essays upon subjects relative to mental diseases. The essay by Dr. Tuke obtained the prize in 1854, and was published by the Society. It is a concise but very comprehensive narrative, with succinct descriptions of the means of mechanical restraint which have been employed, and the "contrivances" which have been substituted for such restraints. For all who are interested in the insane, it is fraught with interest; and in no other place can so complete a compend of the history of the reformation of asylums be found.

The first chapter is devoted to an exposition of the moral treatment of the insane, at the time of Pinel, and of the labours of that reformer; the second, to a history of the reformatory progress in France, during the first half century after its commencement.

Chapter third contains a brief account of the gleanings of the author in a tour of observation among the hospitals for the insane upon the Continent, in 1853. Among the prominent men in this department of the medical profession, Professor Schreder Van der Kolk and Dr. Raemer, in Holland; Professor Ideler, and Drs. Jacobi, Damerow, Martini, and Leubruscher, in Prussia; Drs. Riedel and Kestle, in Austria; Zeller in Wurtemberg; Roller in Baden; Klotz in Saxony; and Falret and Voisin, in Paris. Not one approves of the system of non-restraint by mechanical apparatus, in the treatment of the insane. "But," says the author, "on hearing them express their opinion on this subject, I at once inquired into the means employed to restrain their violent patients, and usually found, with real disappointment, that they had by no means systematically introduced those various appliances without which the experiment cannot fairly be tried."

The subjoined remarks were elicited by a visit to the excellent asylums at Vienna and Prague.

"Prejudiced, as an Englishman is, against the political system of Austria, he naturally expects to find her institutions in a bad condition. At least, this was my case; but I am glad to say that, in regard to lunatic asylums, I was

¹ Dr. Harrington Tuke has charge of the Manor House, an Asylum at Chiswick, near London.

agreeably disappointed. There are bad asylums in Austria, it is true; so there are in England; but those recently built are worthy of all praise, not only in their construction and external appearance, but in their management, the condition of the patients, and the high character of their medical officers."

Our author was "struck with the superiority of continental asylums over our own (the English) in one particular—the greater proportion of medical officers to the patients." As proof of this, he cites the fact that at Illenan, in Baden, an institution with 450 patients, there are five resident physicians. We were forcibly impressed with the same superiority over the American hospitals for the insane, and may add to the example alleged by Dr. Tuke, those of Siegburg, Sonnenstein, and Leubus, at each of which there are three physicians to superintend the management of from 150 to 225 patients. At Hall, in Tyrolean Austria, there are also three, and the number of patients rarely exceeds 110. In short, so far as our observation has extended, none of the continental institutions having one hundred patients, are supplied with less than two, sometimes three, medical officers.

The "successive changes in the moral treatment of the insane in England," from 1792 to 1814, form the subjects of the fourth chapter of the essay. Opinions have differed in regard to the question of merit, as pioneer in the reformed method of treating the insane, between Pinel and the founders of the Retreat. Dr. Tuke, with a justifiable delicacy, does not directly allude to this subject, but, quoting from Dr. Thurnam in one place, and condensing from the *British and Foreign Medical Review*, in another, he makes it appear that "it was in the spring of 1792 that the establishment of the Retreat was proposed by the late William Tuke,"¹ and that, "towards the end of 1792, Pinel, after having many times urged the Government to allow him to unchain the maniacs of the Bicêtre, but in vain, went himself to the authorities, and with much earnestness and warmth, advocated the removal of this monstrous abuse." With Thurnam, he appears to think that neither of those men was aware of the action of the other, and that the approximative simultaneity of their enterprises was but "a singular and interesting coincidence." But, as proof of the disinterestedness and universality of his philanthropy, he closes this chapter of strange expositions and interesting undertakings, as follows:—

"All honour to the men, whether in our country or in France, whose warm hearts and enlightened judgments perceived the inhumanity of the system they found, and, relying on the justice of their cause, broke in pieces the iron manacles which confounded the lunatic with the felon, and swept away that mass of inhuman treatment which reduced him to a level with the brute. *'Egregia sane laus! Proclarum enim humanitas atque disciplina de barbarie reportavit vic-toriam.'*"

Chapter fifth continues the history from 1814 to 1839. In the course of this period (in 1823), Sir Alexander Morison commenced his lectures on mental diseases, fifteen new asylums were opened, and, near its close, in 1838, it was promulgated as a principle, by Mr. Hill, of the Lincoln Asylum, that "in a properly constructed building, with a sufficient number of suitable attendants, *restraint is never necessary, never justifiable, and always injurious, in all cases of lunacy whatever.*"²

One of those strange realities of the present day, which to the physician, the philosopher, or any man of the times of the American revolution, would have appeared but as the dreams of an enthusiast or the chimeras of a mental alien, is exhibited in the history of the Lincoln Asylum, in its progressive melioration of the condition of its patients.

In 1829, it was ordered by the Board of Managers: "1. That the heaviest pair of iron hobbles (weight 3 lbs. 8 oz.) and the heaviest pair of handcuffs (weight 1 lb. 5 oz.) be destroyed; 2. That, of the eleven strait waistcoats now belonging to the house, the worst five be destroyed."

¹ Born 1732, died 1832. This grandson, Samuel Tuke, also one of the managers of the Retreat, and father of Dr. Tuke, wrote, in 1813, the "Description of that Institution," which attracted so much attention in all countries where an interest in the welfare of the insane had been developed.

² Hill on Lunatic Asylums, 1838.

In 1830, of the 92 patients, 54 were placed under restraint an aggregate of 2,364 times, and so kept during an aggregate of 27,113 hours.

In 1833, patients 87, restrained 44, times 1,109, total of hours 12,003.

" 1836, " 115, " 12, " 39, " 324.

From March, 1837, all mechanical restraints were discontinued.

The credit of commencing and prosecuting this reform, to 1835, is due to Dr. Charlesworth; the completion of it, to Dr. Hill.

The sixth chapter is a continuation of the history to the year 1854. Dr. Conolly became connected with the Hanwell Asylum, then containing 800 patients, in 1839, and in the course of that year removed the coercion (*douce-ment* "tranquillizing") chairs, "about forty in number," from the wards, and abolished the use of strait waistcoats, hand-straps, leg-locks, and all contrivances confining the trunk, limbs, or any of the muscles. Since that time, the non-restraint system has been adopted at several institutions; but, as late as 1844, there were others in which the condition of the patients was as censurable as that of those in the Bicêtre anteriorly to the labours of Pinel.

Chapter eight includes an enumeration and description of the "contrivances adopted in the place of mechanical restraint."

"We do not hesitate to assert," says the author, "that, under a defective moral system of government, an inefficient superintendent, and attendants not of the highest moral stamp, any asylum discarding mechanical restraint will not only not have gained any real advantage, but will probably have exposed the patients to another, and it is possible to a worse form of coercion. * * Hence, then, of primary importance in the consideration of the substitutes for restraint, is the moral character of the government of the asylum—using the term in its largest sense. With it, the non-restraint system is practicable and beneficial; without it, however high-sounding and grateful to the public ear, the system carries with it a very questionable advantage."

In place of a more detailed notice of the *substitutes*, we give the *résumé* of the author, at the close of the chapter and of the book.

"It must be confessed that when we have obtained all that is to be desired in the superintendence, attendance, and the construction of the building, the particular contrivances are few, simple, and readily applied. They may be thus summed up: The seclusion-room, including the padded room; a strong dress, as of ticking, secured by locks; the manual strength of attendants, in rare cases; cases for blankets, &c. (to prevent them from being torn); and, lastly, cold affusions, topical depletion, and medicines of a depressing character."

Two other extracts from this chapter will be read with interest.

"It is a curious and interesting fact that suicides occur less frequently without restraint than with it."

"We have again and again witnessed the effect of removing a violent patient to a padded room, and have watched, through the inspection-plate, the expression of the patient, and his actions when left to himself, and, as he conceived, unwatched; and we can truly say that, in many instances, the patient who a minute before was in the most frantic condition—kicking, striking, and swearing—was instantly subdued; and in the great majority of cases the same event happened after no very long interval. The entire silence of the room, the absence of excitement, the impossibility of hurting anything or anybody, appear to convince the patient of the utter uselessness of yielding to his impulses."

The present position of the subject of non-restraint, in England, is thus stated, near the conclusion of the seventh chapter:—

"The abolition of restraint in all cases whatever must still be regarded as an open question, although the current of popular and of medical opinion is strong in its favour; but at whatever conclusion the friends of the insane arrive, we are sure that all will agree with the author of the 'Description of the Retreat,' when he says: 'With regard to the necessity of coercion, I have no hesitation in saying that it will diminish or increase, as the moral treatment of the patient is more or less judicious.'"

"Our own opinion of the question is so precisely expressed in the following

remarks, taken from the third report of the Wilts Asylum, that we shall conclude the subject by introducing them here: 'The writer is not of opinion that in no possible case is it justifiable or proper to have recourse to personal restraint, but he entertains a very strong conviction that the officers and attendants in an asylum should be trained to its habitual disuse; and that it should on no account be resorted to by the medical officer in charge, except upon very grave deliberation, and after the failure of all other methods.'"

In a *brochure* of between twenty and thirty pages, Dr. Tuke has produced the most full and detailed description of the asylums of Holland that we have seen, together with their statistics, and a history of the reformation of those institutions.

"The condition of the insane in Holland," says he, "half a century ago, was no less deplorable than in other countries." The first evidence of a movement towards improvement was in February, 1813, when an "Act of King William I. facilitated the admission of recent cases of insanity into the asylums, previously the receptacles for chronic cases only." In 1816 a census of the insane was taken, but led to no measures of melioration. In 1825, another census showed that there were 868 men, 960 women, a total of 1,828 insane, in a population of 2,253,794, equal to 1 in 1,232. "Of these 1,828, 702 were confined in 47 distinct places—prisons, workhouses, poorhouses, hospitals, &c.; only 23 of these abodes being houses specially for the insane, and not one really and truly adapted to their treatment." War with Belgium prevented progress in this sphere, by government, from 1830 to 1833, but "the Directors of the Utrecht Asylum commenced the work of reformation in that establishment, and this attempt may be considered as the *point de départ* from which all subsequent improvements in the Dutch asylums proceeded. * * * * Professor Van der Kolk, the physician to the Asylum, pioneered the way in this noble cause. * * * * He must be regarded as the Pinel of Holland."

In 1838, a third census gave 931 men, 996 women, a total of 1,925 insane, in a population of 2,583,271—equal to 1 in 1,336. In May, 1841, "an act was passed for the purposes of reforming the abuses of asylums, which has been of so much use in achieving its object—that, *in ten years all unfit houses of this description have been abolished*. Two Commissioners in Lunacy, one of them Prof. Van der Kolk, were also appointed with "full power to inspect all asylums, and report on their condition to the Minister of Home Affairs." Of 32 asylums, or "houses," visited by them in 1842, only *three*—Utrecht, Zutphen, and Deventer—were approved. Government immediately ordered a large part of the others to be closed, "while the rest were left to the more gradual influence of the reformatory act" already mentioned. In December, 1850, "only *six* houses for the confinement of chronic cases remained"—number of all their patients, 51. These also will soon be closed.

"In 1850, the population of Holland was 3,056,591; number of asylums, 11; patients, 1,263; insane *not* in asylums, 1,793, estimating the whole number upon the basis of 1 to each 1,000 inhabitants. Professor Van der Kolk thinks this estimate too low, and that 1 to 800 would be more nearly accurate. Asylums with a resident physician, *three*—Deventer, Zutphen, and Meerenberg; provincial asylums, *one*—Meerenberg. Private asylums are not now (1853) allowed in Holland."

Dr. Tuke visited all the principal Dutch asylums at Meerenberg (near Haarlem), of which he gives an elaborate and interesting description; there were 21 males and 25 female epileptics. "Very minute observations are made of the circumstances attending their fits—the temperature, the state of the barometer, the hour of the day, the night, &c. Dr. Everts finds that the fits of the women are more frequent between 9 P. M. and 9 A. M., than at other times, and most frequent at midnight. On the contrary, with the men, the attacks were of more frequent occurrence in the day."

¹ Since 1850, Resident Medical Superintendents have been appointed to the asylums at Utrecht and Dortrecht; and Friesland has opened a provincial asylum at Francher.

"As regards medical treatment, I may just state that general bleeding is very rarely employed; when it is, the cases are usually apoplectic, and not maniacal. Topical depletion by leeches and cupping is much more frequently resorted to, Dr. Everts stating that the latter would be employed at least once daily. (Number of patients, at the time of Dr. T.'s visit, 391.) Tunod's *ventouse monstre* is also considered of great service in deriving the blood from the spinal cord and cerebrum, and in restoring the catamenia, in hysterical mania. Emetics are occasionally found of use."

In 1851, the average resident number of patients was 297; average attending worship, 114; engaged in work, 190; under restraint, 7.

	Men.	Women.	Total.
The number of epileptics was	25	18	43
Number of fits by day (9 A. M. to 9 P. M.)	1,600	2,415	4,015
" " night (9 P. M. to 9 A. M.)	1,372	3,435	4,807

At the Asylum of Utrecht, "during the first thirty years of the century, the mortality, calculated on the admissions, was 35 per cent.; during the last five years, only 17 per cent."

"Dr. Ramaer (of the Asylum at Zutphen) constantly finds a much larger proportion of noisy patients among the women than the men; at present there are nearly twice as many of the former more or less excited. On mentioning this to ———, as a reproach on the gentler sex, she argued that it was natural to suppose women would be *more* noisy than men, when *insane*, because they are so much *less* so when they are *sane*!"

We extract the *aggregate statistics* of the Dutch asylums for the seven years next preceding December 31, 1850:—

	Men.	Women.	Total.
Patients in the asylums January 1, 1844	424	413	837
Of these, there were cured in the seven years	56	44	100
Died	159	165	324
Number admitted in course of the same years	1,590	1,497	3,087
Of these, there were cured	477	523	1,000
Died	474	327	801
Remaining, of both the above classes, Dec. 31, 1850	619	657	1,276
Average number resident during seven years	608	502	1,110
Mean annual mortality, per cent., resident	15.1	13.9	14.5

In a review of the asylums generally, Dr. T. remarks that it is "evident that they are not yet perfect. A great work of reform, however, has been commenced; and, considering the period of time which has elapsed, I think that the progress made in the amelioration of the condition of the insane is highly satisfactory. One noble institution (Meerenberg) has been reared, of which any country might justly be proud; an institution which, itself the indication and result of the reform in Holland, will in its turn act as the nucleus of an extended improvement in the management of Dutch asylums." P. E.

ART. XXIII.—*Experimental and Clinical Researches on the Physiology and Pathology of the Spinal Cord, and some other parts of the Nervous Centres.* By E. BROWN-SÉQUARD, M. D., of Paris, Professor of the Institutes of Medicine and of Medical Jurisprudence in the Medical College of Virginia, etc. 8vo. pp. 66. Richmond, 1855.

THIS, like all of Dr. Séguard's former contributions to physiology, will be found deeply interesting to the pathologist. In his investigation of an important class of diseases dependent upon lesions of different portions of the nervous centres, the present researches will afford him material aid. The object for which they were undertaken is to determine the place of decussation of the sensitive and voluntary motor nerve-fibres in the cerebro-spinal axis.

This question Dr. Séquard has attempted to solve by a series of vivisections, the correctness of the results derived from these being tested by numerous clinical observations, in which the disturbance of motion or of sensibility during life was carefully compared with the lesions of the nervous centres discovered after death.

The generally received explanation of the fact that an alteration of the brain on one side produces a loss or diminution of sensibility and of voluntary movements on the opposite side of the body, is that the sensitive and motor nerve-fibres proceed to the encephalon along the spinal cord without decussating, but that, when they have arrived at the medulla oblongata, they begin to decussate, and continue to do so along the middle line of what is now termed the *Isthmus of the Encephalon* (medulla oblongata, pons Varolii, crura cerebri, corpora quadrigemina, etc.).

This explanation Dr. S. has endeavoured to show to be entirely erroneous, and to prove that the *sensitive* nerve-fibres decussate almost exclusively in the spinal cord, while the *voluntary motor* nerve-fibres decussate mostly in the inferior portion of the medulla oblongata.

The facts developed by the vivisections performed by Dr. S., in proof of a crossing of action for sensibility in the spinal cord, are as follows:—

“1st. If a lateral half of the spinal cord is divided transversely at the level of the tenth costal vertebra, on a mammal, it is found that sensibility is much diminished, and, in some cases, entirely abolished in the posterior limb opposite to the side of the section. On the contrary, the sensibility, far from being lost, appears to be much increased in the posterior limb of the side where the section has been made.

“2d. If, instead of one transverse half section of the cord, two, three, or more are made on the same side, the same effects are observed.

“3d. If, instead of mere sections, a removal of a lateral half of the spinal marrow is effected, the same results are still observed.

“4th. If the lateral section is not complete, and if the part left undivided is in the neighbourhood of the centre of the cord, it is found that sensibility appears to be increased in the posterior limb of the same side, and that in the other posterior limb there is only a slight diminution of sensibility. If the part left undivided is considerable, sensibility does not appear to be diminished in this last limb, and sometimes it seems rather increased.

“5th. If, in performing the section of a lateral half of the spinal cord, the instrument goes a little too far, and divides also a small portion of the other half in the central part, then the posterior limb on the side of the complete section is less sensitive than in the normal state, and the posterior limb of the opposite side loses completely its sensibility.

“6th. If the section of a lateral half of the spinal cord is made at the level of the second or third cervical vertebra, it is found that sensibility becomes very quickly much greater in the parts of the body on the side of the section, and that, on the contrary, the parts on the other side become evidently less sensitive.

“7th. If, after a section of a lateral half of the spinal cord at the level of the eleventh costal vertebra, we perform the section of the other lateral half at the level of the sixth costal vertebra, so that the two lateral halves of the cord are cut transversely, we find that sensibility is entirely lost, or very nearly so, in the two posterior limbs. Sometimes a very slight degree of sensibility remains, more particularly in the posterior limb on the side where the spinal cord has been divided at the level of the sixth costal vertebra.

“8th. If two sections of lateral halves are made as in the preceding experiment, but at a greater distance from each other; for instance, one on the right side, at the level of the eleventh costal vertebra, and the other on the left side, in the cervical region, nearly the same results are obtained as regards the posterior limbs, but the sensibility is increased in the right anterior limb, and it remains, though much diminished, in the left anterior limb.

“9th. If, after having divided transversely a lateral half of the spinal cord in the neck, at the level of the roots of the second pair of nerves, we lay bare the very sensitive nerves going to the ear, in dogs or rabbits, we find that their sensibility, on the side of the section of the cord, appears increased, and that,

on the contrary, on the other side, they appear either destitute of sensibility or very slightly sensitive.

"10th. Sections of a lateral half of the medulla oblongata give, as regards sensibility, the same results as sections of a lateral half of the spinal cord.

"11th. If a longitudinal section be made on the part of the spinal cord giving nerves to the posterior extremities, so as to divide that part into two lateral halves, then it is found that sensibility is completely lost in the two posterior limbs, although voluntary movements take place in them.

"12th. If a similar separation of the two lateral halves of the spinal cord be made on the whole part supplying nerves to the anterior limbs, then we find sensibility is lost in both these limbs, and that it is only slightly diminished in the posterior limbs.

"13th. If the same operation be made as in the preceding experiment, and if afterwards a transverse division be made on one of the lateral halves of the cord, in the part where the longitudinal section has been performed, then we find that the posterior limb on the side of the transverse section remains sensitive, while the opposite posterior limb loses its sensibility."

In confirmation of the general conclusion drawn from the foregoing experiments, that very nearly all the sensitive nerve-fibres coming from the trunk and limbs cross each other in the spinal cord, and that, in consequence, the transmission of the sensitive impressions made upon one side of the body takes place almost entirely along the opposite side of the spinal cord, Dr. S. adduces a series of pathological facts, in which it is shown that an alteration of one side of the spinal cord is attended by a loss of or deficient sensibility on the opposite side of the body. The first portion of the series comprises cases of partial alterations of a lateral half of the cord, more or less conclusively proving the decussation of sensitive nerve-fibres in that organ. The second portion of the series embraces cases of alteration of one side of the medulla oblongata or of the pons Varolii, which appear to Dr. S. to prove positively the decussation in question.

The next series of vivisections were undertaken to test the influence of a section of a lateral half of the spinal cord, and of the medulla oblongata, upon *voluntary* movements. From these experiments, and a number of pathological facts adduced by Dr. S., he is led to admit one of the four following opinions, each of which, he remarks, appears to be supported by some facts:—

"1st. The decussation of *all* the voluntary motor nerve-fibres takes place in the medulla oblongata, where the fibres of the lateral columns cross each other to form the anterior pyramids.

"2d. The decussation of the voluntary motor-fibres takes place partly in the medulla oblongata, but for a greater part in the pons Varolii, and in front of it.

"3d. The decussation of the voluntary motor-fibres takes place in the medulla oblongata and in the spinal cord.

"4th. The decussation of the voluntary motor-fibres takes place together in the spinal cord, the medulla oblongata, the pons Varolii, and the parts in front of it."

The first of these opinions appears to Dr. S. to be somewhat too exclusive; it is possible, he admits, that some few fibres go up to the pons Varolii, or still higher, to make their decussation, but still there are strong reasons which prevent his admitting that such is positively so. It may be, also, that some fibres decussate in the spinal cord itself, although from pathological cases observed in man this would appear not to be the case. Dr. S. believes that if the crossing of the pyramids do not contain *all* the voluntary motor nerve-fibres of the trunk and limbs, the number of these fibres which do not decussate there is but small.

The general conclusions, therefore, of Dr. Séquard, from the experiments and observations referred to in the publication before us, are as follows:—

"1st. The *sensitive* nerve-fibres of the trunk and limbs appear to make the greatest part of their decussation, if not the whole of it, in the spinal cord, and not in the isthmus of the encephalon, as was generally admitted.

"2d. The *voluntary* motor nerve-fibres of the trunk and limbs appear to make their whole decussation, or, at least, the greatest part of it, in the inferior por-

tion of the medulla oblongata, and not in the other parts of the isthmus of the encephalon, as was before admitted.

"3d. According to the seat of an alteration in the cerebro-spinal axis, producing a paralysis, there are three different kinds of paralytic effects, which may exist.

"a. The alteration being in any part of the encephalon, except the inferior portion of the medulla oblongata, the paralysis of voluntary movement and of sensibility exists in the side of the body opposite to the side of the disease.

"b. The alteration occupying an entire lateral half of the inferior portion of the medulla oblongata, at the level of the decussation of the pyramids, the paralysis of voluntary movement exists on both sides of the body, but is incomplete, and the paralysis of sensibility exists only on one side, and that opposite to the side of the disease.

"c. The alteration occupying the entire thickness of a portion of a lateral half of the spinal cord, the parts of the body situated behind it, on the same side, are paralyzed in their voluntary movements, and the corresponding parts on the other side are paralyzed in their sensibility."

According to the investigations of Dr. S., three different forms of paralysis may be produced by an alteration of a lateral half of the cord, and all characterized by the existence of paralysis of movement on one side of the body, and a more or less extended paralysis of sensibility on both sides of the body.

"1. If an alteration, able to produce paralysis, exists in the whole thickness of a lateral half of the cord, in the entire extent of the part from which come all the nerves going to one of the upper limbs, there will be paralysis both of movement and of sensibility in that limb, and paralysis of movement in the trunk and the inferior limb, in the same side of the body, and besides, paralysis of sensibility in the opposite side of the body—limbs and trunk.

"2. If an alteration, able to produce paralysis, exists in the whole thickness and length of the part of a lateral half of the cord, which gives all the nerves going to one of the inferior limbs, there will be paralysis both of movement and of sensibility in that limb, and only paralysis of sensibility in the opposite limb.

"3. If an alteration, able to produce paralysis, exists in the whole thickness and in the whole length of a lateral half of the cord, the symptoms will be a paralysis of movement in the side of the body corresponding to the side altered in the cord, and a paralysis of sensibility in the two sides of the body—neck, trunk, and the four limbs.

"As the sensitive nerve-fibres, coming from one side of the body, have to pass through the corresponding side of the spinal cord to go to the opposite side of this organ, it is easy to understand that an alteration occupying the entire thickness of the cord is able to produce a paralysis of sensibility, together with a paralysis of movement, in the same side of the body. But it must be remembered that if, in such cases, the paralysis of movement exists in all parts of the body receiving their nerves from the altered part of the cord, and also from all the healthy parts below it—if there are any—the paralysis of sensibility, in the same side, remains limited, almost entirely, to the parts which receive their nerves from the altered portion of the spinal cord."

Dr. S. presents a list of certain pathological and other facts which appear to be opposed to the deductions he has drawn from the facts and reasonings he has presented. These are arranged under the following heads: 1st. Alleged voluntary movement and apparent existence of sensibility in children who are to all appearance deprived of the cerebro-spinal axis. 2d. Alleged voluntary movements and apparent existence of sensibility in parts of the body considered as deprived of their natural connection with the encephalon. 3d. Alleged persistence of sensibility and voluntary movements in men and animals deprived of all parts of the encephalon, except the medulla oblongata and pons varolii. 4th. Cases proving that considerable alterations of the pons varolii and medulla oblongata may exist without producing paralysis either of sensibility or of voluntary movements. 5th. Cases in which an alteration in the two sides of the pons varolii appeared to have produced a paralysis only in one side of the body. 6th. Cases in which an alteration existing in one side of the

pons varolii, or in the neighbouring parts, appears to have produced paralysis in both sides of the body. 7th. Cases in which an alteration in one side of the pons varolii, or of the neighbouring parts, has produced paralysis in the same side of the body. 8th. Cases and experiments which appear to prove that there are, in various parts of the encephalon and in the spinal cord, motor nerve-fibres which are not voluntary motor. 9th. Anatomico-pathological dissections—as those of Dr. L. Türck, of Vienna—which appear to prove that there are nerve-fibres coming from the spinal cord which decussate in parts above the medulla oblongata.

After a careful examination of these several class of facts, Dr. S. presents the following as his conclusions in regard to them:—

“1st. That reflex movements alone, and not sensations and volitions, exist in monsters deprived of a great part of their cerebro-spinal axis.

“2d. That, when the spinal cord, the medulla oblongata, or the pons varolii are altered, even considerably, sensibility and volition may continue to exist, because there are still communications by nerve-fibres through the altered parts, between the nerves of the trunk and limbs and the parts of the encephalon, in front of the pons.

“3d. That, if the reasons given by many physiologists to prove that the pons Varolii is the seat of the centre for volition, and for perception of sensitive impressions, were true, we should have to admit that the medulla oblongata is the centre—or, at least, a part of the centre—for these faculties, because the same reasons appear to prove so for this organ as for the pons.

“4th. That very likely these faculties have not their centre, at least their principal centre, in the pons Varolii, and still less in the medulla oblongata.

“5th. That it seems that, in some men, the sensitive and voluntary motor nerve-fibres do not decussate at their usual place, and that, in consequence, some rare cases may exist in which an alteration in one side of the pons Varolii, or of the medulla oblongata, will produce paralysis of the same side of the body.

“6th. That there appears to be, in many places of the encephalon, nerve-fibres which are not voluntary motor, and which, nevertheless, go to muscles either on the same side of the body as that of the encephalon from which they originate, or on the opposite side, and that these muscular nerve-fibres are able to produce convulsions when they are irritated by an injury or an alteration in the encephalon, so that convulsions may take place either on the paralyzed side or on the other.

“7th. The results of the researches of Dr. Ludwig Türck cannot, in the actual state of science, prove against or in favour of any doctrine relative to the place of decussation of sensitive and voluntary nerve-fibres.”

As this most interesting work of Dr. Séquard may not fall into the hands of a number of our subscribers, we have endeavoured to give them a pretty full account of the results to which the author has been led by his researches on the physiology and pathology of the spinal cord and other portions of the nervous centres. Though in opposition to generally received opinions, their importance, and the close and logical manner in which the investigations upon which they are based have been conducted by Dr. S., press strongly upon our attention his conclusions in respect to the place of decussation of the sensitive and motor nerve-fibres.

D. F. C.

ART. XXIV.—*Observations on Yellow Fever, and its Relations to Quarantine and other Hygienic Measures.* By S. L. GRIER, M. D. 8vo. pp. 41. New Orleans, 1854.

IN relation to many questions, medical opinion would appear to move not forward, but in a circle, and hence we have the constant recurrence of doctrines which were once current for a period, and then repudiated for others of perhaps a precisely opposite character. In nothing is this more strikingly

shown than in the views entertained in regard to the etiology of yellow fever. The doctrine of its invariable importation into Europe and into this country by fomites or patients sick of the disease; of its absolute contagion under all circumstances, or its propagation by fomites, or a specific morbid emanation from the bodies of the sick, under certain conditions of impurity and elevation of temperature in the atmosphere of particular localities, which very generally prevailed during the latter portion of the eighteenth century, and which most of us supposed now almost entirely abandoned by the leading members of the profession both at home and abroad. When we had believed that the opposite doctrine of the local origin of the disease, and its non-contagious character, was become so clearly established by an overwhelming mass of well-authenticated facts as to be very generally received, we find efforts being made in more than one direction to discredit the latter and revive the former.

To effect this, and in the train of the old hypothesis to subject us to all the expense, discomfort, and folly of a system of rigid quarantine, is the object of the publication before us.

Dr. Grier maintains that the yellow fever is not of American origin; that its source can be traced to the eastern continent, in the same manner that we trace epidemic cholera to its nativity in remote India. He admits, nevertheless, that it is now *naturalized* or endemic in certain parts of inter-tropical America. The exact limits of this yellow fever zone, north and south, have, however, not yet been determined.

Dr. G. believes it possible for the yellow fever to prevail in New Orleans either *sporadically*, that is, as "an occasional disease, occurring without any general or prevailing cause;" or *endemically*, that is, as a disease peculiar to the inhabitants of the place—a disease native to the locality; or *epidemically*, that is, as a disease which, although prevalent among the inhabitants of a city or country, is not necessarily native in that locality, and may be extended to other places either neighbouring or remote.

Dr. G. would seem to make a distinction, not in the cause or the more general diffusion of the cause, between yellow fever as an endemic and yellow fever as an epidemic, but in the very nature of the disease itself. Thus, while he admits that by naturalization it has become endemic in—in other words, a disease of New Orleans and other American cities, yet he asserts that

"Yellow fever, *in its epidemic form*, does not originate in the cities of the United States where it has hitherto prevailed, but has *always* been introduced either in the hold of vessels coming from infected ports, in goods, or by subjects of the disease.

"The epidemic virus thus conveyed to our shores is not extended and diffused through the medium of the atmosphere, except to a very limited extent, differing in this respect from a class of epidemics of which *influenza* may be offered as an example, but that it is more allied in its mode of propagation and manner of progression to another class of epidemics which are known to be contagious in their character, of which class *scarlet fever* may be cited as an instance."

Dr. G. includes yellow fever in that class of contagious diseases "which are transmitted from the sick to the well through the medium of the atmosphere immediately surrounding the patient, but only under certain conditions, by virtue of which the atmosphere is rendered more liable to be the conductor of the specific virus."

These "certain conditions" are high ranges of temperature, and that form of malaria produced by undue moisture, marsh effluvia, filth, defective ventilation, and over-crowded population. None or all of those conditions, however, are, according to Dr. G., capable of producing the yellow fever. The combination of a foreign or external cause—the virus of the disease—with these local conditions is, he maintains, essential to its production and propagation. Hence he maintains that yellow fever, *as an epidemic*, is an imported disease—"the *foreign element* which is prerequisite to its production being brought to our shores by the ships of commerce, but is incapable of engendering the disease in this form unless the domestic conditions necessary to the propagation of an epidemic influence are present also."

Now, unless Dr. G. supposes that there are two distinct forms of yellow fever, the endemic and the epidemic, he has admitted too much for the safety of his unqualified dictum that epidemic yellow fever has always been introduced from without into every city of the United States where it has hitherto prevailed. In certain of these cities he informs us that the disease may occur either sporadically or as an endemic; what, then, according to his own showing, is to prevent the disease from prevailing epidemically whenever the malaria, the heat, and the other local elements necessary to the propagation of an epidemic influence, are present also?

We shall not adduce, in refutation of the unqualified dictum of Dr. G. of the invariable importation of yellow fever into the United States, the well-authenticated instances of the occurrence of the disease in our midst, in years when no vessel had arrived, for many months previously, from any port at which the disease prevailed, nor the numerous instances in which the disease has broken out on board of vessels in the midst of the ocean, thousands of miles from land—vessels that had sailed from a healthy port, with a healthy crew; nor still, the fact that neither the most rigid seclusion nor the most rigorous quarantine has always afforded protection against the disease, while, in some instances, it would appear to have increased the certainty of an attack: we shall not adduce these facts, because Dr. G. may be inclined to answer them by asserting that in neither of the circumstances referred to was the yellow fever epidemic, and reminding us that the views he has advanced have relation solely to the disease in its epidemic form.

Dr. G. has presented a show of evidence in support of his opinions, of which it is not our intention, on the present occasion, to enter into an examination. All of it having a direct bearing upon the leading points discussed in the essay before us, has been carefully weighed by those fully competent to the task; and shown to be altogether inadequate to sustain either the importation of yellow fever or its propagation by contagion.

By the General Board of Health of Great Britain, all the facts connected with the origin and propagation of yellow fever have been carefully examined and collated. The conclusion at which the Board have arrived, and which they announced to Parliament in their second report, is—

“That there is no evidence to prove that yellow fever has ever been imported.

“That, consequently, the means of protection from the disease are not quarantine restrictions and sanitary cordons, but sanitary works and operations, having for their object the removal and prevention of the several localizing conditions, and when such permanent works are impracticable, the temporary removal, as far as may be possible, of the population from the infected localities.”

When we reflect that those conclusions are drawn not from a few isolated facts, but from a mass of testimony extending through the history of the yellow fever from 1793 to 1851, and derived from the most impartial observers who had ample opportunities of investigating every point connected with the etiology of the disease, we cannot but acknowledge their force; more especially, when such men as Edwin Chadwick and T. Southwood Smith, by whom the report alluded to was drawn up, assure us—

“That from the most careful examination which we have been able to make of the mass of evidence submitted to us, from which the foregoing conclusions have been deduced, we have not found a *single fact* or *observation* clearly ascertained, and authentically recorded, opposed to the general tenor of such evidence. We have met with no exceptional cases. We have, indeed, found the opinions of some authorities, for whom we entertain great respect, not in accordance on some points; but these have reference, for the most part, to matters of a purely professional and scientific nature. On the great practical question, whether, whatever may be the nature and mode of propagation of yellow fever, quarantine and sanitary cordons can afford any real protection against its introduction and spread, we believe there is now a very general unanimity of opinion that, in accordance with the evidence we have submitted, they cannot. We believe there is the like general agreement in this further practical conclusion, that the substitution of sanitary or hygienic measures for quarantine isolation and restriction, would afford more certain and effectual protection.” D. F. C.

ART. XXV.—*Illustrated Manual of Operative Surgery and Surgical Anatomy.*—By MM. C. BERNARD, D. M. P., and CH. HUETTE. *Edited, with notes and additions, and adapted to the use of the American Medical Student*, by W. H. VAN BUREN, M. D., Prof. of Anatomy, University Medical College, and Surgeon to New York Hospital, and C. E. ISAACS, M. D., Demonstrator of Anatomy, Univ. Med. Coll., and Consulting Surgeon to the Penitentiary Hospital, Blackwell's Island. *Illustrated with steel engravings from drawings after nature*, by M. J. LEVÉILLÉ. *Designed to serve as a companion to the ordinary text-books of Surgery.* New York: H. Baillière, 290 Broadway, 1855. 113 plates, and 513 pages.

THE first half of this well-known and popular manual has been already three years before the profession, and was duly recommended to the notice of our readers in 1852. We take great pleasure in announcing the long looked-for publication of the remaining parts, and in expressing our admiration of the whole work in its present complete and elegant form. It is unquestionably the handsomest compendium of Operative Surgery that has yet appeared; and, withal, so compactly as well as clearly and comprehensively arranged, that we are inclined to think that it may prove even more useful than it is ornamental. The American editors have good reason to congratulate themselves on having successfully supplied a want which has hitherto continued to be felt, in presenting the beautiful and accurate illustrations and methodical accompanying text of MM. Bernard and Huette, as "a complete and concise picture of the science and art of Operative Surgery, in its present advanced and perfected condition, in a portable form."

They do not place their translation in competition with the great work of Bourguery and Jacob, or with the American edition of Velpeau, or the quarto of Prof. Pancoast. We would decidedly prefer it to either of these (except the first, which is altogether out of general reach), as much more recent, and, for most practical purposes, both more available and more reliable in the hands of the student or inexperienced practitioner. "Its compactness and portability will render it," as they justly say, "more desirable to the student as a companion in the lecture and dissecting-room, where its copious and graphic illustrations will assist him materially in acquiring correct general ideas as to the nature and objects of the individual operations of surgery; whilst for more minute and varied details with regard to their history and numerous modifications, the less accessible and more expensive treatises alluded to can be consulted at a more advanced period of study." "Our manual," they further intimate, "whilst it is intended mainly to illustrate the intricacies of operative surgery by appealing to the eye as well as to the understanding of the student, and by familiarizing him with that most useful department of anatomy which immediately relates to surgical operations, will also be found, it is hoped, not entirely useless as a work of reference to those already engaged in practice."—(*P. 6 of Preface.*)

The first twenty-five plates are preliminary, and present, in a large number of remarkably well-executed and colored figures, all the most approved instruments required in surgical operations. These instruments appear to have been selected with discriminating care, although in great abundance and variety. As a matter of course, they are mostly French, and after the patterns of Charrière.

Next in order come six other plates, also introductory in character, occupied with the usual minor or elementary operations; such as the positions of the bistoury, incisions, union of wounds, seton, venesection, arteriotomy, ligature of arteries, and other manipulations of this class.

These are followed by an admirable series of illustrations of the surgical anatomy and operative surgery, side by side, of the ligature of the different arteries. This important subject is here presented in ten plates and some thirty figures, which are not excelled in beauty, accuracy, and practical value by any in the book.

Amputations through the joints, in the continuity of limbs, exsections, and trepanning are severally and successively exhibited in the eighteen subsequent plates; ample demonstrations of the surgical anatomy accompanying, throughout, the delineations of the critical stages of each operative procedure. These are succeeded by attractive representations of a great variety of delicate operations on different organs and regions of the body, which we have not room to specify. Among them we may note especially those upon the eye, upon the ear and other portions of the head, and upon the throat, neck, chest, and abdomen.

Then we have the management of abdominal and intestinal wounds, the surgical and pathological anatomy and surgery of hernia and of artificial anus, and the operations upon the anus and rectum. Plates 60, 61, 62, and 64, on the anatomy of hernia, are in every point of view deserving of particular attention. Were it possible to select, from the great number of rich and appropriate engravings, three, or four, or half a dozen that might be regarded as embodying and portraying the distinctive excellences of the whole work, we would be disposed to point to these pictures of hernia. They are masterpieces of their kind, if not decidedly the best specimens of miniature illustration that MM. Bernard and Huette, or any others, have produced.

The remainder of the volume is taken up with operations on the male genito-urinary organs, including those for stricture and its consequences, and for urinary calculi; with operations on the female genital organs, and, lastly, with tenotomy.

The foregoing brief sketch and outline of its principal contents will afford a faint idea of the character and value of the book before us. The standing of the French original, as that of the most convenient and attractive elementary atlas extant of operative surgery, while it is inferior to none in the fidelity of its teaching, has been so long established that we are spared the necessity of dwelling on it here. It is justly regarded by a Parisian authority as "at once a work of science and a work of art." "Science," says this writer, "is exemplified in its perfect acquaintance with surgical anatomy, and above all in its methodical exposition and judicious choice of the innumerable procedures of operative medicine; whilst art shines in each plate with a characteristic grace and finish which in no way impair the exactness of its details."—(*Archives Générales*, 1855, p. 333.)

The engravings of the American edition are evidently duplicates of the original copies, and hence identical with them in artistic superiority. The letter press is, as already intimated, well arranged, clear, concise, and sufficiently full for the purpose intended. It appears to have been faithfully and elegantly translated; and, according to the promise of the editors, is agreeably free from idiomatic and un-American phrases and technicalities. It cannot fail, therefore, to be sought after by intelligent students as a cherished adjunct to the various text-books of the schools; and will be no less likely to afford, even to teachers and practitioners, an amount of aid and comfort that cannot be hoped for from any other work of similar character.

The additions of the editors, which are most unobtrusively appended, are judicious and appropriate. Although not very numerous or lengthy, some of them are important in connection with American practice; and all are interesting, and such as decidedly add to the value of the work without interfering with the previous context, or perceptibly increasing the size of the volume. The publishers have been equally successful in the paper and printing, and the general getting up; so that we may venture to presume that their transatlantic *confères* will have no reason to be ashamed of the American reprint. With the best wishes for the success of a publication which does the highest honour to the good taste, enterprise, and judgment of all concerned, we most cordially recommend it as on all accounts worthy of a prominent place in every medical library, or on every office table.

E. H.

ART. XXVI.—*The Pathology and Treatment of Leucorrhœa*. By W. TYLER SMITH, M. D., Member of the Royal College of Physicians, Physician Accoucheur to St. Mary's Hospital; Lecturer on Midwifery and the Diseases of Women, in St. Mary's Hospital Medical School; Vice-President of the Medical Society of London; Honorary Fellow of the Obstetrical Society of Dublin, etc. etc. 8vo. pp. 199. Philadelphia, 1855: Blanchard & Lea.

THERE is great truth in the remarks with which Dr. Smith commences the present treatise, which is to be viewed as the more full development of the leading points in reference to leucorrhœa and its allied disorders, advanced in a memoir presented by the author to the Royal Medical and Chirurgical Society of London, and published in the thirty-fifth volume of its *Transactions*.

"Few topics," he very justly observes, "have been more discussed, during recent years, than those relating to the pathology and treatment of disorders of the uterine organs attended by discharges. But it must be confessed that discussion has expended itself chiefly upon verbal criticism, and contributed little towards the more clear comprehension of this department of medicine, there has been much of argument, but, as I submit, little of rigorous examination. Tongue and pen have been plied with remarkable assiduity, yet the difficulties surrounding the subject have been rather increased than diminished. On many points of diagnosis and pathology, apparently the most simple and easy of solution, the greatest uncertainty still prevails. This uncertainty naturally extends itself to the subject of treatment, and shows itself at every turn in practice. The various lesions, real and supposed, of the os and cervix uteri—ulceration, induration, and inflammation—have been attacked or defended by their partisans and opponents with the hottest zeal. In the diagnosis of these affections, some have practised instrumental examinations to an extent hitherto unprecedented in this country, while others have condemned such examinations altogether. As regards treatment, we see at one time injections, at another pessaries, at another cauterizations, are assailed with the utmost vigour, leaving the conscientious practitioner bewildered and uncertain as to what are really the best methods of controlling the confessedly troublesome and prevalent maladies, for which these and other appliances are in turn vaunted or anathematized."

To acquire, if possible, more correct and definite views in regard to the pathology and proper treatment of a class of diseases, of the character and management of which, though much has been said and written, little is actually known, Dr. Smith, aided by the microscope, has investigated anew the nature and structure of the several tissues of the vagina and os and cervix uteri, the secretions of these parts in the healthy state, and their several morbid conditions, from that attended by a simple augmentation of the normal secretions, onward, through the several stages of abrasion and simple ulceration of the vagina, and of inflammation, abrasion, ulceration, induration, and hypertrophy of the mouth and neck of the uterus.

This investigation is one replete with interest, and has been conducted in a systematic and skilful manner by Dr. Smith. Although the correctness of some of the positions laid down by him may admit of dispute, he has nevertheless succeeded in furnishing the foundations for a more correct pathology of leucorrhœa than has hitherto existed, and for a more rational and successful treatment of the several morbid conditions of the uterus and vagina, in connection with which leucorrhœal discharges are liable to occur.

"If, as I have previously shown," says Dr. S., "great discrepancy of opinion has prevailed respecting the sources of the healthy secretions of the vagina and canal of the cervix uteri, there has been still greater confusion as regards the seat and constitution of the morbid utero-vaginal discharges. No one had inquired minutely into the nature of these discharges, so that current opinions upon the subject have had no better foundation than guessing and hypothesis. Let any one who doubts the correctness of what is here advanced, examine for himself the doctrines hitherto taught respecting leucorrhœa, and he will find

that some refer to the vulvo-vaginal glands as the chief seat of leucorrhœal discharges; that others refer to the vagina as a great follicular tract from which the principal amount of these discharges proceeds; that others, again, look to the cavity of the fundus uteri and its mucous lining as the great source of uterine mucous secretions. As regards the causes of these discharges, some have limited their attention to the sexual organs, while others have looked to the conditions of remote parts of the body, for the explanation of leucorrhœal disorders. It would, indeed, be easy to fill a volume with the discordant accounts which, in the absence of a knowledge of the minute anatomy of the parts involved, have been given of the nature and source of leucorrhœal discharges. One or two authors only have referred to the canal of the cervix uteri as the principal seat of mischief in leucorrhœa; but their teachings have been uncertain and without proof, since no one, so far as I am aware, ever made a positive and minute examination into the subject, or recognized to the full extent the glandular organization of the cervical canal. No pathologist has hitherto formed anything like a just appreciation of the parts borne respectively by the vagina and the os and cervix uteri in the production of leucorrhœal discharges. Effects have been constantly mistaken for causes, and secondary phenomena have received the importance due to those which are primary, while in practice the most important structures have frequently escaped attention altogether. The consequence has been that some have recommended the most violent measures of treatment, while others have rejected all remedial measures except the most simple and inert. Meanwhile, this department of medicine has witnessed a contest which for virulence and acrimony has seldom been equalled in the profession."

According to the researches of Dr. S., the mucus secreted by the glands of the ostium vaginæ, in the absence of excitement, is so inconsiderable, or is so mixed up with the scaly epithelium of the mucous surface of this part, that it is extremely difficult to ascertain precisely its microscopical qualities. In some women, a profuse emission of fluid appears to take place from these glands during sexual intercourse. Like the other vaginal secretions, this mucus has an acid reaction.

"The mucus of the vaginal canal is not found in any considerable quantity in the healthy subject; it is only secreted in sufficient quantity to keep the mucous surface in a state of lubrication. It lies upon the mucous membrane as a milky fluid, containing small curdy points or masses, and consists of a transparent or semi-transparent plasma, containing an abundance of scaly epithelium and its *debris*." "The plasma of the vaginal mucus appears, when first secreted, to resemble the plasma of the cervical mucus; but it is less viscid and tenacious. It is only after it has lain a short time upon the vaginal surface that it becomes curdled. The vaginal mucus is, as M. Donné first remarked, distinctly acid, and it is to the effect of the acid in coagulating the albumen of the mucus, and not to the presence of epithelium, that its curdled appearance is attributable."

"As regards secretion, the vagina is always pretty much in the same condition, except that the acidity is constantly increased during pregnancy; but the cervix uteri has to pass through various physiological changes during the performance of the functions of menstruation, pregnancy, parturition, and lactation. It becomes necessary, therefore, to consider the secretion of the canal of the cervix in these several states.

"In the *unimpregnated condition*, when the cervix uteri is found perfectly healthy, little or no discharge is seen issuing from the cervical cavity; but when the labia uteri are separated, the canal of the cervix appears to be full of its peculiar secretion. In examinations after death, in cases in which the uterine organs are in a healthy condition, the mucous crypts and the canal of the cervix are generally found filled with a clear, viscid mucus, so as to entirely block up the passage from the vagina to the cavity of the fundus. This appears to be the normal condition of the cervical canal, in the unimpregnated state. At each catamenial period, the whole of the tenacious plug of mucus must be washed away by the menstrual fluid, as the latter may be seen escaping freely from the os uteri at these times; but, in a few days after the completion of the

period, the mucous plug is again formed. When first secreted, the cervical mucus is less thick and viscid than it afterwards becomes. Thus, it would seem to be the function of the glandular structure of the cervix, in the unimpregnated uterus, to secrete, each month, a sufficient quantity of viscid mucus to fill the canal of the cervix, the mucous follicles becoming comparatively inactive when this has been accomplished, until after its removal at the next flow of the catamenia. The function of the cervix is, therefore, in a certain sense, like that of the fundus, periodical; and we shall see hereafter that this periodicity is discernable in the diseased conditions of the cervix and its secretions. In healthy subjects, the canal of the cervix is always full in the intervals between the menstrual periods, though there certainly seems nothing like a constant flow of the cervical mucus into the vagina. Just enough is secreted to fill the canal. The mucus itself consists of myriads of mucus-corpuscles entangled in a transparent viscid plasma. The plasma is so tenacious that the mucus-corpuscles are found to be arranged in strings when placed under the microscope, and individual corpuscles are frequently seen to be elongated from the same cause.

"The use of the cervical mucus is probably twofold. In the first place, it closes the cervix uteri, and defends the cavity of the fundus from external agencies as completely as though it were a shut sac. In the second place, it appears to afford a suitable medium for the passage of the spermatozoa through the cervix uteri into the uterine cavity."

"After the commencement of *pregnancy*, the periodical functions of the uterus cease, and, in the generality of cases, the plug of viscid mucus, when it is once formed, continues for the most unremoved until the commencement of labour." "Generally, during gestation, the lowest part of the plug is to a slight extent constantly wearing away, and is discharged in the form of *debris* into the vagina; but the secretion from the cervix goes on only to such an extent as to keep the os and cervix closed. In other cases, the secretion is more profuse, but the cervix is still kept full by an increased secretion from the glandular structures. The mucous plug formed during pregnancy is firmer than the mucus filling the cervix in the intervals between the monthly periods in the unimpregnated state, particularly at its lowest part, where it is perfectly white and opaque. In the upper parts of the cervix, it is clear and transparent. The plug consists, in the upper part of the cervix, entirely of mucous globules and plasma; but in the lower portions of the plug these elements are mixed with scaly epithelium in considerable quantity."

During *parturition*, the canal of the cervix secretes a quantity of mucus having a more fluid character than the plug of pregnancy. This secretion continues throughout the act of parturition. The os uteri and vagina are freely lubricated by it.

"It has been generally considered," remarks Dr. S., "a vaginal secretion, partly from the fact of its being found upon the vaginal surface, and partly because no minute inquiry into its nature has ever been made. There is, however, no evidence that the vagina secretes much more profusely during labour than at any other time, and there could hardly be a profuse secretion from the vaginal mucous surface without such a shedding of epithelium as would leave the subjacent structures irritable and painful. Microscopical examination proves that the mucus found in the vagina is chiefly the product of the glands of the cervical canal. At the commencement of labour, the discharge is white and opaque; but as labour proceeds, and after the plug of pregnancy has escaped, it becomes clear and transparent. It is now of the consistence of white of egg, alkaline in character, and consists almost entirely of tenacious plasma and an immense quantity of mucous globules, intermixed with scaly epithelium."

"Upon the completion of natural labour, these glands—those of the cervix uteri—continue to secrete with considerable activity, and their secretion forms a part of the lochial discharge. In many cases, the last secretion which appears after the cessation of the lochia, is the viscid secretion of the canal of the cervix. Thus it is, perhaps, during parturition that the glandular function of the canal of the cervix uteri is most actively performed. The glandular element

seems of more importance, at this time, than either in the unimpregnated state of the uterus or during the course of pregnancy. The uses of the secretion in lubricating the os and cervix uteri and the vagina, during labour, are sufficiently obvious. The physiological condition which obtains at this time is also very closely allied to the pathological conditions which are present in the most common forms of leucorrhœa.

"Mild leucorrhœal discharge is very common during the period of suckling, particularly in women who do not menstruate. The secretion takes place, I have no doubt, chiefly from the glands of the cervical canal. In some cases, it is constant; in others, it occurs only at the monthly periods. It is a common observation that, after labour, the application of the child to the breast causes after-pains, and an increase of the lochial discharge. Uterine contraction and uterine pain are caused, for several days after delivery, every time the child is put to the breast, or the sensation of the draught is experienced. But it occasionally happens that this intimate relation between the breasts and the uterus is preserved to some extent during the whole of lactation, and I have met with some cases in which cervical leucorrhœal discharge constantly occurred whenever the child sucked the breast. Thus there is a marked tendency to increased secretion from the glands of the cervical canal during lactation. Sometimes the foundation of chronic leucorrhœa is laid at this time; but the increased mucous secretion generally ceases after weaning, and the re-establishment of menstruation. In women who are drained largely by leucorrhœal discharges, while nursing, it is only necessary to direct them to wean the child, and the discharge speedily diminishes."

We have given the foregoing general description of the normal secretions of the vagina and neck of the uterus, as laid down by Dr. S., because the views of that gentleman in relation to the pathology of leucorrhœa could not be well understood without it. His account of the secretions differs, in many particulars, from that commonly received; it would appear, however, to be based upon a series of accurate observations, and is, in all probability, correct.

Dr. S. divides leucorrhœa into two leading varieties, *cervical* or *mucous* leucorrhœa, and *vaginal* or *epithelial* leucorrhœa.

"It may be well," he observes, "to revert, for a moment, to the special differences which exist between the vagina and the cervical canal. The lining membrane of the vagina approaches in organization to the skin; it is covered by a thick layer of scaly epithelium; in contains in the greater part of its surface few, if any, mucous follicles or glands; its secretion is acid, consisting entirely of plasma and epithelium, and the chief object of the secretion is the lubrication of the surface upon which it is formed. On the other hand, the lining of the canal of the cervix is a true mucous membrane; it is covered, in great part, by cylinder epithelium; it abounds with immense numbers of mucous follicles having a special arrangement; it pours forth a true mucous secretion, alkaline in character, and consisting of mucous corpuscles and plasma, with little or no epithelium; and this secretion has special uses to perform in the unimpregnated state, and in pregnancy and parturition. Leucorrhœa admits of a similar division. The first, and the most frequent and important, is the mucous variety, consisting chiefly of mucous corpuscles and plasma, and secreted chiefly by the follicular canal of the cervix. The second is the epithelial variety, in which the discharge is vaginal, or is secreted by the vaginal portion of the os and cervix, and consists, for the most part, of scaly epithelium and its *débris*. These two varieties may, of course, exist in various degrees of combination; sometimes the one and sometimes the other preponderates, or is the original affection; but the chief importance must be given to cervical or mucous leucorrhœa, as being the most obstinate and common."

Mucous leucorrhœa, when simple and uncomplicated, is the result of a morbid activity of the glandular cervix. The discharge is at first nothing more than an unusual amount of the elements found in the healthy mucus of the cervical canal. In very severe cases, the mucus of the cervix becomes mixed with pus corpuscles, and the discharge is rendered mucopurulent in character, or the surface of the canal and the os uteri becomes so irritable as to bleed on the slightest irritation, blood corpuscles being added as another element of the

discharge. When the quantity of blood is large and speedily evacuated from the vagina, the discharge resembles the menstrual flux in colour; but when small in quantity and evacuated slowly, it gives to the discharge a greenish or brownish tint. When in cases of simple leucorrhœa the discharge is profuse and long continued, it often proves a serious drain to the constitution, and gives rise to functional or more serious disorders in different parts of the body. In other cases, the secretion is so profuse and watery that the traces of viscosity are nearly lost. This excessive watery secretion, when long continued, is a source not only of inconvenience, but of great debility. Patients suffering from cervical leucorrhœa, besides becoming extremely debilitated by the amount of the discharge, may, also, become hectic from purulent secretion and absorption, or they may be rendered anæmic by the sanguineous complication. In the worst cases, the discharges, in their physical appearances, may resemble those in carcinoma.

"The discharge in *vaginal leucorrhœa* may arise, chiefly, either from the lower portion of the vaginal membrane, or from that part which is reflected over the cervix; but in severe cases, the whole surface of the vagina is involved. The secretion, in these cases, generally consists entirely of epithelium, in every possible phase of development, mixed with acid mucous plasma." "If the case be acute, there are no old and broken scales, such as are found in the healthy secretion, the epithelium being separated too rapidly, in the formation and flow of the discharge, to admit of their coming to maturity and wearing away in the vagina. In mild cases, when the separation is more slow, ripe and well worn scales are sometimes present. When the vaginal form of leucorrhœa becomes very severe, the villi become affected, and not only is epithelium separated with extraordinary rapidity, but pus is formed upon the irritable sub-epithelial or villous surface, which, when mixed with the epithelial matter, can hardly be distinguished from the mucous corpuscles of the cervix, mixed with scaly epithelium. The state of the vagina, as seen by the eye, will, however, remove all doubt as to the nature of the discharge in these cases. A further complication of vaginal leucorrhœa may occur, as when portions of the vaginal surface are so abraded that blood-globules escape and mix with the other constituents of the vaginal discharge. The vaginal secretions now described are those most commonly found in vaginal or epithelial leucorrhœa; but there is another form of vaginal discharge which deserves consideration." In this, "the epithelium is thrown off in large shreds or pieces, in which the pavement-like arrangement of the scales is perfectly preserved. These laminæ frequently have upon them marks of the rugæ of the vagina, and somewhat resemble the cuticle in cases of acute desquamation of the surface of the body. The under surfaces of these masses are also rough from the indentations of the vaginal papillæ. Sometimes, on making a specular examination in these cases, the whole surface of the vagina is seen covered with a white coating, which may be removed by a forceps in membranous pieces of considerable extent and thickness. This affection may be attended with a slight discharge from the sub-epithelial surface; but in many cases the vagina does not contain more secretion than usual, or it may be unnaturally dry. In all epithelial affections of the vagina, the discharge is acid; but the acidity is particularly marked in this—the membranous form of leucorrhœa, as it may be termed. Some of the instances in which I have seen this affection in its most marked form, have been in cases of pregnancy. I have sometimes had patients bring me a mass as large as a walnut, consisting of pieces of the epithelial coat of the vagina rolled up like paper; or I have seen a tumblerful of water rendered perfectly thick with the quantity of shreds removed from the vagina by a single injection.

"In these cases, the simple shedding of the epithelium in great abundance, and the desquamation of the epithelium in masses, might be called *Epithelial Vaginitis*, while the purulent form of the disorder, in which the villi are affected, might be called *Villous Vaginitis*."

So much for the simple forms of leucorrhœa; its sequelæ, when allowed to continue unchecked, are, according to Dr. S., inflammation, abrasion, ulceration, and hypertrophy of the os and cervix uteri, and abrasion and superficial ulceration of the vagina. It is the conviction of Dr. S. that, in the majority of

those diseased conditions of the os and cervix uteri, which have of late been assigned so prominent a rank as distinct and independent affections in medical discussions, are in the majority of cases secondary affections, cervical leucorrhœa being, in fact, the primary and most essential disease. In maintaining the important part played by the cervical secretions in inducing morbid conditions of the os uteri, he does not wish, however, to be understood as saying that they are the only causes of these conditions. But even when disease of the os and cervix uteri has been induced by other causes, cervical leucorrhœa is almost invariably produced, and it generally tends to aggravate the diseased condition of those parts.

Each of the conditions referred to by the author as sequelæ of leucorrhœa are separately considered. The extent to which we have already been led in our quotations from the work before us, admonishes us of the necessity of passing over the interesting remarks presented in reference to these sequelæ.

The constitutional and other derangements consequent upon protracted leucorrhœa come next under consideration. They are general debility, stomachic derangement, amenorrhœa or uterine hemorrhage, anæmia, hectic fever, derangements of the nervous system, pains in various parts of the body, neuralgic affections of the neck of the uterus, and troublesome affections of the bladder and rectum.

The diagnosis between leucorrhœa and cancer uteri, the relations between secondary syphilis and leucorrhœa, are then separately discussed, and a most interesting chapter is given on the relations of vaginal leucorrhœa to gonorrhœa in the female, urethritis in the male, and the ophthalmia of new-born infants.

Dr. S. believes gonorrhœa in the female to be closely allied to vaginal leucorrhœa, and hence impure connection may be ranked as one of the causes of the latter. He has no doubt, also, that urethritis and inflammation of the glans penis may be induced in the male by a female labouring under spontaneous leucorrhœa arising independently of sexual intercourse, and that ophthalmia neonatorum may be caused in children born of females labouring under non-gonorrhœal leucorrhœa.

In Chapter IX., the author discusses the relations of leucorrhœa to disorders of the function of menstruation. He remarks that it is very rarely that leucorrhœa, with diseased conditions of the lower segment of the uterus, exists for any length of time without inducing some disorder of the catamenial function. This occurs chiefly in the cervical forms of leucorrhœa, or those cases of vaginal leucorrhœa in which the affection is confined to the surface of the os uteri and vaginal portion of the cervix. In some cases, the leucorrhœal affection is the secondary disease, amenorrhœa, menorrhagia, or dysmenorrhœa having preceded it; but most commonly, according to Dr. S., the former is found to be the primary disorder in these cases, and the catamenial derangement has slowly followed upon chronic leucorrhœa.

Periodical leucorrhœa, or leucorrhœa vicarious to menstruation, receives a passing notice, and in the ensuing chapter (X.) the relations of leucorrhœa to sterility and abortion are very fully considered, which closes the discussion of the pathological character and relations of the disease.

As constitutional local causes of the disease, Dr. S. enumerates plethora, debility, prolonged lactation, the strumous habit, skin diseases, climate, rectal, vesical, urethral, vaginal, and uterine irritation, gestation, abortion, and labour. Appended to the chapter are some remarks on leucorrhœa in children.

At the risk of being accused of extending this notice to an unwarrantable length, we cannot refrain from presenting to our readers the concluding paragraph of this chapter of the work. It presents a general summary of the author's views in regard to the nature of leucorrhœa, and an expression of his opinion on a question of uterine pathology warmly discussed at the present period.

"From the whole tenor of the present work," says Dr. S., "it will be seen that I differ very strongly from the opinions which refer almost all the conditions upon which leucorrhœa depends to inflammation of the os and cervix uteri. I believe it cannot now be disputed that many of the affections of the

os and cervix recently stated to constitute ulceration of the surface, are, in reality, only epithelial abrasions of more or less completeness. As regards ulceration, I believe the more searching examinations to which its asserted frequency has led, prove that its importance and frequency are much less than were formerly asserted. A modified view of the lesions supposed to constitute ulceration of the os and cervix uteri must certainly be taken; and, in a former chapter, I have stated the grounds upon which I believe that abrasions and superficial ulcerations of the os and cervix uteri, when they occur, are very frequently secondary affections, instead of primary disorders. In like manner, I believe the vaunted importance of inflammation, as the great cause of uterine disorder, must be altogether modified. I think the term 'epithelial abrasion' should, in the great majority of cases, take the place of 'ulceration;' and I believe that the words 'irritation' or 'relaxation' should generally take the place which has been assigned to 'inflammation.' The changes in the uterus, and the increased secretions of the uterus and vagina, found in cases of leucorrhœa, are not such as attend inflammation in other parts of the body. It is not after an attack of acknowledged metritis that leucorrhœa is most prone to occur. The discharge generally comes on in so slow a manner that its advent cannot often be referred to any particular date. No doubt, in some cases—as after suppression of the catamenia from cold or imprudence; after abortion or parturition, or mechanical injury—a genuine inflammatory state lays the foundation of leucorrhœa; but the leucorrhœal discharge and the local irritation constantly remain long after the signs of positive inflammatory disease have passed away. Chronic irritation and relaxation, rather than chronic inflammation, is the state which generally obtains under these circumstances. The most common and immediate cause of leucorrhœa is simple irritation of the glands of the cervical canal, and many of the conditions described as inflammatory, such as abrasions and indurations of the os and cervix uteri, are, as I have repeatedly observed, the results of the long continued discharge, rather than of any inflammation occurring in the os and cervix as a primary affection."

In the *treatment* of leucorrhœa, Dr. S. remarks that undue prominence must not be given to either constitutional or local medication. In some cases, constitutional measures alone will be sufficient to arrest the disease; in others, this may be effected by local means; but, in the great majority of cases, both constitutional and local measures will be necessary to effect a permanent cure. The general principle of treatment, we are told, must be the arrest of the discharge, the removal of the local disorder upon which the discharge depends, and the relief of any constitutional disorder with which the leucorrhœa may be connected, either as cause or effect.

To fulfil these indications, the remedies noticed by Dr. S. are preparations of iron, a combination of iron and alum, tonics, purgatives, vaginal injections, caustic application, pessaries, cubebs and matico, etc., bathing, change of air, rest, and recumbency.

The comments of the author upon each of these remedies, and the particular circumstances and stages of the disease to which they are respectively adapted, are full and interesting. His remarks on the abuse of cauterization are particularly opportune. There are too many practitioners who, in almost every case of impaired health in the female, diagnose disease of the neck of the womb, and, as a necessary consequence, introduce the speculum and make repeated applications of the nitrate of silver to ulcers of the cervix uteri, either real or imaginary.

We thank Dr. Smith for his very excellent monograph, and very earnestly recommend it to the notice of American practitioners. No one, we are persuaded, can rise from its perusal without having acquired more definite and correct views of the pathology of leucorrhœa, and a clearer conception of its proper treatment under the several forms, and with the different complications it is liable to occur.

D. F. C.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *The Entrance of the Spermatozoon into the Ovary.* By M. KEBER.—By the diligence of embryologists, the science of development continues to make wonderful advances. Martin Barry and Nelson had already published observations of the entrance of the spermatozoon into the ovulum; but their observations were not regarded as completely satisfactory, till Keber, following in their footsteps, by a careful observation of the process as it takes place in the fresh water mussel, made out and described the different stages of this wonderful function. Since Keber's work appeared, M. Bischoff has been twice in the press; first of all, with a pamphlet controverting the views of Keber, and then with one fully confirming the same views. Bischoff also takes occasion to make some amends to Martin Barry, whose uncommon accuracy and profound research he has occasionally failed to recognize, till forced by overpowering evidence.

In the fresh water mussel (*unio*), at the time of conception, the ovulum, although still small, projects at one part a minute process which springs from the membrane of the albumen, and perforates the cortical membrane. This process dehisces, lets escape a little albumen, and admits one of the spermatozoa which surround it. This done, the micropyle, as it is called, again closes by constriction or obliteration.

Afterwards (and sometimes earlier) there is formed near the micropyle an adhesion between the membranes of the albumen and of the yolk, then the yolk-bag dehisces, receives the spermatozoon into its interior, and again is closed.

The spermatozoon afterwards sinks deep into the yolk, swells and becomes rounded; after some time a nucleus appears in it, while its outer membrane thins and at length disappears.

The nucleus of the spermatozoon splits up into several irregular divisions which at first lie near one another, and then become diffused through the yolk, so that before the dissolution of the germinal vesicle the yolk is filled with particles derived from the male.

M. Keber has also confirmed the observations of Barry in regard to the small pellucid vesicles not rarely found in the abdominal cavity of the rabbit, attached to the ovary, the fimbriæ, the oviduct, the peritoneum, or the uterus itself; within which are observed vibratory motions over the whole surface, and rotatory movements of various corpuscles, and of a large mulberry-like body on its axis, which increases under the eye of the observer by the apposition to it of roundish corpuscles from the fluid of the vesicle.

Barry observed a similar vesicle imbedded in the mucous membrane of the

uterus. Those found in this situation are smaller than those of the abdominal cavity. In these bodies, Keber has observed changes resembling the formation of the micropyle in the ovulum, and for that and other reasons regards these vesicles as ova. But for further discussion of this subject we refer the reader to Keber's monograph.—*Edinburgh Med. and Surg. Journ.*, October, 1854.

2. *Histology of the Retina.*—The observations of GEGENBAUR, KOLLIKER, LEYDIG, H. MÜLLER, and VIRCHOW, on the body of a beheaded criminal, though chiefly of value in a physiological aspect, throw light on some hitherto obscure points in normal histology.

In some investigations on the retina, it was found that, in the neighbourhood of the yellow spot, the cones are smaller, but of greater length, and closely pressed together, while, external to it, the rods become interposed. The cones, in this situation, were scarcely pyriform, had a pretty uniform thickness of 0.002''' , had no point, and were from 0.012'''—0.014''' in length. At the periphery of the retina, these little bodies were thicker, assumed rapidly a more pyriform or oval shape; so that, with a length of 0.008''' , their greatest breadth was 0.003'''—0.004''' . Their somewhat conical points, which, in the peripheral parts, were marked off by a transverse line, had a length of about 0.006''' ; the isolated rods were about 0.0008'''—0.0009''' in breadth, and 0.012'''—0.014''' long. It was also observed, by careful focalizing, that the points of the cones lay somewhat deeper than the extremities of the rods. Vertical sections through the yellow spot showed a remarkable thinness in this situation; the yellow diffused colour had its seat in the middle layers; but little, if at all, in the inner cell and outer rod-layers.

BERGMANN, of Rostock, had an opportunity of investigating the structures of the eye in the case of a beheaded criminal, six hours after death. His views differ somewhat from those of Kölliker.

Half of one retina was examined fresh; the section had been made through the middle of the yellow spot, behind which only small cones were found, some with numerous rows of rods between them; but this part of the investigation appears to have been very incomplete, owing to the structures breaking up into fragments, the localities of which could not be determined.

The second eye was prepared and hardened before section, and consequently the retina exhibited no folds; but there were some differences of level, owing to the difference of thickness in different parts, while, probably, a great part resulted from the action of the chromic acid. The small angular fovea centralis lay very sharply defined in the middle of a somewhat pyriform field, the point of which was turned towards the colliculus of the optic nerve. This space was bounded on its upper and under sides by delicate borders, which did not reach completely to the point; but there intervened between them here a middle elevation, the plica centralis of former observers. The borders inclined towards each other, but did not touch. Bergmann proposes to call this little field the Area centralis retinæ. In this area the nervous layer becomes suddenly very thin. Some sections from the optic nerve to the fovea showed well the particular disposition of the nervous matter in this situation; the layer of ganglionic bodies was found not to be continued over the base of this depression, while both the granular layers, with the intergranular layer, though very thin, were continuous throughout. Bergmann uses this as an argument against the opinion, that the ganglionic bodies constitute the perceptive part of the retina. Kölliker, Hannover, and others, consider the fovea as a physiologically imperfect part. Bergmann describes a peculiar arrangement of the fibres in the neighbourhood of the fovea, by which those coming from the outer granular layer take an oblique course, and finally pass into the inner layer. This observer claims for this portion of the retina a higher physiological importance than is accorded to it by others.—*Brit. and For. Med.-Chirurg. Rev.* April, 1855, from *Henle and Pfeuffer's Zeitschrift*, Bd. v.

3. *Composition and Action of the Gastric Juice.*—Notwithstanding the numerous investigations to which the gastric juice has already been subjected, the observations made by Drs. O. DE GRUNEWALD and DE SCHROEDER on a woman

affected with fistula of the stomach will be read with interest. This woman, aged 35, and enjoying good general health, weighed 53 kilogrammes [nearly 117 lbs. *avoirdupois*], and was suckling an infant at the time she was under observation. The fistula, which was of two or three years' standing, had doubtless been produced by a perforating ulcer of the stomach. The quantity of gastric juice secreted was estimated, exclusively of the saliva: 65 grammes [a little more than two ounces] per hour, at 584 grammes in the hour, or 14.016 kilogrammes [nearly 31 pounds] daily. This enormous proportion is much greater than that given by Bidder and Schmidt¹—6.4 kilogrammes [a little more than 14 lbs.] each day. The smallest quantity was collected in the morning, fasting; however, it was never less than from 40 [?] to 400 grammes [about 13 ounces] in the hour. The fluid then obtained was in general clear, serous, and colourless; at other times it was more viscid, it sometimes contained bile, without any sign of functional derangement being present. *Sarcinæ* were, with the aid of the microscope, pretty often observed.

As to the chemical constitution of the juice, which was investigated by Dr. Schmidt, the fluid obtained early in the morning, while the woman was fasting, was either neutral or slightly alkaline; after food was taken it was always acid. No hydrochloric acid was found in the analysis of several portions of gastric juice collected at different periods of the day; the presence of butyric and lactic acids is more probable. The following is a *résumé* of these analyses. In 1,000 parts he found, water, 956.595; solids, 43.405—the latter consisted of organic matter, 36.603; inorganic, 6.802; the inorganic contained chloride of sodium, 4.633; phosphate of lime, 0.961; of magnesia, 0.260; phosphate of iron, 0.006; potash belonging to the organic substances, 0.363.

The organic substances consisted of coagulable albuminous matter (pepsin), sugar, butyric acid, uncoagulable protein substances, and lactic acid. The organic acids are not considered as primarily present in the gastric secretion, even as products of the ingested aliments; they vary in quantity according to the quality of the nourishment. Hydrochloric acid, on the contrary, has been regarded as an essential compound, although the analyses do not exhibit it, doubtless because it is easily neutralized by the alkalies of the saliva.

In one analysis, Schmidt found free hydrochloric acid, but only in the proportion of two parts in 1,000, a ratio ten times less than in the dog. As to the question whether the gastric juice prevents the saliva converting starch into sugar, as Bidder and Schmidt assert, the authors found that the action of the saliva was not destroyed; they, however, confirm the observation of those chemists that sugar is not to be found in the stomach of the dog, even after the ingestion of boiled starch.

The digestion of protein aliments was examined by introducing into the stomach through the fistula a certain weight of coagulated albumen, meat, &c., inclosed in thin linen bags; the loss of substance of the particles during a given space of time was thus observed, as well as the changes which took place in the microscopic structure of the elements. It was thus found that for protein substances the solvent power of the human gastric juice is far inferior to that of the dog. Solution is perfected in the stomach of the dog in from two to four hours, while in that of man it requires nineteen or twenty hours. Raw meat is better digested by the human stomach than dressed meat, and veal than beef.

As to the microscopic alterations, the primary fasciculi were found after an hour and a half easily separable from one another, without having themselves undergone any change, the sarcolemma was destroyed. After two hours and three-quarters, the primary bundles began to show transverse fissures; after three hours, only striated lamellæ were seen transversely; after three hours and a half, besides quadrilateral lamellæ, there were some primary fasciculi, longitudinally and transversely fissured, frequently denticulated at their extremities; after three hours and three-quarters, the primary fasciculi were two or three times longitudinally divided. After four hours and a half, there was scarcely any solid residue in the stomach, with the exception of a small num-

¹ Die Verdauungssäfte und der Stoffwechsel; Mitau und Leipzig, 1852, p. 36.

ber of primary bundles, much fissured both longitudinally and transversely, but still exhibiting the transverse striæ. At the end of three hours and a half, or four hours, the stomach was in general empty; the protein substances were then submitted to the influence of the intestinal secretion. As to the digestion of fat, the membrane of the cells is dissolved in the stomach, but the fat itself does not undergo any alteration. Milk, after three-quarters of an hour, formed a thick coagulum, inclosing a large quantity of milk globules and of free fat. At the end of two and a half hours, the casein was observed in part as an amorphous substance, in part as membranous and transparent fragments, with some unaltered milk globules; at the end of three hours and three-quarters scarcely any remained in the stomach.

These changes, which in the stomach of this woman required three hours and three-quarters, or four hours and a half, were completed in the stomach of a dog in two hours.—*Dublin Quarterly Journ. of Med. Sci.*, May, 1855, from *Archives Générales de Med.*, Feb. 1855.

4. *Micrological Characters of Lymph.*—A rare and very remarkable case of lesion of the lymphatics has given to M. GUBLER an opportunity of analyzing this fluid, so seldom in the human subject recorded to have been submitted to chemical or microscopical examination. We, therefore, produce here the most important results of the observation.

A female, in a good state of general health, presented, at the anterior and superior part of the left thigh, about two centimetres below the fold of the groin, several small phlyctenulæ, or translucent vesicles, of the appearance and size of a grain of sago boiled. They were covered only by the epidermis, and appeared manifestly to result from a varicose dilatation of the sub-epidermic lymphatics. They were disposed in two divergent lines, inclosing a very acute angle, the apex of which corresponded very nearly with the opening of the internal saphena; the sides, diverted outwards, were lost before they reached the inguinal region; the superior followed exactly the fold of the groin; the inferior ran a little more transversely. There were four very apparent and prominent vesicles a little lower down, and lying on the border of the sartorius. The largest amongst them, when torn with the point of a needle, gave exit immediately to an opaline fluid, which ran in a little stream down the thigh, falling at the rate of about fifty drops per minute, and this continued until arrested by compression. The smaller vesicles gave exit to but a very small quantity. This liquid, collected in a vessel, coagulated, in from one quarter to half an hour, in the same manner as blood—that is to say, it formed a clot swimming in a fluid; but the serum, so to speak, retained the same colour as the clot, was undistinguishable by the sight, and could only be recognized on shaking the mass.

Submitted to chemical and microscopical analysis, this fluid was found to present the characters assigned to lymph. Other abnormal conditions of the lymphatics existed in this limb, which it is not necessary to specify here. It may be mentioned, however, that on one occasion lymph escaped from a rupture of one of the vesicles, and continued to flow for forty-eight hours; and it has been calculated that about 2,830 grammes, or nearly six pounds, escaped in the twenty-four hours.

Physical properties.—At the moment of exit from the vesicle, the liquid was white, opaque even when seen in drops, having much the appearance of milk deprived of its cream, with a slightly dull yellow tint. It had a strong alkaline reaction, a feeble saline taste, and a scarcely sensible animal odour. As before observed, it separated into a serum and clot, the former still retaining the opaque white colour, and scarcely distinguishable from the latter.

Microscopic examination, with powers from 300 to 500 diameters.—A considerable quantity of yellowish corpuscles occupied the field, similar to those of blood recently withdrawn from the circulation, but of very unequal size. Some had the usual dimensions of blood-corpuscles, but the greater part were sensibly smaller, reaching a diameter of only $\frac{1}{150}$ th of a millimetre. Lastly, there existed a certain quantity of coloured corpuscles, like the preceding, but much smaller, being only about half the dimensions in diameter of the larger

ones—that is, about $\frac{1}{300}$ th of a millimetre. These little globules were seen to be rounded on all sides as they rolled over the field; they were constantly spheroidal, and exhibited no flattening, excavation, or anything which indicated a nucleus; their surface was smooth, their outlines regular, and their yellow colour appeared as intense as that of the best formed blood-corpuscles, if not even more so.

Besides these coloured corpuscles, which, in the opinion of Gubler and Quévenne, are to be regarded as only modifications of those of the blood, there were others less numerous, and pale or colourless, and of very various dimensions. The smallest, having the same dimensions as those last described, were white or colourless at a certain focal distance; but on approximating the object-glass a little towards them, they appeared to present a very light greenish tint. They were spheroidal, covered with small but very apparent granulations, which, however, did not render their outline irregular. The largest of these white globules exceeded in size the largest blood-corpuscles. Their form was regularly spherical, their outlines smooth and uniform, and their walls finely punctuated. There was no visible nucleus, and no appreciable greenish tint. These larger white globules, reaching to $\frac{1}{100}$ th of a millimetre, were very rare, but the smaller white ones were present in abundance; intermediate ones were constantly seen. Lastly, there were suspended in the liquid infinite multitudes of molecular granulations, scarcely visible from their extreme tenuity, reaching to only about $\frac{1}{600}$ th of a millimetre in size.

The above elements may be regarded as those proper to an average specimen of this fluid; but on some occasions others were observed. Thus, in one examination there was found a considerable number of discoid blood-corpuscles, and bodies analogous to the white globules of the blood; in another specimen, the small white globules, and the small spheroidal bodies of a hæmatoid nature above noticed, were less numerous. With these exceptions, the same elements were constantly to be found, and with the same characters, the relative proportions of them alone varying.

Certain changes were produced in these little bodies when allowed to rest, all, however, more or less attributable to histolytic influence, and, as we conceive, in no way characteristic; the discoid bodies became globular, granular, and mulberry-like, and were at the same time diminished in diameter. The small spherical blood-globules manifestly multiplied as the lenticular bodies disappeared; a part of the molecular granules grouped themselves into pellicular masses, more or less extensive, in which were to be seen some very brilliant globules (oil particles).

Weak acetic acid dissolved almost all the red globules, which first became enlarged; a few of the colourless corpuscles resisted its action for some time, but finally disappeared. The white globules were greatly altered by this reagent, but did not become dissolved; the largest became much swollen, their cellular wall becoming pale and thin, and losing its punctuated appearance. The granulations assembled in the cavity of each cell in a single rounded mass, resembling in form and aspect an ordinary ex-centric nucleus. In the small white globules, the granulations of some became more apparent on the addition of the acid; others exhibited a very pale zone, which seemed to be a rudimentary cell-wall, closely embracing a granular nucleus. By the action of ammonia, also, the red globules instantly disappeared; the white globules dissolved in this reagent, but more slowly. The molecular granules were dissolved by ether, oily drops resulting on its evaporation. Iodine coagulated the albuminoid matter, and coloured it yellow, as well as the globules. Water swelled out the discoid bodies, and rendered them vesicular, depriving them at the same time of their colouring matter.

The clot was composed of a mass, which appeared amorphous when thick, but which was manifestly fibrous and striated when examined in thin particles, and showed delicate fibrillæ under the microscope. The following is a *résumé* of the observations on the microscopic elements of this lymph.

It contained, in suspension in a serous liquid: (1) hæmatoid corpuscles, always of a diameter inferior to those of blood, some lenticular-like blood-corpuscles, properly so-called, others very small, spheroidal, and smooth; (2) pale,

scarcely-coloured globules, being those more usually designated as lymph-corpuscles, some exceeding in size the red corpuscles of blood, others much smaller; (3) granular fatty molecules.

The authors regard the first-named elements merely as modifications of the blood-corpuscles, presenting a similar aspect and similar chemical reactions; the second resemble the white corpuscles of the blood, but differ from them in certain regards; these are the veritable corpuscles of lymph of authors. The difference between the white globules of the blood, and the large white globules of lymph, seems, according to MM. Quévenne and Gubler, to be manifested chiefly in the different comportment of their nuclei to acetic acid; but they do not attach much importance to it.—*Brit. and For. Med.-Chirurg. Rev.* April, 1855, from *Gazette Médicale de Paris*, No. xxiv. 1854.

5. *Regeneration of Nerves*.—SCHIFF considers that the regeneration of nervous tissue takes place by the formation of new fibres in the old sheaths, even before the previous ones are completely disorganized. At the seat of section, the parts become red, and somewhat swollen; and between the bundles of fibres, small, rounded, or angular nuclei, with nucleoli, show themselves; between them will be seen a quite structureless mass, resembling connective tissue, in which soon appear nuclei in rows, and at first round, but afterwards oblong, and placed opposite, but in alternate order. On the appearance of these nuclei, the mass becomes separable into laminæ, in which the continuation of the primitive fibres of both ends of the nerve are visible. While at first the whole mass comports itself to potash and acetic acid, like areolar tissue, the cylinder fibres now, on the addition of potash, distinguish themselves from other fibrous tissues, assuming a pale yellow colour, and clear contour. These stages of the development proceed from both ends to the centre uniformly; but the following changes take place more rapidly in the upper. The substance lying between the rows of nuclei acquires a finely striated appearance, and on the side of every finely striated portion may be seen a dark line, which is the expression of a membrane in which the nuclei are placed. The cylinders assume by degrees a somewhat marked, pale grayish-yellow colour, so that the new nerve has now the appearance of the primitive fibres of the olfactory, and, like this, shows here and there indications of a double border, but it is not so dark; the outer line of this double border is stronger than the longitudinal striation in the substance within. Potash brings out the cylinders very clearly, but does not attack them. Acetic acid shows the nuclei distinctly. Somewhat later, the dark investments of their contents (axis-cylinders) seen in the single nerve fibres are, as it were, removed, and between them lie broad quadrangular, or somewhat rounded laminæ, which are found to be fat; when these laminæ exist, the nuclei of the sheath are no longer visible; their number increases, and they at last become united to the sheath, which is at first very thin, and but slowly increases in thickness, and hence the newly-formed nerves for a long time exhibit a small diameter. This author does not agree with Kölliker in the opinion that the formation of a nervous cicatrix is quite parallel to that of embryonal nerve tissue. As to the period at which a regenerated nerve is capable of reassuming its functions, Schiff has observed that, in the infra-orbital and lingual nerves, the communication of sensations was re-established before the regeneration of the marrow in the new part was completed, and at a time when only single fat particles showed themselves in parts remote from each other. The sensitive, and probably also the motor communication, would seem to require, not the marrow, but the axis-cylinder. The period occupied in the regeneration is from eleven to seventeen days; it is shorter in wounds by section than in those by laceration. The vascular nerves heal more easily and quickly than the sensitive, and these, again, than the motor nerves.

The observations are very well borne out by the investigations of Bruck, on the sciatic nerve of a cat, which had been cut across in the middle of the thigh, and examined some months after, when the powers of the extremity had been completely restored. The regeneration appeared to be as complete as possible, union having taken place fibre for fibre, there being no blind or ununited extremities; and in each fibre the cicatrix was still visible where the junction of

the divided parts had been effected. Nowhere was it observed that two or more fibres were united together, nowhere was there any intermediate substance, exudation, or callus; each end of a central fibre had again found a peripheral fibre, with which it had united so as to form a continuous and isolated line. Above and below the cicatrix, the fibres in all these parts, and in all their relations, were perfectly normal. The seat of the cicatrix was marked by a more or less deep circular constriction of the nerve tubes, which on both sides of it were somewhat swollen out, and flask-like. The marrow was in all the fibres, as well above as below the cicatrix for a short distance, somewhat granular, finely striated, and presented a double contour; but at the point of section, and in the dilated parts, it was completely clear and transparent. In this clear space, without the application of reagents, and without further preparation, the axis-cylinder could in many instances be seen, its diameter sometimes unchanged, or, as it might be, a little enlarged or diminished. In a few instances it stopped short on one side or the other, and was no longer visible. The regeneration was most complete in the outer sheath and the axis-cylinder, while the nerve marrow was sometimes not complete, or replaced by another transparent substance.—*Schmidt's Jahrbücher*, No. 9, 1854.

6. *Regeneration of Tendons*.—BONER has instituted some experiments on the regeneration of tendon, for which purpose he made sections of the tendo-Achillis in the rabbit, and examined the parts at various times after the date of section. He finds that when a plastic exudation takes place, the walls of the sheath of the tendon become united, and finally degenerate into a thin solid string, the use of the tendon becoming permanently lost. On the other hand, when an effusion of blood takes place, perfect union of the divided parts is subsequently brought about. The effused blood coagulates very soon, the blood-corpuscles become disintegrated, the fibrin softens, and, after a couple of days, the whole presents a homogeneous, here and there granular, appearance. On the fourth day, the blood-corpuscles have almost completely disappeared, the whole mass being uniformly red, and filled with granules; round cells, with large indistinctly-bordered nuclei, begin to be seen, but soon lose their rounded form, and become elongated, the nuclei assuming a spindle shape. In eight to ten days, the coagulum has become almost completely white, the cells are very delicate in outline, and thin prolongations are thrown off from them, which here and there may be traced connecting one cell with another. The intercellular substance shows a clear longitudinal striation, which is more marked each day, and the whole coagulum more and more assumes the appearance of a true tendon, the tendinous tissue being completely organized about the end of the second week; it is not, however, till the fourth week, or even later, that the normal strength and consistence of tendon are assumed.

Boner concludes from his investigations that the structure of tendon, like that of the cornea, is made up of flat stellate cells, or a fibrous intercellular substance.—*Med.-Chirurg. Rev.*, April, 1855, from *Virchow's Archiv.*, Bd. vii. Heft. 1.

7. *Structure of the Starch Granule*.—The observations of M. Martin, of Vienna, which Mr. Bush believes he has confirmed, have led to the view that the starch granule is a vesicle compressed into a disc-shaped body, and having its edges rolled inwards upon themselves, the concentric striæ on the granule indicating the coils of the volute thus formed. In repeating and modifying the experiments on which this opinion was founded, Dr. ALLMAN was led to doubt the accuracy of the view adopted by these gentlemen; and, from his observations of the action of heat, iodine, and sulphuric acid, on potato starch, considers that the following conclusions may be drawn: 1. That the starch granule consists of a series of lamellæ in the form of closed hollow shells, included one within the other, the most internal inclosing a minute cavity filled with amorphous (?) amylum; that the concentric striæ visible in the granule indicate the surface of contact of these lamellæ; and that the so-called nucleus of Fritzsche corresponds to the central cavity. 2. That while the lamellæ appear to be all identical in chemical constitution, yet the internal differ from the external in consistency, or other conditions of integration. 3. That the order of deposition

of the lamellæ is centripetal. 4. That while the starch granule is thus a lamellated vesicle, it cannot be included in the category of the true vegetable cell, from which it differs, not only in the absence of a proper nucleus, but in presenting no chemical difference between membrane and contents.—*Quarterly Journal of Microscopical Science*, April, 1854.

MATERIA MEDICA AND PHARMACY.

8. *Action of Digitalis*.—Dr. H. BENCE JONES, in an instructive lecture recently delivered before the Royal College of Surgeons (*Medical Times and Gazette*, April 21, 1855), gives an account of some interesting observations, by Dr. TRAUBE, on the action of digitalis:—

“E. Weber has shown,” he observes, “that, when the vagi are undivided, a feeble electric current acting on the medulla oblongata, or on the vagi, causes a diminution of the contractions of the heart.

“Ludwig has proved that, in all mammalia, section of the vagi in the neck is immediately followed by increased frequency of the heart’s action.

“We must recognize in the heart two systems of nerves—

“1st. Musculo-motor, causing contraction.

“2d. Regulator system.

“The ganglia of the heart are the centre of the first system, and the medulla oblongata is the centre of the second system. The regulating nerves pass with the vagi. From the experiments of Weber and Ludwig, it follows; 1st. That abnormal gentle stimulus of the regulator-nerves diminishes the frequency of the heart’s action; and 2dly. That the frequency is greatly increased by the removal of the regulating action.

“From this it may be concluded, that any substance which, when brought into the circulation in small quantity, diminishes the frequency of contraction, but, in large quantity, increases the frequency, acts on the regulator-nerves. Hence digitalis, from Dr. Traube’s experiments: 1st. Stimulates the regulator system of nerves. 2d. Paralyzes the regulators; and when it stops the action of the heart; then, 3dly. It paralyzes the musculo-motor system.

“In small doses, the digitalis acts as a stimulant; in large doses, it acts as a sedative, causing paralysis and death.”

Dr. Traube has also made some interesting observations on the reduction of temperature of the body by the action of digitalis. The determination of the animal-heat was always made in the axilla, and in twelve cases of acute rheumatism, the rate of the pulse and the temperature of the body were taken with extreme accuracy, morning and evening; half a drachm of the leaves of digitalis were infused in four ounces of water, and every two hours half an ounce of the infusion was given. The result was, that generally the temperature fell at the same time, or shortly after the digitalis produced its effect on the heart. Hence, without doubt, the reduction of temperature was a consequence of the slower current of the blood which was produced by the action of the digitalis on the regulatory system of nerves of the heart.

Careful observation showed that digitalis caused a reduction of the temperature in the most different kinds of febrile complaints. Even in puerperal fever the temperature falls when the digitalis is in action.

Dr. Traube draws the following comparison between bloodletting and digitalis as antiphlogistics. Bleeding, while it lessens the force of the pulse, reduces the specific gravity of the blood, and in low inflammations increases the tendency to serous effusion. Hence, in all low inflammations, digitalis is to be preferred as an antiphlogistic to bleeding. Moreover, the effect of venesection is much more rapid, and much more transitory; in its antiphlogistic action digitalis bears a close resemblance to antimony; but it is far less likely to affect the bowels, and hence, in all inflammatory diseases complicated with any affection of the bowels, digitalis is to be preferred to antimony. The employment of

digitalis is, however, accompanied by its own inconveniences and even dangers. It may produce sudden prostration of nervous and muscular action, and even syncope and death; that is, the stimulating action on the regulatory system of nerves may suddenly give place to the paralyzing action. Hence arises the necessity for watching those who are taking this medicine; they should be seen at least twice daily, and if there be sickness, or irregularity in the rhythm of the heart, or great reduction in the rate of the pulse, the medicine must be omitted.

9. *Source of Sarsaparilla*.—After a careful investigation of the subject, Dr. BERTHOLD SEEMANN has arrived at the conclusion that the greater proportion of sarsaparilla imported under the commercial names of “Jamaica,” “Lisbon,” or “Brazilian,” and “Guatemala,” or “Red Paraguay” sarsaparilla, is the produce of one species only, the *Smilax officinalis* of Humboldt and Bonpland; and further, that the *Smilax medica* of Schlechtendahl and Chamisso, and the *Smilax papyracea* of Poiret, are identical with it. *Smilax officinalis* grows in the lower coast region as well as on the mountains to an elevation of five thousand feet above the sea, and is confined, as far as is known, to the South American continent, between the 20th degree of north latitude and the 6th degree of south latitude, and the 110th and 40th degrees of west longitude. The roots, which form the commercial article, abound more or less in starch, according to the age and the conditions under which they have grown. Dr. Seemann considers the Lisbon sarsaparilla to differ from the Jamaica only in the rootlets having been removed by some mechanical means or other before the article reaches the market. The condition indicated by the chief pharmacological distinction into “mealy” and “non-mealy” samples, he believes to depend on the age of the roots, and on the locality in which they are collected. He admits, however, the value of the commercial distinctions as such; for as long as the Brazilian collectors continue to strip the roots of their beard and put them up in long bundles, there will always be Lisbon sarsaparilla; as long as the inhabitants of the Spanish Main continue to preserve the rootlets, we shall have Jamaica sarsaparilla; and as long as the climate and other physical conditions of Guatemala remain unchanged, we shall continue to receive from that locality sarsaparilla distinguished by its abundance of starchy matter.—*Association Medical Journal*, March 16, 1855, from *Pharmaceutical Journal*, February, 1854.

10. *Pterocarpus Erinaceus* or *Kino-Tree* of *West Africa*.—Dr. W. F. DANIELL being stationed at Macarthy's Island, on the Gambia, availed himself of the opportunity afforded him of investigating the botanical source of African kino, and concludes that it is derived from a tree whose botanical characters corresponded with those of the *Pterocarpus erinaceus*; a specimen of which, brought from central Africa, in 1805, by Mungo Park, is preserved in the British Museum. Though growing on the banks of other streams in Western Africa, it flourishes most exuberantly on the upper banks of the Gambia. Macarthy's Island, prior to its occupation, was thickly covered by this tree. The tree is described and figured.

The gum, when it naturally exudes, is in limited quantities between the crevices of the bark, and cannot be detected on account of the external crust being black, and unless the outer has been purposely abraded or incised, with the view of obtaining a larger amount, is likewise somewhat difficult of collection. The exudation, as it first appears, is of a pale and dirty reddish hue; more copious, and of greater liquidity, if the younger branches or twigs are cut. This gradually darkens in colour in proportion as it becomes exposed to the air; and as exsiccation is effected, assumes a ruby tint, which time deepens more and more. In general, African kino oozes forth in very superficial layers, remarkably friable and brittle, and adheres with such tenacity to the abraded surface that it is almost impossible to procure any quantity without being conjoined with a portion of the woody fibre. Owing to its extreme brittleness, wheresoever the larger masses are fractured, the fragments are necessarily small and pulverulent, inodorous, of an irregular angular form (those from the interior being shining), and of a deep ruby colour ultimately acquiring a

brownish shade by atmospheric exposure. The most propitious time for procuring the gum is in the months of March and April, from trees of moderate dimensions. The means most efficient to facilitate its exudation is by a number of transverse incisions or abrasions in the bark, a few feet from the ground.—*Ibid.*, from *Pharmaceutical Journal*, August, 1854.

11. *Cyanuret of Mercury*.—M. DESMARTIS, of Bordeaux, after a careful comparison of the effects produced by the different preparations of mercury, has come to the conclusion that the cyanuret is superior to all others, especially in syphilis. He believes it to be at the same time efficacious and innocuous in its action; he never saw it occasion salivation or any intestinal irritation; and often, when all the preparations of the metal had failed to produce benefit, he has seen it restore to health patients whose cases seemed hopeless in the extreme. He has found its use to be efficacious in certain cases where the patients had suffered, for a long period, obscure pains, for which no cause could be discovered. He has employed it with benefit in iritis, and in syphilitic affections of the nose and fauces.—*Dublin Hospital Gazette*, April 1, 1855.

12. *Modes of Exhibiting Iodine*.—It may be questioned whether the desire to avoid complexity in prescribing, which has recently become so prevalent, does not on some occasions materially interfere with the efficiency of our therapeutics. In the London Hospitals, the iodide of potassium is, for instance, almost constantly used alone, whenever the specific effects of iodine are desired. Now, although we have, perhaps, no single remedy whose powers are more uncontestedly proven than those of this salt, yet there are considerable reasons for doubting whether its effects may not often be very much increased by combination with iodine itself. Most of our pharmacopœiæ contain formulæ for compound solutions, and such, it is well known, were those employed by Lugol and others, who were the first to examine into the properties of the remedy. It was, however, early observed, that the iodide of potassium appeared to possess all the specific powers of iodine, and, from its being a salt of easy use, and very manageable, it soon came to be the favourite preparation. Mr. Lloyd, of St. Bartholomew's Hospital, is one of the few surgeons who still entertain a strong opinion as to the superior efficacy of the compound formulæ, and the result of some cases which we have recently observed under his treatment have strongly supported his view. One of these was that of a woman, who for several months had been attending in the out-patients' room, and taking the iodide in large doses on account of a severe form of constitutional syphilis, marked by an ulcerating tubercular eruption and ulcerated throat. No benefit having accrued, she was ordered for admission, and came under the care of Mr. Lloyd. The remedy ordered was the following: *R. Tinct. iod. co. ℥x, potas. iod. gr. v, aquæ ʒx. Ft. haust. in ter diebus sumend.;* and under it the most rapid benefit ensued, and a good recovery resulted. Mr. Lloyd stated, in directing the attention of his clinical class to this case, that he was accustomed to observe very frequently that patients who had long taken the iodide without benefit would improve at once on the addition of iodine itself to the mixture. He added, that he made it an invariable rule never to give iodine when there was the least febrile disturbance present; always, when necessary, preceding a course of that medicine by a few days of preparatory treatment by purgatives and salines. The dilution with a large quantity of fluid was important, in order to prevent the medicine from irritating the stomach. If otherwise suitable, porter might be used as a diluent in place of water. All who are accustomed to use the iodide know that it is needful in most chronic diseases requiring it, to go on increasing the dose at very frequent intervals. A case of lupus, for instance, or of tubercular syphilitic eruption will improve rapidly for a short time, and then become stationary, to advance again on the addition of a grain or two more of the salt. In this way it is often necessary to go up to an enormous dose in order to have progressive improvement. It appears to be more especially in such cases that the use of a compound solution is indicated; for, on the adding of a little iodine, the proportion of the iodide may be at once reduced.—*Med. Times and Gaz.*, January 6, 1855.

13. *External Use of Conia.*—Conia (or conein), as most of our readers are aware, is an oleaginous fluid, which is the active principle of hemlock. MURRAWJEFF, a Russian physician, has found it to be efficacious in the following diseases: (a). In chronic skin diseases, it allays irritation and favours the cure. He has used it successfully in eczema, psoriasis, acne, lichen, psora, prurigo, &c. He recommends it in favus, and regards it as a specific for that disease when affecting the scrotum. (b). In toothache, one drop allays the pain more rapidly and effectually than even chloroform. (c). In neuralgia and syphilitic pains of the bones, he alleges that it is not a mere palliative, but a radical cure. (d). In chronic synovitis, its use is beneficial, as also in allaying the pain of (e) incised and gunshot wounds. (f). In scrofulous and rheumatic inflammation of the eyes, it speedily diminishes the severity of the symptoms. (g). It palliates the pain in scrofulous and cancerous ulcers. The mode of its employment is as follows: In skin diseases, an ointment is used composed of from twelve to twenty-four drops of conein to $\mathfrak{z}\text{i}$ of ung. simpl. or cold cream. The diseased parts having been first rubbed with soft flannel, the ointment is applied to them, and they are then covered with oiled silk and a bandage. In neuralgia, he first washes the part affected with spirits of wine, and then applies to it three or four drops of pure conein, covering it thereafter with oiled silk and a bandage. In cancer, he mixes the alkaloid with mucilage, and applies it to the diseased surface with the finger, previously covered with a glove. For a collyrium he adds 1-3 drops to $\mathfrak{z}\text{vi}$ of Aq. destil. and $\mathfrak{z}\text{ij}$ of decoct. cydonii; and, when used as an enema, he administers two or three drops in starch emulsion.—*Dublin Hospital Gaz.* April 15, 1855, from *Med. Zeit. Russl.* 17, 1854.

14. *External Uses of the Acid Nitrate of Mercury.*—A solution of the nitrate of mercury in strong nitric acid is in very common use at the Hospital for Cutaneous Diseases, and constitutes a very convenient form of caustic. Its formula is—*R.* Hydrargyri $\mathfrak{z}\text{j}$, acidi nitrici (specific gravity 1.50) $\mathfrak{z}\text{ij}$; solve. The solution produced is a clear, colourless fluid. The following may be mentioned as some of its chief uses:—

In Carbuncle.—Mr. Startin usually applies the caustic if the carbuncle be of not more than moderate size, to but one central spot, where it is freely painted for an extent of about a shilling in size. Its effect is to produce an eschar, from beneath which the core afterwards escapes.

In Acne.—A very minute drop of the acid is placed, by means of a finely-pointed glass brush, on the apex of any indolent tubercles, whether suppurated or otherwise. It has the effect of opening the pustule, if matter have formed, and if not, induces the disappearance of the induration. The application is followed only by a little smarting pain, and if it have been carefully made leaves no scar.

In Boils.—There can, we think, be little doubt as to the superiority of the caustic treatment over that by the knife, even in the case of very large boils. The pain of the incision, the large sore caused, and the unsightly scar which follows, constitute very formidable drawbacks to a practice for which there is no real necessity. At this hospital, where cases of boils are very common, the knife is never resorted to. The general treatment consists in giving aperients and steel conjointly, and the local in applying to the apex of the furuncle a full-sized drop of the acid nitrate solution. The morbid action generally terminates coincidently with the application, and the core is thrown off through a comparatively small opening, the resulting cicatrix being insignificant.

In Lupus.—The acid nitrate is one of the most efficient and convenient forms of caustic in this disease. Mr. Startin does not, however, employ it solely, but uses also the biniodide of mercury, and a paste of which arsenic is the principal ingredient. The acid nitrate is chiefly used in indolent tubercles, and to indurated patches not actually ulcerated. After ulceration has occurred the arsenical paste is preferred.

For Sloughing Ulcers.—The practice of treating unhealthy ulcerations, wherever situated, by means of caustics, is much pursued at this hospital, and with excellent results. The pain attending the application of nitric acid has

been much overrated by the profession generally, and its use has consequently been avoided in many instances in which it would have been efficient to completely change the course of the morbid action and induce healthy processes. Its powers in cases of phagedæna are now widely recognized, and its use will probably soon extend to various other kinds of ulceration of somewhat similar nature, but much less severity. The pain spontaneously caused by an unhealthy sore during a single night is probably much more than that produced by an application of caustic. In most cases of sloughing or unhealthy ulcers Mr. Startin employs either the solution of the acid nitrate or the arsenical paste just referred to. The rapidity with which the surface granulates afterwards is often surprising.

In Moles, Nævi, etc.—Small moles on the face, if superficial and not too thick, may be readily destroyed by the acid nitrate. A cicatrix of course results, but it is small, and far less unsightly than the original disease. Small cutaneous nævi are often treated both at this and the various other London Hospitals, by means of the nitric acid. Unless the disease be of very small extent, the employment of a ligature appears to be a much more certain means of effecting the end desired. If there be a subcutaneous base to the morbid structure it often persists in growing, despite frequent applications of escharotics. There is a mild form of dilated cutaneous capillaries which produces the marks known as "port-wine stains," "spiders," etc., in the treatment of which much benefit may be obtained by the dexterous application of fluid caustics. With a finely-pointed glass brush, charged either with nitric acid or the acid nitrate of mercury, the tortuous vascular trunks should be severally painted, a minute streak of the caustic being thus left along the whole course. In this way, by repeated applications, the whole of the larger vessels may be destroyed, and the disfigurement, to a large extent, diminished. The "port-wine stain" is of course very much more difficult to remove than the less diffused forms of this condition, such, for instance, as are of frequent occurrence on the cheeks or nose; even in it, however, much benefit may by patient treatment be gained.—*Med. Times and Gaz.*, January 6, 1855.

15. *Glass Brushes for Applying Fluid Caustics.*—It is desirable, in the use of any of the mineral acids as escharotics, that their strength should not be diminished by the employment of any material susceptible of being charred. A serious objection, therefore, lies against the use of wood, cotton-wool, lint, etc., all of which have been recommended for that purpose. Glass is by far the best material, being at once durable, cleanly, easy of use, and quite insusceptible of the action of the fluid. A glass rod, rounded at one end, and drawn to a fine point at the other, may be made to serve most purposes, one or the other extremity being employed according as it is wished to apply the acid over a large or a small extent of surface. A few brushes, of different sizes, made of spun glass, are, however, yet more convenient. Those sold in the shops are much too large for most of the purposes mentioned in the above notice of the uses of the acid nitrate, and we have seen none which would exactly meet the required conditions, excepting those in use at the Hospital for Skin Diseases. With a little glass tubing, of the thickness of a quill, a skein of spun glass, and a little sealing-wax, they may be inexpensively made by any one of ordinary ingenuity. The brush part is first made by uniting together with sealing-wax a tuft of the spun material, and is then introduced into the end of a tube, which has been either flattened out or brought nearly to a point while heated. A little additional heat easily fixes the tuft in position, and by scissors it may then be cut down to the requisite size.—*Med. Times and Gaz.*, January 6, 1855.

16. *Pencils of Diluted Lunar Caustic.*—At the Samaritan Hospital, Mr. SPENCER WELLS has introduced the use of nitrate of silver in the solid form, diluted by a mixture of one, two, or three parts of nitrate of potass. The salts are melted together, poured into moulds, and allowed to cool. Those used by Mr. Wells are prepared by Mr. Bastick, Chemist, of Brook Street. They have certain advantages in practical application over solutions of the same strength. When applied to the conjunctiva of the eyelid, for instance, it is not easy to

prevent a solution from extending much further than necessary, or even from affecting the conjunctiva of the bulb or cornea.¹ On the other hand, an undiluted stick of nitrate of silver acts too powerfully on the mucous membrane. By using the stick diluted with varying proportions of nitrate of potash, the required activity can be obtained, and the effect limited to the exact seat of morbid action. When the conjunctiva of the lid is alone affected, and it is desirable to avoid the action of caustic on the conjunctiva of the bulb, Mr. Wells is accustomed to wash the lid, after applying the caustic and before the eye is closed, first with a solution of common salt, which converts the unchanged nitrate into a chloride of silver, and then with pure water. In this manner, all the good effects of caustic may be obtained without any of its inconveniences or evil consequences, and may be limited to any desired spot. In gonorrhœal affections of the urethra and vagina, and in various indolent or irritable sores, the same mode of applying the caustic becomes useful. The saving of expense is also worthy of some attention in Charitable Institutions and Union practice. —*Med. Times and Gaz.*, January 6, 1856.

17. *Use of Lime-Water in making Bread.*—To neutralize the deterioration which the gluten of flour undergoes by keeping, bakers add sulphate of copper or alum with the damaged flour. Professor LIEBIG, however, has conceived the idea of employing lime, in the state of solution, saturated without heat. After having kneaded the flour with water and lime, he adds the yeast, and leaves the dough to itself; the fermentation commences, and is developed as usual; and if we add the remainder of the flour to the fermented dough at the proper time, we obtain, after baking, an excellent, elastic, spongy bread, free from acid, of an agreeable taste, and which is preferred to all other bread after it has been eaten for some time. The proportions of flour and lime-water to be employed are in the ratio of 19 to 5. As the quantity of liquid is not sufficient for converting the flour into dough, it is completed with ordinary water. The quantity of lime contained in the bread is small—160 ounces of lime require more than 300 quarts of water for solution; the lime contained in the bread is scarcely as much as that contained in the seeds of leguminous plants. Professor Liebig remarks, that “it may be regarded as a physiological truth, established by experiment, that corn flour is not a perfectly alimentary substance; administered alone, in the state of bread, it does not suffice for sustaining life. From all that we know, this insufficiency is owing to the want of lime, so necessary for the formation of the osseous system. The phosphoric acid likewise required is sufficiently represented in the corn, but lime is less abundant in it than in leguminous plants. This circumstance gives, perhaps, the key to many of the diseases which are observed among prisoners, as well as among children whose diet consists essentially of bread. The yield of bread from flour kneaded with lime-water is more considerable. In my household, nineteen pounds of flour, treated without lime-water, rarely give more than twenty-four and a half pounds of bread; kneaded with five quarts of lime-water, the same quantity of flour produces from twenty-six pounds six ounces to twenty-six pounds ten ounces of well-baked bread. Now as, according to Heeren, nineteen pounds of flour furnish only twenty-four pounds and one-half ounces of bread, it may be admitted that the lime-water bread has undergone a real augmentation.”—*Dublin Medical Press*, April 25, 1855, from *Chemist*.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

18. *Hæmoptysis as a Sign of Tubercle; Curability of Consumption; Effect of Cod-liver Oil, &c.*—The value of hæmoptysis, as an indication of incurable tubercle and consumption, has been a medical question often debated, and this

¹ [The operator who cannot prevent this has little claim to dexterity.—ED. AM. J.]
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sign or symptom by itself perhaps a little overrated. The chances of pulmonary hemorrhage are, no doubt, increased by whatever tends to diminish the capacity of the chest, as in persons of different trades—tailors, dressmakers, &c.—with crooked spines. Again, in advanced consumption, with cavities, one is too often called upon to witness total destruction of lung tissue and hæmoptysis. In both these, however, we may understand the cause of hæmoptysis. In the present instance, however, we wish to speak of the popular and professional notion of phthisis following hæmoptysis as certain, so to speak, as a shadow its substance, and in such matters of every-day practice as signing a certificate for life insurance, hæmoptysis is considered conclusive evidence against doing so. In a large number of cases where hæmoptysis occurs through life, the tendency is evidently towards tuberculosis; a certain proportion of cases probably go on to consumption, whilst the residue are cured. If, out of 500 cases, 100 or 200 escape, it becomes an interesting question, what are the general conditions that lead to a cure? what, on the other hand, are the conditions to facilitate the inroads of consumption? It is instructive to compare the results at a general hospital, like the London Hospital or Guys, and the results at Brompton. At the latter we find, on inquiry, that hæmoptysis takes a very formidable position indeed in the chapter of symptoms preceding tubercle; we find also that cod-liver oil, given in over-doses or in particular cases, has a very manifest tendency to produce hæmoptysis. Amenorrhœa, also, and heart disease are often attended by this symptom. Leaving all these, however, out of the calculation, we have had reason to be more hopeful of the curability of consumption.

We have been singularly struck with the importance of this question, and with the practical value of the facts springing out of it, from observing the notes of cases, tables, and general results arrived at from the investigations of Dr. Andrew Clark, at the London Hospital. It appears that Dr. Clark entertains the opinion that phthisis of a limited kind is of much more frequent occurrence, and becomes much more frequently cured, than is ordinarily admitted or supposed. This opinion he conceives to be capable of demonstrative proof in three ways: first, by the results of a large number of *post-mortem* examinations conducted by himself, which show that in the bodies of patients dying from accident, or some non-tubercular disease, obsolescent or healed tubercles are found in a very large percentage of cases; secondly, by showing that out of a given number of persons presenting themselves indiscriminately for relief at the London Hospital, many are found to have had hæmoptysis, and that of these a certain proportion has proceeded to the development of unequivocal phthisis, whilst another proportion has appeared to terminate in complete recovery from symptoms of pulmonary disease; thirdly, by showing that cases of limited chronic phthisis, proved by the presence of air vesicles and tubercular matter together in the sputum, as we have seen it, do not unfrequently proceed to arrestment of the general symptoms, suspension of the progress of the local pulmonary lesion, and subsequent cure.

There can be no doubt that a series of investigations in this threefold aspect, if followed to their utmost ramifications, and conducted with minuteness and care, will lead to great practical good. In the meantime, without committing ourselves to any specific opinion upon all the questions mooted, we are desirous of commenting upon the second aspect of Dr. Clark's investigations as one which is eminently practical both in its relations to the disease itself in hospital wards, and to the value of hæmoptysis in its bearings upon the signatures to life assurance, as we have already hinted.

In the second aspect of these investigations, Dr. Clark proceeds upon the opinion, that in all cases when hæmoptysis has occurred to the extent of an ounce in the absence of amenorrhœa, aneurism, heart disease, and ulcerated throat, tubercular matter is really present in the lungs. He holds the same opinion even when the hæmoptysis is to a much less amount, provided it be of frequent occurrence, uncomplicated with marked cough and any acute affection of the lung.

Dr. Clark then proceeds to show that, out of a given number of persons applying *indiscriminately* for relief at the London Hospital, and carefully exa-

mined relative to this point, hæmoptysis will be found to have occurred in a large percentage; and that in these instances the hæmoptysis is followed in a certain number of cases by the induction of phthisis and in a certain proportion by suspension of the symptoms accompanying it and ultimate recovery, and immunity from pulmonary disease. Having established the fact that hæmoptysis, preceded, accompanied, and followed by pulmonary symptoms, often disappears without the return of it or of any signs of pulmonary lesion for years afterwards and sometimes not at all, Dr. Clark then endeavours, as the object of greatest importance in the inquiry, to determine under what conditions this return to health takes place; to what extent, if at all, they can be superinduced by art; and with what amount of certainty we can predict in a given case, termination in recovery, or in phthisis.

We have not space to enter more fully into these details at present. We have pointed out the broad relations of the subject, and shall satisfy ourselves with abstracts of a case or two as types of those in which hæmoptysis has not been followed by confirmed phthisis.

J. B—, aged sixty-five, a labourer, short, spare, and bent, presented for dyspepsia, accompanied with depression and headache. He was delicate in youth, and considered to be "declinish" at the age of three or four and twenty. At that time he had cough and expectoration, and frequently spat blood. He used to have pain in the left side, but particularly under the left scapula and about the shoulder. Never had dyspnoea, and never was confined to bed for any length of time. Was very temperate at that time; ate little, and took care of himself. Continued much in the same way for two or three years; was always much subject to colds, and frequently blistered for them. Afterwards began to be better, exposed himself to all weathers, lost his liability to colds, lived rather irregularly, worked hard, took beer and spirits as occasion offered, and lost all note of his symptoms at about thirty, except one, which was pain or gnawing under the left scapula. When he has a cold now, he suffers from a pain in the chest, and has yellow expectoration. Does not often have a cold, and for some years has only suffered from occasional bilious fits and rheumatism.

He has, at present, no distinct cough, and no dyspnoea; has a habit, however, of clearing his throat, and expectorates occasionally in the morning, vitreous-looking gelatinous masses, about the size of peas. The pain under the scapula has always recurred at intervals of from three to six months, and lasted variable periods. Left side of chest is flatter than right, and is found, by placing the hands under the axilla, not to fill so well. There is dulness on the left side, jerking respiration, and increased vocal resonance; no râles; around the dulness the lung sounds preternaturally clear, as also does the whole anterior surface of the right lung; heart-sounds healthy; every part of the chest thrills with the reverberation of the patient's voice.

We have room for the notes of only one other case, which is a significant and instructive one, but not occurring at the London Hospital.

Mr. B—, aged nineteen, tall, slim; with fair hair, blue eyes, large pupil, transparent skin, and long incurved nails, was seen, in 1846, when he presented all the symptoms of early phthisis. After a time he got better, and was lost sight of for some months. In 1847, he was seen again, and had had hæmoptysis, followed by cough, expectoration, night-sweats, and wasting. No cavity could be made out, *but the expectoration contained patches of air vesicles, which Dr. Clark has preserved.* He again got a little better, and was moving about when he met with an accident—a fall—which was followed by the formation of an abscess on the upper and outer part of the thigh. The abscess was opened, discharged, and continued to discharge a large quantity of matter. Under this exhausting discharge he was with difficulty supported. He became extremely emaciated, and almost helpless. At the same time, a remarkable change was observed in the pulmonary symptoms. They began to abate from the day the abscess was opened, and ultimately disappeared entirely. Gradually he regained flesh and strength, and the pulmonary symptoms did not return. In 1848, he appeared to have perfectly recovered; but contraction

having followed the obliteration of the abscess, he had left his former service, and become a railway clerk.

On the 9th of November last, Dr. Clark met this patient accidentally at the Crystal Palace; and so great, then, was the alteration in his appearance, that Dr. Clark failed at first to recognize him. He was unusually fat for so young a man—corpulent, in fact, and robust-looking. He declared himself to be in perfect health, and to have been so for some years. The pupils were still large, and the nails pink, long, and incurved.

Andral states, that only in one instance in which hæmoptysis had occurred to him—and even then the immediate cause of death—had he ever found the substance of the lungs free from tubercles. Louis, we need hardly say, gives an equally fatal tendency in 2,000 cases in which he made the inquiry, in later years, after the mode by Dr. Andrew Clark; but in 87 private cases under his care, 4 in 6 had hæmoptysis.

Pinel gives a singular case of hæmoptysis in a female, which occurred regularly every month, at the woman's monthly period, during forty-two years. It was originally caused by fright, and was always subsequently somewhat increased by strong mental excitement. When suspended for a month or two, the patient invariably suffered from intense headaches. She had usually before the hæmoptysis a sensation of weight and uneasiness about the lumbar region and pelvis; soon followed by chilliness, lassitude, oppression at the chest, headache, and ultimately a distinct sensation of stinging or bubbling in the bronchial tubes and trachea; then, finally, sharp cough, and spitting of blood. The woman was fifty-eight years of age when it stopped; she was stout and plump. What conditions here saved her from getting phthisis, like the patients of Andral or Louis, would be an interesting subject of speculation.

The older observers differ very materially as to the frequency of hemorrhage from the lungs in relation to tubercles. Hæmoptysis may precede tubercle for years and years, and even be almost forgotten, till the patient is reminded of it. The use of cod-liver oil, we believe, has very materially changed the rate of mortality and curability of phthisis of late years. Andral found in those dying of phthisis, in his time, that one in six had never had hæmoptysis at all; in two in six the hæmoptysis appeared to mark the development of tubercle as a cause; in the remaining number, or half the deaths in Paris from phthisis, it followed rather as a consequence from unequivocal phthisical disease, with diarrhoea and wasting, and a breaking up of the lungs into anfractuous sinuses and cavities.—*Lancet*, March 24, 1855.

19. *Goitre in an Infant*.—Professor SIMPSON exhibited, at a meeting of the Medico-Chirurgical Society of Edinburgh (Feb. 7, 1855), a pathological specimen of great rarity and of much interest, as it satisfactorily disposed of not a few of the current theories as to the production of the disease. Mr. Syme, for example, was inclined to ascribe it to the fact that persons affected with the disease were compelled from their occupations to climb great heights, where the air was less dense, and who lived in valleys where there was an equal stagnation; others ascribe it to the use of snow-water, or of water impregnated with iodine. Now the specimen in question proved that goitre was not due to any of these influences. It had been obtained from a child which lived but a few hours after birth. When born, it was nearly asphyxiated, and it had only been kept alive by the presence of a catheter in the throat obviating the pressure of the mass. The brain was of small size, and was covered by a quantity of watery fluid, on the removal of which the brain was found to occupy only the posterior segment of the cranial cavity. The mother was an Italian by birth, and had never suffered from the disease. She had had seven dead children and three living. The cause of this mortality was diseased placenta; and latterly she had taken large quantities of chloride of potass with good results. No other malformation was ever observed in her other children. In the present instance, labour was brought on at the eighth month by the passage of a probe between the membranes, as the child's heart was observed to beat slowly. The enlargement of the neck had the same influence in throwing the head back as the arm of the child when placed under the chin; the labour in consequence was tedious.

Dr. Keiller had met with a similar case nine months ago, and the child, he was happy to state, was still alive. The mother came from the neighbourhood of Cockermouth, where he (Dr. Keiller) believed goitre was prevalent; she had at one time of her life been affected with the disease. The tumour in the child's neck had become smaller since birth.

Dr. Simpson believed that there were different kinds of swellings met with in the necks of children. Large ranulæ were occasionally present from an enlargement of the salivary glands; a beautiful specimen of which existed in the late Dr. Hamilton's collection; the tumour was fully half the size of the child's head. He had himself seen two other cases which occurred in the old town of Edinburgh, both of which proved speedily fatal from the gradual enlargement of the ranulæ.—*Monthly Journ. Med. Sci.*, April, 1855.

20. *Peculiar Concretions in Typhus Stools*.—ZIMMERMAN has repeatedly observed in the stools of typhus patients considerable numbers of peculiar concretions, which varied in size from that of a large Turkish to that of a common white bean. He considers that there exists some relation between them and the millet-seed-like white corpuscles which, it is known, are found in typhus stools. The concretions varied in form and appearance; some were round, others spherical; some were smooth, and others rough on the surface. They were of a yellowish-white colour, of tolerably firm consistence, but capable of being crumbled between the fingers. When chemico-microscopically examined, they were found to contain, besides large numbers of epithelial cells, various fatty matters (1,000 parts contained 237.8 of solid residue, and 156.9 of fats), among which there was almost no cholesterine; they also contained biliary constituents—a sort of albuminous substance, which Zimmerman believes to be casein and fibrin; and finally various salts (viz: sulphates, carbonates, and phosphates of soda; chloride of sodium, and earthy phosphates). The concretions differ from the corpuscles in their containing protein compounds, while the others consist of fat, with merely a little earthy matter. In the concretions, the mineral ingredients are to the organic in the proportion of 1 to 17; in the feces they are as 1 to 1; the former contain little chloride of sodium, while in the latter, this salt exists in large proportions—constituting nearly a third of the whole.

Sigmund found similar fatty concretions in the feces of a lady affected with biliary derangement (*Med. Chemie*); and Kletzinsky (*Heller's Archiv.*, I., 1853) found similar fatty matter in the cæcum of a person who had died of typhus, which was deficient in cholesterine, but contained biliary matters, intestinal mucus, and various salts, especially earthy phosphates.—*Monthly Journ. of Med. Sci.*, March, 1855, from *Deutsche Klinik.*, 28, 1853.

21. *Perforation of the Septum Cordis*.—Prof. HAUSKA, having had a heart sent him, as furnishing an example of the aorta arising in both ventricles, found, on examination, that the appearance of this being the case arose from the septum of the ventricles having become perforated. He takes the opportunity to draw attention to an anatomical fact, allusion to which he can nowhere find, viz: that there is in the normal state a spot in the septum cordis, varying in size from a bean to an almond, entirely destitute of muscular substance; the two chambers being there separated only by the layers of endocardium that line them. Examining the septum from the left, after slitting up the aorta, we may remark a thin diaphanous spot, close under the angle formed by the convex borders of the right and posterior semilunar valves of the aorta, being closed above by a thin muscular bundle, coursing along the contour of the ostium arteriosum sinistrum. In the right ventricle the deprivation of muscular substance is covered by the end of the tricuspid valve; and so thin is the duplicature of the endocardium, that the lines and markings of the finger held under it can be seen through. M. Hauska observed the appearance himself only a year since; but since then he has found it in every heart (about 300) he has examined, of whatever age or sex.

This appearance is of great interest in a pathological point of view. In endocarditis, the endocardium becomes loosened and friable, and it is not seldom

actually torn, as the rupture of the valves and the rapid formation of aneurism of the heart show. If such inflammation happened to attack this spot, a communication between the ventricles, by rupture of the endocardium, might easily result. On examining the heart sent him as an example of anomalous origin of the aorta, the ostium arteriosum sinistrum was found directed towards the right, as well as the left ventricle, while the swollen edge of the septum ventriculorum, covered with opaque and thickened endocardium, sloping from before backwards, was carried up to the middle of the orifice of the aorta. This condition necessarily arises as soon as the endocardium closing the aperture is torn. The blood of the right ventricle passes, in gradually increasing quantity, through the new opening, which becomes proportionally enlarged, and, owing to the simultaneous contraction of the two ventricles, the blood does not pass into the left ventricle, but immediately into the aorta, where it becomes mingled with the blood of the left side. This newly-established stream of blood from the right ventricle to the aorta, gradually forces the commencing portion of the aorta towards the right, so that at last the ostium arteriosum sinistrum is placed obliquely over the perforated septum, and with its orifice turned towards both ventricles.—*Med. Times and Gaz.*, April 28, 1855.

22. *On the supposed Effects of the Suspension of accustomed Stimulants in the production of Delirium Tremens.* By Dr. PEDDIE.—In order to obtain some additional evidence on this disputed point, I submitted some queries to Drs. Simson and Gibson, the medical officers of the large prison establishment of this city and of Glasgow, and to Mr. Page and Dr. Scott, surgeons to the county jails of Carlisle and Dumfries; and the following information has been kindly furnished by them, as to the effect of the sudden withdrawal of all stimulants from civil and criminal prisoners known or presumed to be of intemperate habits, and the immediate substitution of prison fare, which is well known not to be of the most generous description.

As regards the prison of Carlisle, it appears that, although the annual number of commitments during the last fifteen years has been about 600; and that, although three-fourths of these are considered to have been, in one way or another, the consequence of drunkenness, Mr. Page states emphatically he has never yet seen any ill "result from the sudden abstraction of stimulants from habitual drunkards, who had been drinking to excess up to the time of being placed on prison fare." Mr. Page had also, during nine years' experience in connection with the Carlisle County Pauper Lunatic Asylum, observed the same impunity with which all stimulants could be at once withdrawn. (*Letters 9th and 21st June, 1854.*)

Of the jail of Dumfries, it is stated by Dr. Scott (*Letters 12th and 21st June, 1854*) that, during the last fifteen years, the number of civil and criminal prisoners have amounted to 5,539; that of this number he supposes about two-thirds were committed for crimes resulting from intemperate habits; that he believes a very large number to have been habitual drunkards; and that, although all of these, of course, were deprived of their usual libations, and at once put on prison allowance, only five cases of delirium tremens are found on the register of disease, and that all of these patients but one were admitted to the prison with the disease on them; and that in regard to that one, although entered as under delirium tremens on the day after admission, there is every probability for believing that *she* had had the disease on her when admitted, although not reported to be ill. Dr. Scott also notices, as an important fact, that during the time the railways were being constructed in the county of Dumfries, a very large number of navvies were committed to prison, who had led a very dissipated life for many months, and although deprived of liquor from the moment of apprehension, not a single case of delirium tremens occurred.

Then, as regards the prison of Glasgow, in which the annual commitments amount to upwards of 4,000, the experience of the year 1850 is adduced by Dr. Gibson (*Letter of 16th June, 1854*), as affording an approximation to the facts wished to be elicited. A calculation made in that year showed that, while 4,122 were imprisoned, the number of assaults, with few exceptions, committed under the influence of liquor, and "the drunk and disorderly," amounted to 1,519; and

of this number only three cases of delirium tremens occurred—a very small proportion indeed, especially when it is considered that the debtors, who are almost all habitual drunkards, and drinking up to the moment of incarceration, are not included in this list. Many hundreds more, therefore, may be considered to have belonged to the drunken population of the jail. The average of the last ten years, however, is greater (5.7), there having been fifty-seven cases altogether during that period, but, after all, this is a very small proportion to the number of dissipated and drunken characters gathered together there, and at once broken off from intemperate habits. Dr. Gibson, however, states that he does not altogether enter into my views as to the proximate cause of delirium tremens, although he admits that “it does not so frequently occur as the advocates of the theory which attributes it to the total withdrawal of accustomed stimuli, such as Blake and others, are inclined to suppose;” and he mentions, in proof of his objection, that he had never seen it occur in less than twenty-four or beyond seventy-two hours after apprehension, which necessarily put a stop to dram-drinking. As I have already explained, however, and as the cases given at the conclusion of this paper will show, there is always, whether the individual is drinking much or little, more or less of a premonitory stage present in this affection, distinguished by digestive derangement, nervous irritability, restlessness, and sleeplessness, before much tremor is displayed, or any illusions manifested; and it is easy to suppose that these might not be brought immediately under the notice of the medical officer of a large criminal establishment, such as the Glasgow prison. But even granting that no incipient symptoms of the disease were observed, and that this proportion of the habitual drunkards were not quite on the verge of being affected with it, it is quite in accordance with the views already advanced to suppose that, when there was a certain amount of alcoholization existing, the disease might be hurried on more speedily than otherwise would have been the case in individuals of a nervous and excitable temperament, by the agitation or shock of apprehension, and the deprivation of liberty. But, further, I should suppose it a very just, nay moderate calculation, to assume that out of a population of 2,000 confirmed drunkards belonging to any class of society, although enjoying unrestrained liberty and uninterrupted opportunities for indulgence to excess, at least from three to six instances of delirium tremens would annually occur.

But, in fine, on this point, the evidence communicated by Dr. Simson, the medical officer to the prison board of this city (*Letter 4th July, 1854*), is sufficiently satisfactory; for while the number of civil and criminal prisoners, committed during the last year, was 5,864 (which may be assumed as a sample of the previous fourteen years, over which Dr. Simson’s experience extends), only four cases of delirium tremens occurred within the last eighteen months. The average number of cases during former years, Dr. S. states as from 2 to 3 per annum. Dr. S. considers that at least one-half of the whole prisoners may be assumed as dissipated characters, and that at the very lowest computation, 500 must have been regular systematic drunkards, from whom all drink was suddenly abstracted; and he goes on to state as his decided opinion, that “the sudden taking away of spirits, etc., does not produce delirium tremens. In every case, the prisoner had symptoms of the disease on him when admitted—that is, they were all restless, irritable, etc.; and I have no doubt, but that in many instances the crimes committed were the effects of this disease. I do not remember a single case of delirium tremens occurring when the prisoner was quite well when received into prison. There is not the least doubt that a peculiarity of constitution predisposes to delirium tremens,” etc.

Here, then, it has been shown, that hundreds of individuals among the public at large, and of the criminals committed to our gaols, leave off or are suddenly deprived of the stimulants to which they had been previously addicted, without being seized with delirium tremens, or anything approaching to it. On the other hand, also, it is unquestionable that numerous instances of the disease do occur in which there has been no suspension either voluntarily or by compulsion of the amount of liquor consumed, nay, even an increased excess in drinking up to the very moment of seizure. The assumption, consequently, that this disease is produced invariably, or chiefly, or even occasionally, by the

diminution or abstraction of an accustomed stimulus, is not supported by facts. Any cases, therefore, noticed as occurring under these circumstances, are simply of an exceptional character, but which, in my apprehension, fall quite short of proof from the considerations already so fully explained.—*On Delirium Tremens.*

23. *Protracted Constipation.*—Mr. ANDERSON related to the Harveian Society (Feb. 15th, 1855) a case of this in a child, aged three years and a half, who came under his care on the 21st of June, 1854, and had passed no motion since the 21st of April, although he had taken strong medicine for the purpose of opening the bowels. His abdomen was enormously distended, tympanitic from flatus, but evidently containing no fluid, and respiration was materially interfered with from the mechanical pressure upon the diaphragm. There was occasional sickness from disordered digestion, but no stercoraceous or even constant vomiting to indicate intussusception or any other cause of strangulation. The mother stated that the child had suffered from constipation for the last two years and a half, sometimes having no evacuation for a fortnight, and sometimes for nearly a month, but had never gone so long as upon the present occasion. Purgatives and enemata were used without relief until the 26th, when an examination was made per anum, and a hard globular mass was felt, distending the whole of the rectum, and precluding the possibility of defecation. The sphincter was then dilated gradually, the mass broken down, and removed by means of a spoon and the forefinger; pressure and friction were then applied to the abdomen, when the pent-up bowels began to act immediately, evacuating an enormous quantity of dark-coloured, offensive feces. Aperient medicines now acted, and under their use the bowels were thoroughly emptied, having been completely shut up for more than nine weeks. Tonics in combination with aperients were subsequently employed, and he eventually recovered the natural action of the bowels. This case, yielding so readily to treatment after the removal of the mechanical cause, led to the consideration of several most important points connected with such subjects. A case of Dr. Ridge's was quoted, in which the most undeniable evidences of internal strangulation existed as a cause, and for which, after the unavailing employment of appropriate remedies, the operation of gastrotomy was performed by Mr. Hilton. The question then arose as to whether purgatives were so generally certain in the majority of cases as to render their exclusive use justifiable, and arguments were brought forward in refutation of such a plan of treatment. A case by Dr. Clutterbuck (recorded in the *Lancet*, 1843, vol. ii. p. 957) was mentioned, in which a gentleman, thirty years of age, after violent exercise, was suddenly seized with severe pain in the left iliac region, which soon shifted to the opposite side. This part soon became tender to the touch, and tumefied. Warm baths and purgatives were employed without any effect, and subsequently venesection, which was again repeated after an interval of four hours. Thirty-six leeches were applied to the abdomen. There was no evacuation; the fever was higher, and the pain greater. Croton oil was administered by the mouth, and turpentine per anum; calomel and opium were given freely for twenty-four hours. He vomited, but the matters ejected were not stercoraceous, and he was troubled with urgent tenesmus. Several small watery evacuations took place, still accompanied by the vomiting, which had continued for three days. This symptom ceased for a short time, but soon returned, and the bowels were not again relieved, though purgatives and enemata were employed. The patient died, and, on examination, marks of extensive inflammatory mischief were found in the abdomen; the intestines were agglutinated, and other marks of disorganization existed. There were ulcerated openings in the cæcum and colon; four or five inches of the lower portion of the ileum were sphacelated, and just at the union of the ileum with the cæcum was found a filbert, complete in its shell, floating in fecal matter. Dr. Clutterbuck thought the filbert sufficient to explain all the mischief; for, by acting as a local irritant, it set up inflammation, and this became of the general and destructive character, which examination after death revealed. This case was one of much interest, and particularly as regarded the treatment by purgatives in cases where inflammatory action existed in the abdomen, either as

the result of intussusception or otherwise. Would it not be better in such cases always to make the inflammatory symptoms the object of the greatest attention, and to avoid all drastic purgatives, which he believed were often most baneful? Mr. Anderson then stated that Rokitsky, in giving various causes of obstruction, says that it may be produced by perforations in the mesentery, or by fissures in the omentum altered by disease; he, moreover, particularly objects to purgatives, and advises operation as the sole means of relief. Again, Lawrence, in his *Treatise upon Ruptures*, page 630, states that "the disease, if left to itself, is inevitably fatal." A case was then quoted from Sir Astley Cooper's work on Hernia, page 75, showing that strangulation does occur in an aperture of the mesentery. The patient died with symptoms of internal strangulation; and "upon inspecting the state of the abdomen, the immediate seat of the disease for some time eluded detection, owing to the collapsed state of the intestines, but upon a more accurate and minute examination, an opening was found through the mesentery, forming a stricture, inclosing the inferior portion of the ileum, and which decidedly and sufficiently accounted for the melancholy termination of the case." Plate xvi. Fig. 1, in the same book, is a drawing of the preparation, showing a smaller portion of intestine strangulated than was mentioned in Dr. Ridge's case. In a foot-note at page 77, by Aston Key, the following passage exists: "The possibility of affording relief when the abdomen is laid open, and the obstructing cause exposed, may in some instances be decided in the affirmative; thus constricting bands may be removed, involved portions of gut may be released, an adherent convolution be detached, or a gut distended above the seat of stricture be relieved; but it is to be borne in mind that-gastrotomy will be had recourse to only as a *dernier ressort*, when other remedies have failed, and the symptoms have assumed an aspect promising but little chance of success."

Dr. Ballard mentioned the case of a lady reported to be suffering under uterine disease, but which, in reality, proved to be protracted constipation, which yielded readily to treatment. He also related another instance in which five hundred grains of calomel were given in ten-grain doses, without any specific effect beyond removing the constipation. A large quantity of the calomel passed unchanged.

Dr. Ramsbotham alluded to the frequency of a nucleus existing in such cases, and quoted an instance where a ball of worsted acted in this manner.

Dr. Fuller related a case of intestinal concretion in a lady, from whom a large quantity of feces passed after months of constipation. He advocated the use of electricity and galvanism, and more particularly the employment of nuxvomica for the purpose of counteracting the want of nervous action.

Mr. Borham mentioned a case of a child who had a needle fixed transversely across the rectum, causing obstruction, and forming a nucleus. On removing it, the obstruction ceased.

Mr. Weedon Cooke suggested that strychnine had been used long ago, and that the credit of its original employment was not to be given to the homœopaths. He gave an instance of three months' constipation in a boy, aged ten, which yielded to injections.

Mr. Ure mentioned a case of a gentleman who suffered from constipation in conjunction with retention of urine. He gave him scruple doses of calomel every four hours, a large quantity of which passed unchanged, but the constipation was entirely removed.

Dr. Pollock related an instance of constipation arising from over-stimulation of the bowels by medicine. This was left off, and an electro-magnetic current employed for five months. He strongly advocated the use of small doses of aloes, or ten grains of sulphate of magnesia every four hours.

Dr. Handfield Jones related a case accompanied with severe spasmodic pains and flatulent distension, indicating a want of nervous action, for which he gave nuxvomica. This, in itself, did not produce an evacuation, although it gave relief; but the first aperient administered after it produced a motion.

Mr. Alexander Anderson and Dr. Powell each mentioned a case, the one in which a peach-stone, and the other a quantity of cherry-stones, caused obstruction and accumulation.

Dr. Hamilton Roe was strongly opposed to the use of purgatives in obstinate constipation, and stated that calomel and opium, with injections, had always proved successful in his hands.

The President then mentioned the case of an insane gentleman who had taken large quantities of carbonate of magnesia, which had formed concretions, and could be felt through the abdominal parietes, though a large quantity had been removed. He gave another instance of a lady who had suffered from prolonged constipation. Injections were employed, and a large quantity of feculent matter came away. He considered that no purgatives could act in such cases, and alluded to the value of percussion as a diagnostic, the distended intestine being easily detected by this means.—*Lancet*, March 10, 1855.

24. *Treatment of Acute Rheumatism*.—With certain modifications, according to peculiarity of case, the basis of the treatment of acute rheumatism now most generally pursued in the London Hospitals, consists in the exhibition of the neutral salts. It is rare, indeed, to see a case treated, into the prescriptions for which neither the acetate, tartrate, or nitrate of potash have entered. The congratulatory remarks which are frequently made at the bedside by physicians of long experience, must be considered as strong evidence of the good effects of the improved practice. The disease no longer needs “the six weeks and patience” which it once required. Lemon-juice still holds a high place in the esteem of some physicians, among whom we might mention Dr. Burrows, Dr. Ridson Bennett, and Dr. Rees; while many others employ it as an adjuvant without relying on it alone. That in certain cases it exerts almost no appreciable influence, is generally admitted; while that, in others, it not unfrequently cures like a charm, is equally certain. What we desiderate is, an appreciation of the class of cases in which it, and in which other remedies are severally most likely to be of use. Until that is more or less known, the only refuge is in complexity of prescription, an expedient always to be regarded with distrust, but not to be shrunk from when required by duty. An illustration of the occasional uselessness of lemon-juice was afforded by a man under Dr. Babington’s care, about a year ago, in Guy’s Hospital. He was a man aged 47, and was admitted in the third week of an attack of acute rheumatism, for which he had kept his bed, and had consumed, during the last four days, no less than seven pints of lemon-juice without the least relief. The juice had been obtained directly from the fruit, of which he had used eighteen large ones daily. Dr. Barlow, of Guy’s Hospital, is accustomed to state to his clinique, that the most rapid recovery from acute rheumatism that he ever witnessed was under treatment by the acetate of potash. We may quote the following case as a fair example of the usual treatment pursued by that excellent practical physician, and also of its average results. The plan of treatment is, however, by no means peculiar to Dr. Barlow. H. T., a strong, robust man, aged 33, was admitted on the third day of his first attack of acute rheumatism. The disease was severe. Ordered to take a draught containing half a drachm of acetate of potash, ten grains of the nitrate of potash, ten minims of the vinum opii, diluted with barley-water. For nine days he was kept on low diet, and on the tenth the improvement was so far advanced that the decoction of bark was substituted for the barley-water in the prescription, and a better diet was allowed. On the fourteenth day he was convalescent, about the ward, and marked for discharge in a few days.—*Med. Times and Gaz.*, 3d March, 1855.

25. *Treatment of Acute Rheumatism by Large and Frequent Doses of the Bicarbonate of Potash*.—Dr. A. B. GARROD, in a paper read before the Royal Medical and Chirurgical Society, February 13, 1855, observed that he was induced, in May, 1852, to try a new method of treating acute rheumatism; and, finding great success at first, resolved steadily to pursue the plan, and has done so up to the present time. The object of his communication has been to record the method adopted by him, and also the results obtained in fifty-one cases of rheumatic fever, which have been admitted, under his care, in University College Hospital, during the last two years and three quarters. The main part of his plan of treatment consists in the administration, in a diluted form, of two-

scruple doses of bicarbonate of potash, every two hours, day and night, until the patient has been free from all articular affection and febrile disturbance for two or three days, using local depletion over the heart's region, if any cardiac disease is present or threatened. The author then detailed three cases of rheumatic fever, illustrating this mode of treatment: The first, a girl, 10 years old, in which the duration under treatment was five days, the total duration eight; the second, a young man, aged 20, with a complication of heart disease, where the duration under treatment was eight, the total duration fifteen days; the third, a young woman, aged 18 years, in the fifth attack, the former ones having always lasted for a month or five weeks, but which, by the adoption of this plan, yielded in nine days; total duration being but thirteen days, four having elapsed before her admission into the hospital. He afterwards gave a table of fifty-one cases of acute rheumatism; and of each patient the following particulars are noted: The age, occupation, hereditary predisposition; the number and causes of attack; the symptoms before admission; the symptoms during treatment; the nature of treatment; and the duration of the disease. From these cases, the following deductions are made, viz: that in twenty males the duration of the disease under treatment averaged between six and seven days, and the total duration between eleven and twelve days; and, in thirty-one females, the disease under treatment averaged from seven to eight days, and the total duration between fifteen and sixteen days—giving, in all, an average under treatment of seven days and a-half; and, for the total duration, about thirteen days and a-half. The author then alluded to the influence of the bicarbonate of potash, when administered in large and frequent doses, upon the different organs and functions of the body; and remarked, that it produces neither nausea, vomiting, nor purging; in fact, no symptom of gastro-intestinal irritation. It now induces a strongly alkaline condition of the urine, causes it to effervesce freely, with excess of acid, but does not appear to promote an increase in the quantity of the secretion. It appears to render the secretion of the skin less acid, sometimes almost neutral. That it acts as a powerful controller of the heart's action, reducing greatly the frequency of the pulse, but without causing the faintness often produced by digitalis, colchicum, etc. That it probably increases the alkalinity of the serum of the blood, and diminishes the coagulability of the altered fibrin occurring in rheumatic fever; and hence, probably, checking or preventing the deposits of lymph on the endo- or peri-cardium. He (Dr. Garrod) stated his opinion, that the influence of the bicarbonate was felt not only in shortening the duration of the articular affection, but also in preventing or moderating the cardiac disease. After enumerating many details of the method adopted, and the value of certain adjuncts, as opium, calomel, and occasional general depletion, he proceeded to recommend a plan of treatment which, from his experience, he considered calculated to insure the greatest amount of success, and thought it probable that the total duration of the disease might, on the average, be reduced to about ten days, provided that the treatment was adopted early, and no serious complication existed.—*Med. Times and Gaz.*, 3d March, 1855.

26. *New Method of Treating Neuralgia by the direct application of Opiates to the Painful Points.*—Dr. ALEX. WOOD, in an interesting paper in the *Edinburgh Medical and Surgical Journal*, for April last, expresses his conviction that “An immense improvement was effected in our treatment of neuralgic affections, when M. Valleix directed attention to the fact, that while, on the one hand, the superficial nerves of the body are of all others the ones most commonly affected with this disease, there are some points of their course in which it is much more liable to be seated than in others, although in these, no structural alterations can be discovered to account for this liability. These points are usually more or less morbidly sensible to pressure, even in the intervals between the attacks of the sharp lancinating intermittent pain. A very slight touch in these situations is often sufficient to excite acute suffering; in other cases, however, even firm pressure is borne without any complaint. The points in the course of any nerve which are thus liable to be the seat of tenderness are, according to Valleix:—

- "1. The place of emergence of the nervous trunk.
- "2. The point where a nervous twig traverses the muscles to ramify on the integuments.
- "3. The point where the terminal branches of a nerve expand in the integuments.

"4. The point where nervous trunks become superficial during their course.
 "It is perhaps scarcely necessary to remark that all these points are precisely those where the nerve tends towards the surface, and therefore where, of course, it is the most amenable to local treatment.

"Acting on the result of this observation, M. Valleix introduced a plan of treatment which, as an external remedy, I have largely employed ever since my attention was first directed to his work in 1842.

"It consists in the application of a succession of small blisters *over the points* in the course of the nerves which are painful on pressure. Valleix does not recommend, as a general rule, the application of morphia endermically, but suggests that it may be attempted with advantage in some cases. I have almost invariably employed an ointment containing morphia to dress the blistered surface, and have been accustomed to ascribe much of the benefit of the treatment to this. In some cases, I have seen relief follow the application of an ointment containing strychnine to the blistered surface; but this must be used with great caution, as very disagreeable results often ensue from its use.

"It has frequently occurred to me, however, that a more direct application of the narcotic to the affected nerve, or to its immediate neighbourhood, would be attended with corresponding advantage, and as the painful points so frequently correspond with those in which the nerve becomes superficial, I thought this might perhaps be accomplished. In pursuit of this object, I have made several attempts to introduce morphia directly by means of acupuncture needles and otherwise, but without success.

"Having occasion, however, about the end of 1853, to endeavour to remove a *nævus* by injection with the acid solution of perchloride of iron, I procured one of the elegant little syringes, constructed for this purpose by Mr. Ferguson, of Giltspur Street, London. While using this instrument for the *nævus*, it occurred to me that it might supply the means of bringing some narcotic to bear more directly than I had hitherto been able to accomplish on the affected nerve in neuralgia. I resolved to make the attempt, and did not long lack opportunity.

"Miss —, an old lady, who had laboured long under gastric and nervous symptoms, had suffered severely for four days from cervico-brachial neuralgia. This lady had the idiosyncrasy of not being able to take opium. Of this she had warned me many years before, when she first came under my care, and I consequently never prescribed it for her; however, once, when she was seen with me by the late Dr. J. H. Davidson, he, disbelieving her former experience, prescribed opium, with the effect of bringing on a severe fainting fit.

"The narration of her case may date from November 26th. She had not been able to sleep for the three previous nights from the violence of the neuralgic pain, and was quite exhausted with severe suffering. The usual internal remedies, with the exception of opium, had been tried, but without the least alleviation of her agony. Under these circumstances, I resolved to put in practice the plan which I had so long revolved in my mind.

"Accordingly, on November 28th, I visited her at 10 P. M. to give the opiate the benefit of the night. Having ascertained that the most tender spot was the post-clavicular point of Valleix, I inserted the syringe within the angle formed by the clavicle and acromion, and injected twenty drops of a solution of muriate of morphia, of a strength about double that of the official preparation.

"In about ten minutes after the withdrawal of the syringe, the patient began to complain of giddiness and confusion of ideas; in half an hour the pain had subsided, and I left her in the anticipation of a refreshing sleep.

"I visited her again about 11 A. M. on the 29th; was a little annoyed to find that she had never wakened; the breathing also was somewhat deep, and she was roused with difficulty. Under the use of somewhat energetic stimuli,

however, these symptoms disappeared, and from that time to this the neuralgia has not returned."

Dr. Wood relates several other cases in which he resorted to this method of treatment with advantage. He has satisfied himself, however, that in those not unfrequent cases where the disease has a central, not a centripetal origin, it is quite useless.

The following are the conclusions at which Dr. Wood has arrived from his experience with this mode of treatment.

"1st. That narcotics injected into the neighbourhood of the painful point of a nerve affected with neuralgia, will diminish the sensibility of that nerve, and in proportion diminish or remove pain.

"2d. That the effect of narcotics so applied are not confined to their local action, but that they reach the brain through the venous circulation, and there produce their remote effects.

"3d. That in all probability what is true in regard to narcotics would be found to be equally true in regard to other classes of remedies.

"4th. That the small syringe affords a safe, easy, and almost painless method of exhibition.

"5th. That, destitute as we are of any precise experiments as to the applicability of cellular tissue as a medium for the reception of medicinal agents, the experiments made with the syringe show that it seems to offer an excellent surface for the absorbent action of the venous system.

"6th. That the method now detailed seems as extensively applicable as any of the methods of applying remedies to the skin, whether epidermic, iatraplectic, endermic, or by inoculation."

27. *Treatment of Cholera in the Stage of Collapse, by the Artificial Production of Peritoneal or Cellular Dropsy.* By BENJ. W. RICHARDSON, M.D.—We extract the following remarks on this subject from a paper in the *Association Medical Journal*:—

"The more I have considered the matter, the more thoroughly have I been confirmed in favour of the now generally received opinions, that the symptoms of collapse are due to the loss of fluid which the body has sustained; that the poison of cholera, like a dose of a violent purgative, has a limit to its action on the body; and that if a patient can sustain the loss caused by the dose of the poison he has received, or if he can be sustained artificially from that loss, his recovery is to a great degree secured.

"A rapid removal of fluid must not always be considered in regard to quantity, because all physiological learning goes to prove that a certain balance between the densities of arterial and of venous blood must be kept up, to secure for the system many processes upon which its existence directly depends.

"Now, in cholera, as the serous loss must be derived mainly from the arterial circuit, it is obvious that a very few copious discharges from the stomach or intestines lead quickly to the destruction of the balance that ought to exist, speedily arrest the course of the circulation, and of necessity interfere with the respiratory process, the generation of animal heat, the function of the nervous system, so far as they depend on the blood, and with every other process which we consider essentially vital. . . . Now, in observing a patient in the collapsed state of cholera, . . . one sees at once why so much of incessant failure attends many of those remedial measures which are commonly adopted.

"Thus we see that the application of external warmth over and above the natural animal temperature, and for the purpose of increasing the heat of the patient, is a proceeding at once useless and unphilosophical; since the body does not absorb caloric from without, but makes it from within, and diffuses it externally. And, as the coldness of cholera arises solely from the fact that the internal chemical process, by which heat is produced, is arrested, it is obvious that the only means by which a new supply of heat can be obtained, will be by endeavouring to rekindle the natural calorifying influence. It is of course strictly physiological to surround the body of a cholera patient with a temperature somewhat above the ordinary point of animal heat, or by a non-conducting material; for this is a conservative step, tending to retain such small amount

of caloric as may be generated within; but to do more—to heat the atmosphere of the room, to shut off currents of fresh air, without which respiration is a nullity—is a system directly opposed to our knowledge of first physiological laws.

“Again, no one who has observed cholera much, can fail to be convinced of the frequent inutility of drenching a patient with large amounts of simple liquids or medicines, during such times as copious vomiting and purging are going on. For there can be little or no absorption by a membrane, at a time when it is pouring out excreted matters from every point of its surface; nor can we suppose that, even in the last stages of the disease, when from sheer want of fluid to exude, the watery evacuations have ceased, the membrane from which they have all proceeded, and of whose epithelium immense portions have been washed away, is in a very happy condition for the imbibition of substances, medicinal or otherwise. And, indeed, we find in practice that even the most powerful styptic remedies prove of little avail in arresting the discharge, unless given in that early stage of the disease, when their retention is secured.

“As, then, the loss of fluid from the system is the cause of the collapse, as in many cases it cannot be replaced by the natural process of imbibition from the intestinal surface, and as the blood cannot circulate and reaction occur, without such replacement, there is open before us these plain and simple physiological problems. Are there any other means by which fluid can be introduced into the exhausted circulation? and, if so, which of these is the safest, and most calculated to effect the object desired?

“We have seen that the mucous membrane of the alimentary canal, while carrying on an extensive excretory process, cannot invariably be made a medium for absorbing fluid to any great extent. Some practitioners have, however, believed that the administration of large quantities of fluid by the mouth, during the intervals of rest from vomiting, has been attended with benefit; and there surely can be no objection to a plan so reasonable, unless the swallowing process should itself excite vomiting, and especially as patients themselves almost invariably crave urgently for drink.

“The most important step that has as yet been made on the human subject towards supplying the exhausted system with fluids, in cholera, is that of throwing a considerable quantity of fluid directly into the circulation through the veins; and it cannot be denied that, in practice, this plan has been attended with some amount of success. That it should not be generally successful is, I think, strictly explainable on physiological grounds. In the first place, the plan is objectionable on the mechanical argument, that to throw a large amount of fluid through a vein directly into a heart itself exhausted, is sufficient to paralyze that organ to a serious degree; for I have found, by direct experiment, that a weakened heart often succumbs instantly from over-distending it with blood itself.

“Another objection to the direct transfusion of water, or of water impregnated with saline or other materials, into the circulation, is, that such fluid is not the proper stimulus for exciting the heart to contraction; and that, even if the heart does contract upon it, and throws it round the pulmonic circuit, it is, again, not a substance upon which the process of respiration can properly act. When a saline substance, injected into a vein, accomplishes anything, it does so, I imagine, by dissolving the half-clotted blood in the cardiac cavities, and by carrying the blood, thus diluted, round the lungs.

“But the last and main obstacle to the success attending injection into a vein is, that by this process no such competent amount of fluid can be introduced so as to produce a permanent effect; and, in truth, we too often find in practice that, after such injection, the patient only rallies for a brief period, to sink again under a continuance of the discharge from the intestines. I do not know what is the largest amount of fluid that has ever been injected into the vein of a cholera patient; nor yet the largest amount of fluid that has been known to be lost during the disease.

“Speaking from general observation, however, I may say that the largest amount of fluid I have ever heard of as having been injected into a vein in the course of a case, is from seven to ten pounds, and the most I have ever seen

injected is from three to four pounds; while I have known a cholera patient to pass off by one vomit from two to three pounds, and repeat this many times in the course of three or four hours.

"In contemplating the subject of transfusing watery matter into the system after the modes I have described, the question occurred to me, whether we ought to confine ourselves to these modes; or whether any other means existed of introducing fluid matters into animal bodies in very large quantities, and in a manner which should secure their absorption. This thought led me to look back upon the pathological characters and treatment of those diseases in which serum is largely thrown out into serous cavities or cellular tissue. I recollected that, in cases of this class (dropsical cases), not only were several gallons of fluid often thrown out of the circulation into the system without immediate danger, but that patients thus situated could tolerate a degree of purgation which would absolutely destroy healthy individuals.

"The idea furnished an important suggestion; and I consequently commenced a series of experiments on animals, which, as they are as yet in an imperfect state, I shall only describe in general terms, reserving the particulars for my next report.

"The experiments up to this time have mainly consisted in exhausting dogs and cats by starvation and violent purgation with large doses of elaterium, and afterwards injecting either their peritoneal cavities or their cellular tissue with large quantities of distilled water, at the blood temperature. The results, in general terms, are as follows:—

"I find that either into the peritoneal cavity, or into the cellular tissue, a quantity of water, varying from a tenth to a fifth part of the weight of the animal may be injected with little risk. The effect of this is to induce a sleepy condition, which lasts from twenty to thirty hours, long before which time all trace of the injection is lost. If more than this is thrown in, the sleep or rather torpor (for the animal only remains quiet, and rouses when spoken to), ends in death.

"Setting aside casualties, I infer from my experiments as they now stand, that into the peritoneum or the cellular tissue of a patient in a proper state of collapse from cholera, water to the extent of at least a tenth, or even a fifth, part of the whole weight of the body might be injected with safety; and that the absorption would be almost immediate.

"The performance of the operation for producing this artificial dropsy is exceedingly simple. The instruments required are simply a scalpel, lancet, or trocar, a common catheter, and an ordinary enema syringe. If the cellular tissue is to be injected, a deep puncture is made with a lancet in some fleshy part, the abdomen is the best, and the blunt point of the catheter is then introduced, and wedged sufficiently far into the cellular substance to allow the skin around it to be firmly grasped, so as to keep it steady in its place. The other end of the catheter being connected with the enema pump, an assistant very gradually throws in the fluid, and, as the tissue distends, gentle compression may be made with the hand so as to force on the fluid under the skin.

"As yet I have had no favourable opportunity of trying the suggestion I have here thrown out in cholera; but that it might be put into application in desperate cases, and might prove advantageous, I have no doubts whatever. The advantages of it would be:—

"First. That an abundant store of fluid would be supplied, which the exhausted circulating system would rapidly and effectually take up.

"Second. That as this imbibition would take place through the capillary system, the fluid would enter the veins freely charged with the solid constituents of the blood, and would pass to the lungs in a condition suitable for respiration.

"Third. That the process would in no way interfere with the administration by the mouth of astringent solutions, or such other medicines or liquids as the practitioner might consider indicated.

"But, although I am thus sanguine on this subject, I wish it to be understood that the first trial of this process should commence in a case where the

patient is in the last stage of collapse, where other remedies have failed, and where death is imminent.

"I should scarcely omit to refer, in conclusion, to the fact (though it is one pretty generally understood), that impostors, in feigning diseases, have been known to inflate their own cellular tissue or that of their children with air, without any harm following; and the authors of the article, 'Feigned Diseases,' in the *Cyclopædia of Medicine*, make special reference to the statement, that some French conscripts were known to inject water into their own peritoneal cavities to produce a factitious ascites; while numerous instances have lately been recorded, in which, after the operation of tapping, such irritating substances as iodine and brandy have been thrown into the peritoneum without leading to fatal results."

28. *Injections of Milk or Milk and Water into the Peritoneal Cavity or Cellular Tissue, or Venous System in Collapse of Cholera.* By WM. BIRD HERAPATH, M. D.—In a paper read to the East Surrey Cholera Society, and published in the last number of the *Association Journal*, by Dr. Richardson, a proposal is made to inject the peritoneum and cellular tissue of cholera patients in the stage of collapse with large quantities of water, for the purpose of rapidly supplying the loss of serum experienced by the excessive discharges from the intestinal mucous membrane. This extremely philosophical and ingenious suggestion is certainly highly deserving of a mature consideration, and a careful digest of properly conducted experiments. But it has occurred to me, whilst reading these remarks, that the injection of a fluid more closely approaching the character of serum in its chemical constitution, would be more likely to give permanent benefit, and avoid the chances of destruction of the blood-corpuscles, occasioned by the difference existing between the specific gravity of their contents and of the rapidly imbibed water.

The most readily obtained liquid, having all the qualities we can desire, is most assuredly cow's milk; it is always at hand in any quantity, whilst its tendency to coagulate may be obviated by adding a little solution of carbonate of soda or potassa; perhaps about one scruple of the salt to a pint of milk would be sufficient.

The only difficulties about the matter would be the adulterations to which it may be subjected by fraudulent dealers, and the accidental presence of foreign bodies.

The entrance of foreign bodies into the circulation, or into the cavity of the peritoneum, or the meshes of the cellular tissue, may be easily prevented by attaching a fine muslin or gauze filter, or sieve, to the mouth of the injecting syringe. It remains to be proved whether this fluid would be absorbed by the peritoneal vessels as readily as water, or with sufficient facility to be of service.

The specific gravity of good pure milk varies from 1.041 to 1.033 or 1.020; serum varies from 1.026 to 1.037, and even 1.050, according to the presence of health or disease. Now to produce the difference in specific gravity required by the laws of endosmosis to act in a state of health, water may be added to the milk. But the *viscosity of cholera blood* would assuredly indicate an increase in the specific gravity, and no dilution would be *necessary* in this disease; but if it were adulterated with water only, it would be a matter of no great importance.

It seems highly probable that milk, or milk and water, would be a much more successful fluid for this purpose than water only, and would certainly offer many great advantages, especially if the injection were to be made directly into the venous system, as the corpuscles of the blood do not suffer any material alteration in form, when examined microscopically, after dilution with milk; they suffer nothing from the admixture, especially if the milk is obtained from an animal of the same kind as the blood experimented on, and if the milk used be pure and unmixed with water.

I apprehend also that the introduction of an albuminous constituent is essentially necessary, to supply the waste of this vital pabulum experienced during the exhaustive discharges of this disease.

It is quite a question whether the subsequent symptoms and fever, exhibited during the recovery from collapse, do not depend as much upon the loss of the albumen and salts of the serum, as upon the great difference subsequently existing in the relation between the quantity of the *solids and watery fluid* of the blood.

The chemical constitution of milk does not differ very materially from that of the chyle obtained by healthy digestion, which would of course be the only means *nature* would employ to regenerate slowly the lost liquor sanguinis.

Art physiologically directed comes to the aid of nature, and by employing her own Divine laws, assists her early efforts and wonderfully aids the cure—*gains time*, an element of vital importance in this marvellously rapid and fatal disease.

Nature would then merely have to regenerate the lost epithelial cells, which would of course be a work of time; and no efforts of art could remedy this defect; if the amount of the epithelial exfoliation had been very excessive, the exhaustive discharges of the choleraic diarrhœa would go on in spite of all our injections; for the intestinal mucous membrane would then be in the condition of the dermis denuded of its epidermis by artificial vesication; exosmosis must occur by serous transudation until the protective covering had been reformed, when endosmosis would again recommence from the re-establishment of the physiological action of these wonderful cells.—*Association Medical Journal*.

[In recording the above communication, we would merely allude to the effects which have followed the use of milk in the only instance, we believe, in which it has been injected into the human body—for hydrocele; in this case the milk coagulated, gave rise to serious ill consequences, and had to be removed by free incision, in the form of curds, mixed with a great quantity of pus. Sir Astley Cooper, who records this circumstance in his "Lectures," merely adds: "Be upon your guard, therefore, against making experiments of this kind."—Ed.]—*Dublin Hospital Gazette*, September 15, 1854.

29. *Treatment of Spermatorrhœa*.—M. TROUSSEAU thinks that the advantages of Lallemand's *porte caustique* have been considerably overrated, and that there are only certain cases in which its use is productive of benefit. It is very useful where chronic urethritis co-exists with the spermatorrhœa; but where that is absent, he thinks we ought to trust to other modes of treatment more suited to the cause of the disease.

The excessive debility induced by spermatorrhœa demands our most serious attention. If, in serious cases, we find neither urethritis nor cystitis present; if we discover neither calculi nor ascarides, nor any other thing which can explain the persistence of the emissions, we ought to ask ourselves whether the disease does not depend upon a condition of the vesiculæ seminales analogous to the spasmodic state of the bladder in certain forms of incontinence of urine. Puerile enuresis is not due to atony of the bladder, or to any undue accumulation of urine, but to a spasmodic condition of the bladder. The same phenomenon occurs in the vesiculæ seminales; and belladonna, which acts so beneficially in the case of the bladder, is also very useful in this other spasmodic condition. M. Trousseau prescribes accordingly, in such cases, powders containing each 1 centigramme of the powdered root of belladonna, mixed with sugar. He orders one to be taken daily during the first week of treatment; two daily during the second, and so on until the patient experiences a sensation of dryness in the throat. At the same time, he orders frictions of the perineum with an ointment composed of 10 grammes of the alcoholic extract of belladonna to 20 grammes of axunge. If necessary, he also uses suppositories containing each 10 centigrammes of the extract. belladonnæ.

M. Trousseau doubts the utility of cold hip-baths in this affection. They may do good the first time they are used, but, although they may temporarily arrest venereal excitation in nymphomania and priapism, this calm disappears on the occurrence of reaction, and the evil is increased.

Heat acts in an opposite manner. Hence M. Trousseau believes that, in cases where erotic feelings are conjoined to spasm of the vesiculæ seminales, it is the best sedative which we can employ simultaneously with belladonna. The

form in which he employs it is that of bags of heated sand, which he applies to the perineum for a few minutes, morning and evening. The simultaneous administration of lupulin may be very beneficial; but, where we desire decided anaphrodisiac effects, M. Trousseau recommends, on account of the certainty and efficacy of its action, the bromide of potass, in doses of from 15 grs. to 3ss. daily.—*Monthly Journ. of Med. Sci.*, March, 1855, from *Journ. de Med. et de Chirurg. Prat.*, December, 1854.

30. *Gallic Acid in Pyrosis*.—Dr. BAYES says that, in pyrosis, where this disease is unaccompanied by extensive ulceration, or organic malignant disease of the stomach, or by disease of the liver, the most marked benefit will follow the use of the remedy. Gallic acid, here, not only checks the secretion with a certainty and rapidity he has never seen follow the administration of any other remedy, but it gives general tone to the stomach, increases the appetite, and (what I very little expected when I first used it) in many cases removes constipation. This I can only account for on the supposition that the relaxed atonic state of the stomach which favours pyrosis is continued throughout the alimentary canal, the constipation in these cases arising from want of power in the muscular coats of the intestines to expel the feces. The want of tonicity is remedied by gallic acid.—*Association Med. Journ.*

31. *Sal Prunella in Polydipsia*.—M. DEBOUT observes that polydipsia is an affection of more frequent occurrence than would be supposed from the silence of most pathologists. Scarcely a year passes without examples being met with in the Paris hospitals; the patients usually, however, applying for some coincident affection. So great has been the difficulty of relieving it, that from among twenty-six cases, collected by M. Lacombe from different authors, only two were cured, and perhaps not permanently.

The slight specific gravity of the urine, and the absence of sugar, distinguish the affection from diabetes, and there is no example of simple polydipsia having terminated fatally. Its pathogenetic conditions being unknown, we can only treat it empirically, and M. Debout relates the case of a man, aged 24, who, the polydipsia having commenced three years previously, after exposure to the sun during great exertion, drank between four and five gallons of fluid per diem. When seen by M. Debout, he was the subject of acute rheumatism, and 450 grains of nitr. potassæ in four quarts of tisane were ordered him in the twenty-four hours. For the first time since three years, so small a quantity of fluid sufficed to stay his thirst; and both rheumatism and polydipsia became cured together, no return having occurred when he was seen a year later.

On searching, M. Debout found that sal prunella had already been given by Rivierius and T. Frank, and in his next cases he administered this in doses of 60 grains per diem. Rapid melioration was almost always the result; and, although this has not been usually permanent, it has again been procured on recurring to the remedy. At all events, nitrate of potass would seem to be the best remedy yet discovered, and sal prunella the best form in which it can be given.—*Bulletin de Thérapeutique*, tom. xlviii. p. 97.

32. *Pomade of Proto-Sulphate of Iron in the Treatment of Skin Diseases*.—M. DEYERGIE recommends the use of this remedy in those affections of the skin which in their essence are secreting, and which generally occur in lymphatic temperaments and constitutions, observing that its curative results are especially decided in eczema and eczema impetiginodes. Similar success attends their use in impetigo and intertrigo; and in eczema of the scrotum, often an obstinate affection, he had met with remarkable success from this application. It is a useful application, also, to the ulcerations which succeed to the vesicles or pustules of rupia and ecthyma cachecticum. It is useless in the scaly affections, and absolutely injurious in the bullæ, and in those essentially vascular, as pemphigus and herpes with large vesicles. It is also injurious in acute ecthyma, acne rosacea, and mentagra. In addition to being resolvent, M. Deyergie considers that it modifies the vitality of the diseased tissues, a property which does not belong to tannin, oxide of zinc, calameus, or alum. The fol-

lowing is the formula employed: Axunge 30 grammes (seven and a half drachms); proto-sulphate of iron crystallized and washed, 50 centigrammes to 1 gramme (eight to fifteen grains); dissolve the salt in a few drops of water, and incorporate it immediately with the axunge. Keep it from contact with the air.—*Bulletin Gén. de Thérap.*, 1854, p. 553.

33. *Employment of Alkaline Carbonates in the Treatment of Membranous Angina.*—M. MARCHAL DE CALVI advocates the employment of alkalis in this sometimes most intractable disease, and cites cases treated by the bicarbonate of soda, in which the recovery was rapid, immediately following its administration. In the first case, the mucous membrane, at the back of the throat, was very red and œdematous; the mucous membrane, too, covering the palate and tonsils, was covered with stripes and patches of false membrane. This exudation was beneath the epithelium, for it could not be scraped off with the nail. Deglutition was performed with considerable pain. Twelve leeches were first applied to combat the inflammation, and three drachms of bicarbonate of soda were ordered to be divided into twelve packets, one to be taken every half hour, in a spoonful of water. Upon returning, after an absence of four hours, he found that the leeches had drawn a large quantity of blood, which was decidedly less plastic than natural. Upon examining the throat, the false membrane was found to have disappeared entirely. This the author attributes to the administration of the bicarbonate of soda, whose action he thus explains. Its first or chief action is to render the blood less plastic. It is with this object in view that the medicine is prescribed; but there seems to be another mode of action in this peculiar disease, which M. Suresure has supposed likely to exist, viz: a local action; which may account more satisfactorily for its rapid action in the case just recorded.—*Dublin Hosp. Gaz.*, 15th May, from *Gazette Médicale de Paris*.

34. *Inhalations of the Fumes of Opium in Coryza.*—Dr. LOMBARD, of Geneva, has found that in those severe cases of coryza which are accompanied by great pain and sense of weight in the frontal sinuses, the inhalation of the fumes of burnt opium affords the patient the most marvellous and speedy relief. The pain ceases as if by enchantment; and the patient passes from a state of misery into one of comfort. Dr. Lombard recommends a few grains of powdered opium to be thrown upon a slip of metal, previously heated in a spirit lamp; and the patient is desired to hold his head over, and forcibly to inhale the fumes of the drug.—*Gaz. Médicale*, July, 1854.

35. *Chloroform for the Treatment of Lead Poisoning.*—Chloroform is recommended by M. ARAN, Physician to the Hôpital St. Antoine, in Paris. He has just published a somewhat lengthy paper on its use, in which he gives some very interesting illustrative cases. One of these occurred in a house painter, æt. 31, who was affected with lead colic for the second time, after having followed his trade for twenty years. There were obstinate constipation, bilious vomiting, pain in the epigastrium, tormina, scanty urine, and a very low pulse—only 48. A compress, on which nearly a drachm of chloroform had been poured, was placed on the umbilical region, and retained there for several minutes. In spite of the sensation of heat and burning occasioned, immediate relief was experienced by the patient. In addition to this, a sulphureous bath was ordered; a mixture containing 30 drops of chloroform was administered; and a domestic enema was prescribed, to be followed by a small clyster, with 20 drops of the anæsthetic in it. This treatment was continued for a week with great benefit, the chloroform being given morning and evening; and sixteen days after admission the patient was discharged cured. Rather an interesting incident occurred in this man's case; he swallowed, one day, by mistake, nearly an ounce (30 grammes) of chloroform. Symptoms of narcotic poisoning appeared, but the patient recovered satisfactorily. Several other cases are given, in which the same treatment proved very efficacious.

M. Aran says that the compress saturated with the chloroform should be covered with dry compresses, and that it should be allowed to remain in contact

with the skin from two to five minutes. After the preliminary large doses, the following mixture should be used: *R.* chloroformi gtt. 20-50; gum. tragacanth ʒj; syr. simplic. ʒij; aquæ ʒiij. Misce. Dose, a teaspoonful frequently. He gives the following formula for the chloroform enema: *R.* Chloroformi gtt. 20-50; gum. tragacanth ʒij; ov. vitelli, j; aquæ ʒiv. Misce.

The patient should be desired to endeavour to retain this enema as long as possible.—*Monthly Journ. Med. Sci.*, April, 1855, from *L'Union Médicale*, Jan. 1855.

36. *Iodide of Potassium for Cure of Lead Poisoning.*—It has been asserted by MM. Natalis Guillot and Melsens that the administration of the iodide of potassium is the best means for ridding the system of lead and mercury, inasmuch as the insoluble compounds which the salts of these metals form with the organic substances are all soluble in the iodide of potassium, which is a salt very easily and rapidly eliminated from the animal economy. Mialhe denied these assertions, which were founded on experiments made on dogs. M. MALHERBE, chief physician to the Hôtel-Dieu, at Nantes, has recently tried the drug in some cases of lead poisoning, with great benefit. He never gave more than 15 grains daily, but often much less. His experience of the remedy has not been sufficiently extensive to enable him to arrive at definite conclusions; but he thinks, from the trials he has made of it, that the following deductions are warrantable:—

1. In lead poisoning, elimination of the metal takes place naturally by means of the urinary organs, which is probably due to the influence of the alkaline chlorides contained in the animal fluids, as has been thought by M. Mialhe. But this eliminative process is not sufficiently rapid to procure a spontaneous cure of lead poisoning.

2. This elimination of lead by the urine is rendered much more active by the use of the iodide of potassium; the excretion of the phosphates by this channel is also considerably increased; and these salts yield an insoluble residue which contains the greater part of the eliminated lead.

3. Although lead is not naturally eliminated by the saliva, the iodide of potassium seems capable of occasioning its discharge through this medium.

Malherbe gives the following rules for the rational treatment of lead poisoning, in which he has arranged the remedies according to their relative importance, and not in the order in which they should be administered, which must necessarily vary in different cases. The indications are:—

1. To eliminate the lead contained in the economy by means of iodide of potassium, which should be administered methodically, and should be given as long as the urine and the saliva give the reactions of the metal.

2. To cleanse the cutaneous surface by means of sulphureous and soap and water-baths; and to purify the mucous surfaces by the internal use of the preparations of sulphur and by purgatives. The latter medicines, by inducing abundant bilious evacuations, doubtless eliminate a part of the lead contained in the liver; but they cannot reach those portions of the metal which are combined with the tissues of other organs.

3. To calm hyperæsthesia and nervous symptoms in general (as epilepsy, delirium, convulsions, coma) by narcotics, and especially by belladonna, which, in addition to its sedative properties, is possessed of a cathartic action which often renders the employment of purgatives unnecessary.

4. To combat paralysis by means of strychnia and electricity.—*Monthly Journ. Med. Sci.*, April, 1855, from *Gaz. des Hôpitaux*, 21st Dec. 1854.

37. *Employment of Wine as an Enema.*—M. ARAN observes that, in many cases of dyspepsia, chlorosis, phthisis, and convalescence from severe disease, the use of wine is strongly indicated, whilst the heat and irritation of the digestive organs it induces absolutely prevent its employment. Having found vinous enemata of great utility in obstinate diarrhoea, he tried them in various other cases, and for the last three or four years he has administered them whenever slow convalescence has been united with irritability of the digestive organs. He has also obtained great and unexpected advantages in phthisis,

having at first administered the wine on account of the accompanying diarrhoea. When the disease is advanced, little, or only temporary, benefit results; but, when given at an earlier period, and associated with cod-liver oil or other means, the wine does exert a salutary influence that merits attention. In gastralgia, while the general debility seems to indicate the use of tonics, yet wine cannot be given, even in small quantities, without exciting a sensation of burning; but in several examples of this the vinous lavements have not only restored strength, but have so diminished the gastric irritability as to allow of a little wine being taken with the food. Another form of dyspepsia alike susceptible of benefit is vomiting, with the rejection of alimentary matters—the strength and *embonpoint* rapidly returning under the use of the enemata.

But the affection in which M. Aran has been most surprised at the amount of success obtained is chlorosis. Influenced by the so prevalent opinion that iron is the specific remedy, he at first gave it in conjunction; but he afterwards omitted it, only prescribing, besides the wine, general frictions with an ammoniacal liniment, and a powder consisting of bismuth, rhubarb, and valerian. In a few days, the patients seemed to have recovered their normal health just as if iron had been given, and that even in severe cases. In their relation to iron, cases of chlorosis may, indeed, be divided into three categories. In the first and most numerous, iron properly administered furnishes a durable cure. In others, a rapid amelioration ensues, which is followed by a relapse on its discontinuance. Temporary amendment attends its resumption, and so on for years, iron thus becoming a constant necessity. Thirdly, there are not a few cases in which iron fails altogether, or in which the amelioration never rises to the dignity of a cure. M. Aran has also advantageously employed the injections in various other conditions characterized by debility, and especially in paludal, syphilitic, or cancerous cachexy, in some cases of anasarca, and in different circumstances in which alcoholic stimuli are indicated.

The lavements are found to act advantageously, in proportion as the individuals have been previously unaccustomed to alcoholic drinks. Thus, they are, in general, more useful in women than in men, more still in young girls, and more so in the inhabitants of the country than those of Paris. Until the patient becomes accustomed to them, they induce a kind of drunkenness, but very different from that following the swallowing of wine; and if the enema be given in the evening, so that the patient may afterwards remain quietly recumbent, refreshing sleep ensues. It is to be remarked, that effects are produced by a dose of wine that, if swallowed, would induce little effect. It is sufficient, in slight cases, occurring in impressionable persons, to employ 150 grammes; but, in obstinate cases, 250 to 350 grammes may be required. In the chlorotic, it seems much to hasten the cure to divide the dose into two parts, administering one morning and evening. In general, from 150 to 250 grammes suffice, and it is better, if the wine is rich, to commence its use by diluting it with water. Immediately before giving the enema, the rectum should be emptied by a lukewarm injection, and the wine should be employed tepid, so as not to induce contraction of the rectum. At first, the patient should lie down, and endeavour to retain the fluid; but those who are accustomed to the treatment retain it with little trouble in any position.—*Med. Times and Gaz.*, March 17, from *Bull. de Thérap.*, 1855, t. xlviii.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

38. *Discussion on the Curability of Cancer, and its Diagnosis by means of the Microscope.*—[We continue, from p. 520 of our preceding number, the interesting discussion, at the Imperial Academy of Medicine, relative to this subject.]

Nov. 7, 1854. M. VELPEAU said: At the commencement, I advanced two propositions: 1st. That cancer was sometimes curable; and, 2d, that certain cases

of cancer could be recognized without the aid of the microscope. To these questions I mean now to confine myself, seeing that the discussion has embraced too many topics for me to overtake; and I intend also to state by what observations and researches I was led to form the opinions I then expressed.

I have said that the "cell" is not the specific element of cancer, as these gentlemen thought; and that there are epithelial cancers which contain no cells. M. Larrey must have spoken unthinkingly when he appealed to the unanimity of microscopists in this matter, for certain of them admit what others deny. Contrary to the opinion of Lebert and Broca, it is admitted by Mandl, Virchow, Bennett, Paget, and others, that there are epithelial cancers without cells. Microscopists are not all agreed, because cells do not exist in all cancers. There is some other thing in cancer of greater importance than the cell; and in the blood of patients having the cancerous cachexia there certainly exists something peculiar to the condition which we do not as yet know. The cell is one of the manifestations and material characteristics of cancer; but it is not the principle of the disease, or the malady itself. This cancer-cell has been found elsewhere than in cancers, and it existed in a non-cancerous portion of the vesical mucous membrane, lately exhibited to the Société de Biologie. It appears to me not to be a sufficiently constant characteristic to enable us to distinguish the different varieties of tumours, and, therefore, I have tried to diagnose them by other signs. I have examined a very large number of cancers, and admit the occasional, but by no means the constant existence of the so-called cancer-cells. It may be said that the cases where the cells were absent were not true cancers; be it so, I am willing to set them aside as doubtful. But I have seen tumours containing cells which were *not* cancerous. It has been asked whether I consider them to have been benign on account of their non-recurrence, and it has been objected that I cannot base my opinion on this character, if I admit that cancer is sometimes curable, and consequently does not constantly recur; but it is not from the non-recurrence of the tumours alone that I judge, but from the *tout ensemble* of their physical characters and clinical signs.

I affirm, then, that cancer-cells have been demonstrated in non-cancerous growths, and that they are often absent in true cancers.

M. Malgaigne has suggested that in these cases of cancer without cells I have mistaken fibro-plastic for scirrhus tumours. This I have taken special care to avoid doing, and when I speak of cases of schirrus, they were undoubtedly true cases of it. It has been said that the cells were not found, because the whole tumour was not examined. I have never attempted to puzzle the microscopists, and I have always given them the entire tumours for examination. Cancerous tumours, like other pathological specimens, are not equally diseased throughout their whole extent; there is generally a central portion in them in which the disease is most marked; but if we have a section made through this part, it is just as good as if we had the entire tumour. If cancer-cells are not found in such a section, they are certain not to exist elsewhere. I have cited cases of tumours, which contained no cancer-cells at first, recurring afterwards, and exhibiting these cells in their structure. I have seen two or three such cases. In one of these, a man had a cancerous tumour, a portion of which was removed and sent to M. Lebert for examination. Finding no cells in it, he pronounced it to be benign. M. Richet, therefore, operated on the patient; things went well at first, and the cicatrix looked healthy; but ere long the tumour returned and killed the patient. The second tumour contained cancer-cells. Again, a woman had a mass of vegetations on the *left* breast, which resembled neither encephaloma, scirrhus, nor fungus. I removed one of these vegetations, and found that it contained no cancer-cells; but I was not therefore convinced that it was not a cancer. The woman returned after some time, having the *right* breast affected with all the most marked characters of cancer—partial indurations, and retraction of the nipple. She soon died of the affection. M. Robin found cancer-cells in the tumour of the right breast, and also numerous nodules, in the lungs and liver, containing cancer-cells. Can we say that in this case the tumour of the right side was a cancer, while that of the left was non-malignant? Certainly not. Moreover, the cells were found

on the side on which the tumour was least degenerated. M. Leblanc has attempted to explain these differences, by admitting the existence of "mixed tumours;" and he believes that cancer may become developed in a tumour not primarily malignant. Microscopists, however, deny the transformations of tumours. I myself am doubtful as to whether benign tumours may degenerate, and I would not be understood either to affirm or to deny it; for facts have led me to believe that such a change may be quite possible.

It appears to me, then, 1st, *that there are cells without cancer*; and, 2d, *that there are cancers without cells*; and if these facts be once admitted, they go very far to negative the assertions of the microscopists. A new definition of "cancer" is asked; but the remodelling of definitions is a barren and unprofitable work. It is far easier to attack an old definition than to make a new one, even when it relates to something we understand very well. For example, I should be puzzled to define M. Robert with exactitude, yet I can quite well recognize him. I would characterize cancer as a malady, which may appear as an excrescence, a tumour, an ulcer, or a fungosity; the specific nature of which is, when once established, to spread, to invade diseased tissues, and, finally, to cause death.

Let us look now at the tumours which the microscopists refuse to admit as cancers. They deny that epithelial tumours are cancerous, because their tissues are homomorphous, and such a structure, they say, cannot be malignant. They allege that these tumours do not recur, or if they do, that they affect only the original spot, but that they do not affect the lymphatics, or become generalized. Every-day experience, however, shows that these tumours may recur, not only on the original site, but also in the glands; and that their recurrence, far from being exceptional, is very frequent. Hence, M. Lebert and other microscopists have considerably modified their first opinion. Yet they affirm that epithelial tumours never, like cancer, become generalized. Now, M. Robert himself has adduced instances of their generalization; and others are recorded by Paget, Virchow, and Rokitsky, in which they become developed in the heart, lungs, and liver. Yet this, say they, happens very rarely. I do not know; but whether it occurs often or seldom does not at all alter the question. We remove a small tumour of this sort from the lip; at the end of two, three, six, or twelve months, we find that it returns; again we excise it, and again it recurs. Finally, we dare not touch it; and it kills the patient as surely as cancer. Wherein lies the difference between them?

Again, as to fibro-plastic tumours. To constitute them a distinct group from cancers, the microscopists have been obliged to class with them many tumours to which they have no analogy. How can fibro-plastic tumours, which prove fatal to the patients, be confounded with simple glandular hypertrophies? and in what respects do they resemble each other? Although the anatomical structure of both may be similar, there doubtless exists in them some unknown element which constitutes their essential difference. M. Lebert has said that these tumours, like the epithelial, do not recur, or if they do, that it is only locally; but I have shown that gentleman many instances of their reproduction in the viscera. M. Robert has cited cases to prove the non-recurrence of fibro-plastic tumours; but—so great is the blindness which an adherence to a preconceived idea occasions—all these cases occurred before the use of the microscope. For thirty years I have constantly applied the great law of Bichat, cited by M. Robert, to everything in pathological anatomy. But, in pathology, anatomy, though very necessary, is not all-important; for we must also take into consideration the oftentimes obscure nature and causes of disease. There exists in cancer something special, something not anatomical, which has hitherto escaped all the investigation of our senses, and of which the microscopists have taken no note.

Here, then, is my basis for the distinction of tumours. *Cancerous tumours all possess one character in common: they have all a tendency to destroy the tissues by becoming generalized.* All that can be said against this proposition lacks demonstration, and proves nothing.

I turn now to the question of its *curability*; and the first difficulty that presents itself is *diagnosis*. And, first of all, I would remark that it has been

said that I lay pretensions to infallibility of diagnosis, and that I boast I am never deceived. God knows that I lay claim to no such lofty pretensions. I have merely said that there are certain tumours, whose clinical characters are so well marked that it is impossible for *any one* to be deceived. I have taken notes, for the past year, of 120 cases of tumours; 66 of which were cancers. Among these 66 cases, I have removed 49 cancerous mammæ; in every case, the extirpated tumours were examined by the microscope, and in every instance the microscope confirmed the diagnosis I had made before the operation. Let me add, however, that I have never alleged that a correct diagnosis can *always* be made. When I say that in certain cases I cannot be deceived, I allude to those in which the characters are so well marked that any surgeon could recognize them.

I come now to the important point. Why may not cancer be curable? When I advanced facts supporting the curability of cancer, it was said that they were inconclusive, because they were anterior to the use of the microscope. But when, in the 49 cases I have mentioned, which were microscopically examined, my diagnosis was in no instance wrong, how can it be said that I was deceived before the introduction of the instrument? As regards my statistics, it has been said that 20 cures in 50 cases is too good to be true; but I have not alleged that I cured 20 out of 50 patients, but out of 200, one-half of whom died. (Here M. Velpeau cited several cases of persons who were cured of cancer; that is to say, who never had any recurrence of the disease for a greater or less number of years after the extirpation of a cancer—cases which were almost all witnessed and examined by the microscopists.) These facts, said he, are authentic; and the curability of cancer cannot be denied. M. Roberf has alluded to the case of a lady operated on in 1850; but he has not observed that, in detailing this case, I made reservations in respect to it.

As to relapses recurring long after the operation, as after ten, fifteen, or twenty years, may we not inquire whether or not it is possible that cancer, like other diseases, may become twice manifested in the same individual, without this being looked upon as a recurrence? I am always asked why we are unwilling that cancer should be curable? and what interest microscopists have in maintaining that it is so? I have never been able to understand it. Statistics, and the authority of highly respectable names—as Munro, Boyer, etc.—have been cited in support of the incurability of cancer. But the statistics of Munro and Boyer are of great value; those of Hill and Flajani do not militate against mine; and as to the recent ones of MM. Lebert and Broca, they do not appear to me a whit more conclusive, as many of the patients enumerated in them were entirely lost sight of.

What we seek here is truth; but we should not make the obtaining of it depend upon a mere theoretical opinion, since theory alone has given rise to the unbelief in the curability of cancer, which I so much deplore. As to the microscope, I maintain that it has done good service to science; I believe that it will render still more; but I am confident that it has also committed several errors. I accept all the new facts which it has revealed to us; and I only contest the false or doubtful explanations which have been given to them. And as regards the question at present under debate, I think that its resolution should be made with great caution and reserve, inasmuch as there probably exists some special element with which we are not yet acquainted. I have ever encouraged, and I will continue to encourage fresh researches; but I will only accept as true what I find to be satisfactorily demonstrated. I hail the march of progress with unmixed delight, so long as the car of science is propelled, but is not overturned.

Nov. 14, 1854.—The secretary read the following letter, addressed by M. MANDL to the president:—

“Mr. President: M. Velpeau, in his last address, has done me the honour to class me among the microscopists who do not admit the existence of characteristic cancer-cells. I beg to say a few words to the Academy on my reasons for so thinking:—

“Shortly after Müller, in 1839, discovered cells in cancerous tumours, I published my observations in the *Archives de Médecine*. But in 1843, I owned

that the sanguine hopes which I had at first formed of the advantages to arise from the employment of the microscope in the examination of such pathological specimens, had not been realized; and my researches since then have confirmed this opinion. I had soon abundant opportunities of examining tumours which had appeared to clinical observers to be cancerous, from their origin, progress, and fatal termination, but which contained none of the cells considered by Lebert to be characteristic of the malady. I can remember cases of epithelial and fibro-plastic tumours; but I would more particularly direct attention to cancers of the retina. In four out of five tumours of this last kind, extirpated by Sichel and Bérard, it was impossible to discover any specific cell; and yet subsequent general infection left no doubt as to their malignant character.

"On the other hand, in the cells found in cancerous tumours, I have not observed the characters which they are said to present in the majority of cases. According to M. Lebert, the nucleus of the cancer-cell has always a mean diameter of, from the $\frac{1}{1000}$ th to the $\frac{1}{100}$ th of a millimetre. (*On Cancer*, p. 30.) Yet, in encephaloid tumours, I have found the nuclei not above the $\frac{1}{1000}$ th or the $\frac{1}{100}$ th of a millimetre; and in such cases it is impossible to distinguish them from the other nuclei of normal tissues. Again, as to nucleoli, I have often found their characters very imperfectly marked. In M. Lebert's work, we find examples of these inferior dimensions in cancers of the bones, the stomach, etc. As good, and even better observers than M. Lebert, believe that it is necessary to take into consideration all the elements, and also to know the history of the disease, before pronouncing a judgment upon its nature. Does not this show that the cell is not sufficiently characteristic of cancer?

"The truth of this is proved, not only by observation, but also by the laws governing histogenesis. It seems to me that, in order to solve the problem which we are at present studying, it is indispensably necessary that we engage in the comparative study of normal histology, and especially in the study of the development of the tissues. Space precludes me from entering into any detailed explanation of development, so far as it regards this subject; but I shall briefly state the principal results of my researches.

"All the tissues of the organism form two grand series; the one composed of cells and scales (*ou lamelles*), the other of fibres; and this essential distinction characterizes the different elements from their very first appearance. Nuclei placed in a soft amorphous substance, become surrounded, in the first instance, by membranes; they increase in size, multiply, and constitute the cells, of which the glandular, epithelial, and other tissues are composed. In the second class, on the other hand, the nuclei are placed in an amorphous, homogeneous, solid substance, which splits, and goes on subdividing into more and more delicate fibres, so as eventually to constitute a fibrillated tissue. There is no cell-formation in all this series of organic elements, among which may be classed the fibrous, cellular, and serous tissues, etc.

"This principle of development and of texture, inherent in each series of the tissues, is rigorously maintained in all their various physiological and pathological phases. In the normal renovation which accompanies secretion, growth, etc., as well as in anomalous renovation or in regeneration, we always observe that cells are developed in those places where cells originally existed, and that fibres are developed in the other tissues. Although hypertrophy may occur, the same principle continues to preside over the production of the new elements; fibres do not produce cells, nor cells fibres, but cells are always developed in the midst of cells, and fibres in the midst of fibres.

"Facts have proved to me that these principles hold good in the case of cancer. The cancerous diathesis may affect the blastema, which ought to produce either cells in the glands, scales in the epithelial, or fibres in the fibrous tissues; and according to the nature of the tissue infected by the diathesis, the morbid blastema will produce either cells or fibres. The product will then be characterized according to the species of the affected blastema; and we will have either cancer (*cancer à cellules*), fibro-plastic tumours (*cancer à fibres*), or canceroid growths (*cancer à lamelles*). But if it be permitted to us in *most*, though not in *all* cases, to recognize the alteration produced by the diathesis in the cells—that is to say, if we are allowed to recognize the cancerous

cells, and to distinguish them from other analogous elements—why may we not also take into account *fibres* and *scales* as elements of cancerous tumours? So long as we are unaware in what respect fibres and scales are altered when they are produced under the influence of the cancerous diathesis, so long will the microscope continue to give imperfect information to the surgeon in the diagnosis of tumours.

“As to the frequency and gravity of recurrences, all histologists know that there is no element which is increased, developed, multiplied, and regenerated with so much facility and rapidity as the cell. Fibres are much more tardy in the stages of their development. Now, in which of the varieties of cancer are the recurrences the most frequent and the most serious? Which species extends most rapidly, invades neighbouring organs most speedily, and destroys most energetically the whole economy? Undoubtedly, it is the *cancer with cells* (*cancer à cellules*.) The conditions are completely changed when the cancerous diathesis affects a blastema which ought to produce fibres; and when we see it form incompletely developed fibres, we call it the *fibro-plastic element*. Fibres, in general, are developed very slowly. I have seen a regenerated tendon presenting incomplete fibres at the end of seven years; that is to say, there was a blastema in which fibrillar division had not been completed. Thus the microscope is capable of informing the practitioner of the severity of the affection, and it also enables him to pronounce a proper prognosis as to its course, and as to the greater or less probability of its recurrence, inasmuch as it acquaints him exactly with the nature of the elements which constitute the tumour.

“I believe, then, that cancer, capable of occasioning general infection, can exist without the cancerous cell—that heteromorphous element which has been considered to be a peculiar and specific product of the diathesis—being present. This belief of mine has been long ago expressed by M. Velpeau, as well as by Bennett, Paget, Virchow, Vogel, and others.

“The cancerous diathesis may affect cells, scales (*lamelles*), and fibres, and accordingly there are as many different varieties, yet microscopical anatomy agrees with clinical observation in classing them together as one family.”

M. AMUSSAT felt confident that he had cured many very serious cases of cancer, which otherwise must have terminated in a painful and horrible death. The influence of grief appeared to him to be, speaking generally, the most frequent cause of cancer in persons of otherwise good constitution; hence he was led to believe that the seat of cancer lay in the nervous system. He trusted that this discussion on the microscopical characters of cancer would elicit fresh researches, which might elucidate the nature of the disease, and lay the foundation for a rational, efficacious treatment of it. He thought the question of the greatest moment in relation to cancer was, whether or not there existed any hereditary taint. If this was present, the most prompt and energetic measures were necessary. When it was resolved to operate, either by means of the knife or caustic, we should freely destroy the surrounding tissues, as the timidity of the operator often occasioned recurrences. When caustics are preferred to the knife, only the strongest should be used.

M. CLOQUET. Cancer was like inflammation—there were many very different varieties of it; yet all its various forms possessed certain common characters; they never healed spontaneously; they were very prone to recur, and, unless extirpated, they infallibly occasioned death. He considered that the microscopists had not formed a separate school of their own; they belonged to the anatomico-pathological school, and, being possessed of more powerful means of investigation, they had arrived at most important results. To insure our obtaining still more certain and important results, it was necessary that all medical men should unite in strengthening the hands of the microscopists, and in placing within their reach everything which they considered likely to advance the interests of science.

M. DELAFOND proposed to examine the question of cancer in relation to comparative pathology, to surgery, and to micrography.

It had been alleged by M. Leblanc that herbivorous animals were less affected

by cancer than carnivorous; but this proposition was too wide; and he believed that the herbivora were equally subject to its ravages. It was by no means rare to find cancer affecting the teats of mares and the testicles of horses, and it often occurred in the viscera of these animals. Asses and mules were very subject to it. Cancer was common among oxen, especially affecting the jaw. Pigs were very liable to cancer of the teats, and dogs were often affected by this disease. It also occurred among gallinaceous fowls, and many varieties of birds. Therefore, it did not appear that animals deriving their sustenance from the vegetable were less disposed to cancer than those whose nourishment was drawn from the animal kingdom.

He had found, from microscopical investigations, that fibro-plastic tissue recurred like cancer. He did not consider the cell characteristic of cancer. The cancer-cell varied in its appearance according to the stage of the malady. At first, the cells found in a small tumour, not compressed by the surrounding tissues, were of a rounded form, having a nuclei and nucleoli in their centres. Compression subsequently rendered them elongated and ovoid; subsequently, they became still more elongated; and, in scirrhus cancers, compression deformed them still further. In old cancers, they become infiltrated with calcareous matters; their nuclei become flattened, and the cells become thin, like fibro-plastic tissue. Secondary cells also become developed within the parent cells, both being inclosed within a common membrane. In encephaloma, where there existed abundance of liquids and fatty matters, the cells were rounded or ovular.

He had read that a German pathologist had been able to produce cancer by injecting the juice of a cancerous tumour into the veins of a healthy dog. He had several times tried this, but had never been able to obtain any such successful results, although he had injected cancerous juice *containing abundance of cells* into the veins, and had put it into the trachea, and had also inserted it beneath the integuments of animals. The characteristic element of cancer was not the cell, but *the juice* in which it floated. Inoculation with the pus of glanders produced glanders; that of the varioloid disease of sheep gave rise to the same disease; and the saliva of the hydrophobic dog occasioned canine rabies; yet the pus and the saliva in these diseases presented no special characters when microscopically examined. Thus there existed different properties with identical appearances.

As regarded the *dimensions of the cells*, he considered that the cells of fibro-plastic tissue were the smallest; those of encephaloma the largest; whilst those of scirrhus were of a medium size, between that of the other two. The appearance of the nuclei and nucleoli varied very much, according to the variety of cancer examined; they were very small in fibro-plastic growths. These facts accorded well with clinical results; for it was known that encephaloid tumours grew more rapidly, spread more certainly, and produced general infection more speedily than any other variety of cancer.

He described the effects of various reagents on the cancer-cell, and showed the importance of acetic acid in rendering more distinct the nucleus and nucleolus.

He eloquently defended the use of the microscope, and maintained that it had great claims to the attention of all practical men, because if by means of it they perceived the cells which ordinarily accompanied cancer, their diagnosis became more certain, and they knew that they had to deal with either fibro-plastic, scirrhus, or encephaloid tissue.

As regarded the *treatment of cancer*, he recommended early extirpation. He had found that, after the removal of a cancer, there were a great many of the—he would not say characteristic, but *habitual* cells of cancer, disseminated through all the normal tissues near the spot affected, and also through those which were at some distance from it. Hence, in excising cancers, we ought to cut away a considerable portion of the surrounding tissues, even though they appeared healthy and unchanged, and that we ought even to cauterize them afterwards. By microscopically examining the discharges from wounds left by operations for the excision of cancer, he had ascertained that, so long as the pus contained cancer-cells mixed with the pus-cells, cicatrization did not

take place; and that, simultaneously with the disappearance of the cancer-cells, the process of reunion commenced. He was, therefore, of opinion that, after removing a cancer, we should allow the wound to suppurate for a certain time, in order to allow the cancer-cells to be eliminated by this channel.—*Monthly Journ. Med. Sci.*, Jan. 1855.

39. *On the Formation and Extension of Cancer-Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation.*—The following are the conclusions of SCHROEDER VAN DER KOLK:—

1. Through an interchange of material, taking place between cancer-cells and intercellular fluid, the latter acquires the property of forming new nuclei and cells of a similar nature.

2. This intercellular fluid passes, along with the parenchymatous fluid pervading the sound parts, into the textures adjoining the tumour. The parenchymatous fluid thus acquires the same constituents and tendency to form similar cells, which now become developed among the healthy surrounding tissue, in the course of the areolar membrane.

3. On account of the minuteness and small number of the last-mentioned cells, their presence cannot be detected with the naked eye; so that the surrounding parts may appear to be perfectly sound, notwithstanding that they contain the germs of the advancing formation of cancer.

4. It is, therefore, of importance, in removing cancer by operation, not only to take away, at the same time, a large quantity of the adjacent sound parts; but also to examine the innermost sectional edges under the microscope, in order to ascertain whether any trace of cancer-cells in process of formation is to be discovered in them.

5. The existence of burning, shooting pains in carcinoma, may be taken as a proof that the cancer-cells have reached the neighbouring nerves, and the disease can then scarcely be looked upon as a local one, in which an operation might be permanently successful.

6. By the absorption of the infected parenchymatous fluid through the lymphatics and veins, the whole body seems to become more or less tainted, so that secondary cancer ensues in distant situations, when, as is self-evident, operation can no longer be thought of.

7. This altered parenchymatous fluid penetrates the organic tissues, which are washed by it, the sarcolemma of the muscular fibres, the tubes of the nerves, &c. These membranes, too, both the sarcolemma and the walls of the nervous tubes, appear to take up the altered nutritive fluid; the consequence of which is, that both within the sarcolemma and the nervous tubes similar nuclei and cells arise, accompanied with an absorption of the muscular fibre and of the contents of the nerve, and attended with the deposition of fat, by which these parts waste and are destroyed, while the surrounding membranes (sarcolemma and walls of the nervous tubes) remain.—*Brit. and For. Med.-Chirurg. Rev.*, April, 1855, from *Lederlansch Lancet*, Sept. 1853.

40. *Cancer with Benign Clinical Features.*—Dr. ROBERT D. LYONS, in a paper recently read before the Dublin Pathological Society, states that he has on several occasions seen examples of undoubted cancer, which wanted many of the so-called malignant features; and he relates the following case as the most remarkable one of the kind, in many respects, which he has met with.

"The patient was aged 37. At the time I saw him, the disease had been in progress somewhat more than a year. He had enjoyed excellent health, and was not aware of the existence of any disease, until, having been tripped one day in the street by a dog, he shortly afterwards noticed a small tumour on the inside of the left groin, which subsequently increased in size, but gave him little pain. Indeed, with the exception of the inconvenience caused by its gradually enlarging dimensions, he had no cause of complaint whatever. Advice, however, was sought, and the question of the propriety of operation was entertained; but a second opinion having been taken, it was finally determined to allow matters to follow their own course.

"At a subsequent period, I saw the case, when the following observations

were made, which I transcribe from my note-book : The patient was pale, sal-low, and emaciated. The tumour had steadily increased in size, until, when measured over its greatest prominence, the limb was found to be two feet one inch in circumference, the opposite one in a corresponding situation being but 17½ inches. The tumour occupied the inner and anterior parts of the upper two-thirds of the left thigh, was of a uniform oval shape, its elevation from the surrounding parts being gradual and regular. It was excessively hard, but entirely free from pain, and could be manipulated and examined most minutely through its whole extent without producing the least suffering. The skin covering it was smooth, a little shiny, quite devoid of any irritation, and colourless, except where a few small veins coursed through it. The femoral artery crossed the tumour on the outer side, and could be felt with ease. A small circular, movable, painless tumour existed above the crest of the right ilium, and was apparently confined to the integuments.

"Casting the eye on the trunk, as the patient lay uncovered, it was manifestly unsymmetrical ; there was an extensive uniform and regular enlargement of the left side, occupying the left infra-mammary region, encroaching a little on the epigastrium, and extending through the left hypochondriac region to the back ; it also passed downwards towards the ilium. On manual examination and percussion, it was found that one or more solid tumours existed in these situations. He said that he some days previously experienced uneasiness in the epigastrium, but it was only temporary ; his chief suffering was altogether from flatulence : the urine, from time to time, presented a good deal of purpuric deposit. The chest was generally resonant, the respiratory movements tranquil, the heart's action normal ; pulse 100. These points will be found worthy of note.

"I experienced considerable difficulty in arriving at a satisfactory conclusion as to the nature of these tumours. The absence of pain, of any tendency to ulceration, the patient's indifference to any kind of manipulation, the normal condition of all the vital organs, the freedom from local or general suffering, the entire absence of any of the so-called malignant characters—for throughout the clinical features of the disease were as benign as those of any form of tumour that I am acquainted with—made it a matter of great difficulty to pronounce a positive diagnosis. If the tumour had been confined to the limb, I should probably have fixed on the idea of enchondroma ; if to the abdomen, on some form of the simple fibrous or lipomatous tumours which we know are capable of reaching a vast size. But this is not the place to discuss all the bearings of the question of diagnosis in such a case.

"The progress of the disease from this period was steady, and unmarked by any incidents worth noting till towards its close.

"The tumour in the thigh gradually but sensibly increased in size ; the enlargement of the abdomen went on from day to day, and the tumour or tumours—for it was yet impossible to decide whether there were more than one or not—began to cause some uneasiness, merely, however, from their size, and from their consequent mechanical interference with the organs around them. The colon could be traced nearly through its entire course, but, especially in the transverse portion of its arch, thrust forward before the mass, and often thus imprisoned between the latter and the wall of the abdomen. Constipation and flatulence now caused more suffering than the patient had at any time before experienced.

"His appetite continued pretty good, but he was much emaciated, for the whole vegetative force of his system appeared devoted to the supply of these immense growths. The abdomen at length became so large as to render the poor patient quite unwieldy to himself, and he lay on his back unable to turn, and hardly able to move.

"A little dyspnoea had been complained of from time to time, but with relief of the abdominal flatulence this generally disappeared too. Of late, however, it became increased, and was pretty constant. On one occasion a paroxysm of dyspnoea came on, and after a couple of days of rather severe distress of breathing, with symptoms referable to the left lung, I contrived to effect an examination of the left side of the thorax on the lateral and posterior aspects, a thing

by no means easy of accomplishment; as it was almost impossible for him to sit up. Dulness on percussion was found as high as the infra-scapular region behind and the fifth rib in front. Coupling this with the paroxysm of dyspnœa, cough, and other symptoms referable to the left lung, and as I had now for some time regarded the case as one of cancer, I concluded that cancerous deposit had taken place into the left lung. How far this conclusion, from such symptoms and signs, was warranted may, perhaps, be questioned now; but it also admits of doubt, whether we have arrived at any principles of physical diagnosis capable of deciding what the actual state of parts is under similar conditions. I may here first briefly state that, in the case in question, it was not what I supposed. As will be seen in the sequel, cancer had been deposited within the thorax, but not in the organ, nor in the manner in which I was led to believe.

"It will be unnecessary to follow the case in further detail; suffice it to say that the disease progressed steadily, and the tumours continued to enlarge gradually but sensibly. To the end the only suffering was from flatulence, the sense of tension and distension in the abdomen, and occasional dyspnœa.

"In about a year and a half from the first date of the disease, or rather, to speak more accurately, from the period at which the small tumour in the groin was first noticed, he died somewhat suddenly.

"I examined the body with much interest and attention.

"The tumour in the left thigh was found to be one uniform mass, of oval figure, smooth on the surface, entirely unbroken, free from any connection whatever with surrounding parts, and easily detached from them; it had passed in deep, so as to reach the neck and shaft of the femur, but there was no mark or erosion of any kind on the bone, which in all respects appeared perfectly healthy, and the growth but lay in contact with it.

"On opening the abdomen, a tumour of enormous dimensions came into view, partly obscured by the colon which ran in front of it and was attached to it; the lower part of the peritoneal cavity was filled with extravasated blood. The tumour was found to be one continuous solid mass, reaching from the diaphragm to the groin, moulded on the spine, but free from it, as it appeared to be from everything with which it lay in contact, as the intestines, the liver, the spleen, and above all, the left kidney, which was inclosed in a kind of cleft in the tumour, and flattened out to the shape and dimensions of a hand, but in all respects healthy, and its peritoneal covering uninjured.

"I examined with care for the source of the effused blood, and can only suppose the hemorrhage to have taken place in the following way, by rupture of veins, and this I believe to have been the immediate cause of death.

"The colon, as I have stated, lay in front of the tumour, so did the omentum and part of the meso-colon, both of which were thus compressed between the tumour and the wall of the abdomen, and in consequence their numerous vessels had undergone distension to an enormous degree, and when this distension was carried beyond a certain point the veins had given way, had poured their blood into the peritoneal cavity, and had thus caused death, for the quantity of blood effused was very considerable.

"I directed particular attention to this great tumour, with a view to determine, if possible, from what point it had sprung; but this I was quite unable to do; it was connected only with the mesentery, for though it touched almost every part and organ within the abdomen, it had no connection with any of them. There was no disease of the lumbar glands, no deposit in the liver, spleen, or kidneys, and all these organs were in a perfectly healthy condition.

"On opening the thorax, the lungs were found quite healthy; no inflammation, congestion, or deposit of any kind existed in them. The left pleural cavity was, however, greatly diminished in size in the vertical direction, the left ala of the diaphragm being thrust up before an immense lobe of the great abdominal tumour, so as to be on a level with about the fifth or sixth rib. This enlargement had probably taken place in this direction with some rapidity; hence the sudden paroxysms of dyspnœa, and hence, also, the absolute dulness and absence of respiration at the infra-scapular region, and up to the fifth rib in front.

"And here I may call attention to what I conceive constitutes an unsolved problem in physical diagnosis. Given, dulness and absence of respiration on either side of the chest, with an immovable diaphragm from whatever cause, enlarged liver or spleen, or tumour such as that now under consideration: required, to determine the condition of the intra-thoracic organs.

"But I have said that there was a cancerous deposit within the thorax. Though not into the lung, a deposit had taken place into the heart; a mass of a square form, and about two inches in diameter, was impacted in the wall of the left ventricle, projecting into the pericardial sac: but however long or short a time it had existed in this situation, I never had the least suspicion of such a deposit, nor was there in the action of the organ any one abnormal condition to draw attention to it, though I had repeatedly examined it with care.

"On section, these several tumours presented as *beautiful* an appearance as I have ever met with in dissection of such structures. The surface of section was of a rich cream colour, lightly tinted with the delicate lemon yellow one often sees in cancer, the whole looking like the section of some rich, ripe fruit. The substance was pretty firm and homogeneous throughout, and these appearances were almost identical in all the specimens. The histological elements were, I may briefly say, those of cancer, exhibiting cells, nuclei, and other elements agreeing with those of cancer in all respects. That this remarkable disease was cancer no doubt remains on my mind, though to prove to an absolute demonstration that it was such is, I believe, hardly possible, except to those who admit the existence of special histological elements.

"If we take a survey of the chief features of its clinical and anatomico-pathological details, we shall find that it wants several of the characters assigned to what we may term the normal type of cancer, or cancer *selon les regles*.

"At no one period did it present a single symptom or character which could be called **MALIGNANT**.

"Confining ourselves to its clinical history, its course was undoubtedly **BENIGN**, for it did not ulcerate, it caused no pain, it simply grew with a vegetative force.

"Taking the phenomena presented during life at any given time, they could be readily explained by supposing, for example, that the tumour in the thigh was an enchondroma, and that in the abdomen some accidentally co-existent indifferent growth, a fibrous one, for instance. Other equally obvious explanations will occur to your mind.

"Looking to its pathological results, we find these also very singular: it attacked no neighbouring structures; it lay on a bone, and did not erode it; it inclosed, compressed, and flattened out a glandular organ, such as the kidney, and yet in no way invaded its structure. I know not how to trace any anatomical or physiologico-pathological connection between the seats of the primary and secondary growths; in what relation the groin, the mesentery, and the left ventricle of the heart stand to each other, I am at a loss to conceive. The ordinary channels of communication, the bloodvessels, or the lymphatics, could not have been the medium of transit of the germs of the disease; neither the lumbar glands, nor the liver, nor the lungs, which are so often the points of arrest, were here in the least degree affected; there was not, then, in this case, secondary infection or deposit in distant organs, that great criterion of cancer diagnosis in the ordinary sense. This, usually regarded as perhaps the most highly **MALIGNANT** and fatal feature in true cancer, was wanted in the case before us.

"From the absence, then, of the various characters usually denominated malignant, and from a consideration of the mild clinical features presented by the case through its whole course, I have used what may seem a bold, if not unwarrantably hazardous figure, and called it an example of **Benign Cancer**; malignant it was not, in any sense of that ill-used word; it was fully as innocent or benign as any of the tumours known as such, and I have taken advantage of these facts to associate the term **Benign** with that of **Cancer**. This I have done advisedly, and not without a purpose. I believe that the slovenly language which is so often used to cloak ignorance or imperfect diagnosis, and

for this purpose employs the word *malignant* to tie together several diseases essentially different, betokens a slovenly pathology worse than useless, for it is an immediate impediment to the advancement of our science. I have contended against it, and I hope to see it soon exploded.

"It is often asked me, when a specimen of disease is put into my hands for examination, whether it is 'malignant' or not, to which I always reply, 'THAT is a question to be answered by the clinical history of the disease, but it is entirely outside the province of pathological anatomy.' A disease may be highly malignant, like the eroding cutaneous ulcer or the *squirre en cuirasse*, and yet not be cancerous; and I believe a growth may be found to be Cancer histologically, and yet to have exhibited a comparatively Benign course in all its clinical features.

"The diagnosis of the so-called MALIGNANT diseases has been cast as an opprobrium against the faces of the rising school of scientific pathologists, because they cannot reduce an equation essentially complex in its nature and the number of its terms, to a popular expression intelligible to those who will not go deeper than the *a b c* of our science. With the words BENIGN CANCER, to use a term warranted by the days in which we write, I throw a shell into the opposite camp, the explosion of which will, I trust, help to carry away the mists of doubt, error, and unscientific pathology which crowd round the word MALIGNANT."—*Dublin Hospital Gazette*, April 15, 1855.

41. *Hysterical Affections of the Joints*.—F. C. SKEY, Esq., in a clinical lecture delivered at St. Bartholomew's Hospital (Jan. 19, 1855), made the following remarks on these interesting affections, to which attention was first drawn about 20 years since by Sir B. C. Brodie.

"You will see abundance of hysteric hip affections, and in yet more abundance, hysteric affections of the knee. So general are these forms of disease, that I declare I think less error would result if every example of disease of these two joints, not caused by accident, which occurs in young women between sixteen and thirty years of age, were treated as hysteria. I state this opinion with the less hesitation, because we rarely have much difficulty in detecting the real disease. We can distinguish the real from the spurious much more readily than we can detect the spurious from the real. An hysteric affection of a joint is marked, first, by the sex and age of the person—generally a female, and young; second, we have more pain than in real disease; the pain is irregular—sometimes severe, sometimes entirely absent. On examination, the approach of the hand to the neighbourhood of the joint is attended by an exclamation anticipatory of pain, and a shrinking from pressure, often with tears. Pain is expressed on slight, but not increased by heavy, pressure, and rough manipulation. You may boldly shake the entire limb while the patient's attention is engaged in some collateral thoughts. The head of the femur may be boldly revolved in its socket, and the limb carried round in all directions, at first slowly, and then with gradually increasing force.

"I saw a lady, some months since, of about thirty-five years of age, who was brought up to London for surgical advice, on account of a severe affection of the hip-joint which she had suffered from for some two or three years. She was on the point of leaving town when I paid her a single visit. When I entered the sitting-room of her lodgings, she was lying on the sofa. I requested her removal to a bed in the room adjoining. While being carried to bed, her cries were most distressing, and some few minutes elapsed before I was admitted. When I again saw her, she said she really feared she could not submit to be examined; but I tranquillized her by speaking kindly and gently, and she at length submitted. I watched her face while I extended my hand towards her joint, and I never saw suffering more clearly depicted. I laid my hand with the utmost gentleness on her trochanter, and very gradually pressed it against the bone, and she did not shrink. Distracting her attention to the early history of her case, I quite raised her thigh, and turned it first gently, and then with force, though slightly, in all directions, and I then struck my hand repeatedly, and with force, on the trochanter. Everybody round the bed appeared astonished; and well they might be; for such force, applied to a

joint that had been kept absolutely motionless for months, which appeared permanently inverted, and apparently abridged of its length, by reason of the curvature of the lumbar vertebræ, with a limb much wasted by inaction, might be deemed a curious *exposé* of its past history. This lady's health was wasted by long confinement and severe local and general depletive treatment. Of the result I know nothing, though I had one letter from the family, stating her improvement. I should mention, that the especial reason which led to my opinion being taken on this lady's case was to ascertain whether or not her femur was dislocated, and the question was not, judging from the appearance of the limb, an inappropriate one. The thigh was drawn up, considerably inverted, and apparently fixed. All the tissues were soft and flabby, and the limb extremely pale. Any attempt to move it with the consent of the patient was followed by acute suffering. On examination, the anterior spine of the ilium was drawn up by long-continued curvature of the lumbar vertebræ, but the length of the entire limb was unchanged.

"The difficulty of these cases is greatly increased by the local appearance of disease. In hip cases, the position of the thigh is distorted—more or less fixed; and the whole limb is inverted. In the knee examples, the joint is swollen, and often hot. It rests, however, perfectly straight in bed. In the former, we can detect the absence of that form of dislocation which its condition simulates, by the normal relation of the trochanter major to the anterior spinous process of the ilium. We are rendered suspicious of the existence of true disease by the more advanced age of the hysteric patient; by the severity, and, at the same time, the irregularity of the pain; and by the constitutional character of the patient. In the knee cases, although the joint is swollen and the skin hot, yet the cavity of the joint is, for the most part, free from an undue quantity of fluid; and the swelling, as has been remarked by Sir Benjamin Brodie, is confined to the sides of the ligamentum patellæ. We more generally find irregular catamenia than otherwise, although this feature is not invariable; and we have an hysteric diathesis on which to base our inquiries.

"In some cases, the harmony of the muscular action of the limb is entirely lost, and one set of muscles prevails in action over its antagonists. These in-subordinate agents are mostly those of flexion: the arm may be bent up to an acute angle at the elbow joint, or the fingers may be closed, or the thigh drawn forcibly and closely up on the pelvis, one example of which I have mentioned. Amputation of such limbs formerly practised left the diseased cause behind, but to be developed in other localities of the body. All these patients betray a weak condition of bodily health; the pulse is small and quick; the tongue pale-coloured; and severe and chronic cases are generally coupled with more or less of emaciation and debility of frame consequent upon it.

"When local pain is a prominent symptom, it rarely fails to succumb to the external application of opium, combined with a moderately stimulating liniment, such as the compound soap liniment. It is needless to apply for aid to any form of opiate tincture; nothing short of the extract will suffice: and I commonly employ the fluid extract of opium—such as is now a good deal used by the druggists in the neighbourhood in which I reside, and to several of whom I have myself introduced it.

"The catamenia should, of course, be encouraged, but not forced; and, for general treatment, we must resort to iron, bark, good nutritious diet, with which a certain quantity of fermented liquor, whether beer or wine, in some form, should be combined. The profession is indebted to Sir Benjamin Brodie for an excellent practical work on this interesting subject."—*Lancet*, February 24, 1855.

42. *Efficacy of Opium in the Cure of Chronic Ulcers of the Leg*.—Mr. F. C. SKEY, in a clinical lecture, delivered January 12th last, at St. Bartholomew's Hospital, makes the following remarks on the efficacy of opium in the treatment of chronic ulcers of the legs:—

"I venture to attribute to this remarkable drug (opium) the property of promoting the formation of healthy granulations on a surface that, notwithstanding all the previous appliances of surgery, is yet flat, pale, and ungranulating.

Now, there is no example of the power of opium to effect this object, more conclusive, or in which there is more work to be done, than that form of disease of which I am speaking—which consists of a gap formed on the surface of the body, of greater or less depth and diameter, and in which there exists not even a trace of a curative action—and yet the object is accomplished by means of this agent, and often with remarkable celerity. We call opium a stimulant and a sedative. As a stimulant, it is not very often employed in practice; while its properties, as a sedative, are well known, and are in daily requisition. Its property of mitigating pain and of promoting sleep, is that for which it is almost exclusively employed, and so completely is its action associated with this sedative principle that its occasional influence as a stimulant is almost entirely lost sight of, and the stimulating property has merged in the supposed sedative. How otherwise can we explain the reasoning of Mr. Pott, who may almost be termed the father of modern British surgery. In speaking of the subject of opium in the treatment of gangrena senilis, he refers its undoubted efficacy to its property of soothing pain. He says: 'I have always found that whatever tended to calm, to relax, and to appease, at least retarded mischief, *if it did no more.*'

"But, in truth, pain, though common, is by no means an invariable concomitant of senile gangrene; and it is tolerably notorious that opium is a valuable remedy in all cases of this disease. How, then, on what principle, does opium act in those numerous cases of senile gangrene that are destitute of pain, and in which it is an equally efficacious remedy?

"I believe that its sedative properties have little concern with the result. In truth, opium is a most valuable stimulant of the vital powers, and whether its action originate with the centre or the periphery of the circulation, whether primarily on the heart or on the capillary vessels, I do not pretend to know; but there is no drug, simple or composite, known to our pharmacologists that possesses an equal power with opium, of giving energy to the capillary system of arteries, of promoting animal warmth, and thus maintaining an equable balance of the circulation throughout the body. To maintain the balance of the circulation! How much of meaning is attached to these words! How many affections of the bodily frame may not be brought within the range of this definition! Take the common chilblain; what is it but a local congestion of blood caused by defective capillary power?—there is no better remedy than opium; cold feet, as characterizing a person or a constitution, equally relieved; senile gangrene, the result of arrest of the capillary circulation, or its apparent opposite, local hyperæmia—these diseases, one and all, manifest a loss of local power, a failure in the balance of the circulation. The term 'inflammation,' a word formerly in the mouths of our professional brethren on all occasions, is now limited in its application, and should be yet more limited, and I believe, in a yet more advanced state of medical science, will be restricted to an actually rare condition of the system. The influence of opium in such conditions is that of promoting a genial warmth over the system, a glow exactly resembling, and in fact identical with, that produced by the reaction on the system which is caused by the cold bath. *It is local health*, and the sensation is most agreeable. The benefit derived from opium, when administered for the purpose of arresting inflammatory action of the vessels, admits, I think, of much doubt, and should be resorted to with some hesitation as a remedial agent, though I am quite persuaded that the evil of its administration is greatly overrated. But who will profess ignorance, in these days, of the inestimable value of this agent when resorted to immediately after an attack of inflammation has been subdued by a local or general bleeding? Here we can imagine that, the activity of the disease being checked, the diffusing influence of opium on the circulation may act as a simple derivative, operating on the vessels at the moment they are not indisposed to yield up their blood, and to which indeed they are compelled by the diffusive power of the general stimulus.

"Many years ago, and before the introduction of railway travelling, I was summoned late one afternoon to see a patient some eighty miles from London. I travelled outside the mail. This occurred in the month of December, and the night was extremely cold. By some mistake I omitted to bring my great-coat;

and, for the first hour, I suffered a good deal. On reaching a town at some ten miles distance from London, I took the opportunity, while changing horses, to run across the street to a druggist's shop, where I ordered a draught, containing twenty-five drops of tincture of opium. I believe I was the only person outside the coach that night who did not suffer the slightest sensation from cold. But it will be urged by many, who have experimented on, and who have observed less than I have done, the medical properties of opium, the infinite importance of studying the reactive effects of this deadly poison, and they would inquire into the condition of a person so treated on the following day. You may be assured that it amounts to *nil*. You will, I am sure, readily understand what I mean when I say the cold and the opium mutually balanced and mutually neutralized each other. There could be no reaction, because the influence of the depressing agent, viz: the cold, rather than otherwise, exceeded in duration that of the stimulant. If the period of prostration were brief, and limited to one or two hours, the argument might hold; but it is but a sorry objection to be urged after all.

"I wish I could impress on the minds of the medical authorities in the Crimea the real benefit that might be derived to our noble troops, beaten down by intense cold and suffering in its various forms, from the judicious administration of opium. If twenty-five or thirty drops of tincture of opium, in addition to his ordinary quantum of rum, were administered to each soldier whose nightly services are required in the trenches or on guard, you would hear little complaint of cold for that night, neither would it produce the smallest tendency to sleep. And what do you imagine would be the objection urged against the proposition? 'You would destroy the efficiency of the entire army; you would corrupt their morals; you would engender the most enervating habits; they would all degenerate into professed opium-eaters; and, in fact,' say the alarmists, 'the idea is preposterous.' Here, again, I assert that no possible evil could result; the only sensation, present and future, would be the absence of cold. If cold beget suffering, opium is the antidote to that suffering, and the one will assuredly neutralize the other.

"Notwithstanding the prejudices and the bigotry that have long beset the public mind on this subject, and from which our profession is not totally exempt, there is no comparison to be drawn between the practice of dram-drinking and the excessive indulgence in the use of opium. The man who indulges in spirituous liquors makes daily inroads on his digestive powers not less than on his brain. His appetite is destroyed, and the pabulum for his blood is withheld from his circulation. He is stamped for life, and his perfect health is irrecoverable. The influence of opium, when taken as a means of indulgence though deleterious, is not permanently injurious. It exercises no serious influence on his digestion or on his cerebral organs, and, the practice once controlled, leaves him in a condition to regain, without difficulty, the fullest vigour of both bodily and mental health.

"I have related to you the particulars of several cases of chronic ulcer in which recovery was attributable to the medical properties of opium, and almost to opium alone. The character of these ulcers strongly marks the inactivity of their nature, and hence the class of society to which they belong. They are marked by a flat base, which indicates, by its pale, flabby uniformity of surface, that no reparatory action has approached it. It is often surrounded by a thick, high ridge of lymph, covered by unhealthy integuments. The depth of the ulcer, which may be seven or eight lines, is caused partly by the ridge, and partly by the excavation of the ulcer below the natural level of the healthy integuments. So long as this ridge exists, although granulations may form, and will form, from the date of the employment of the opium, yet cicatrization will never complete the process of cure unless the wound or ridge be absorbed. Now the action of opium is not alone exhibited in the development of healthy granulations, but in the entire complement of such actions as are required by the sore, viz: the formation of new material, and the absorption of the old.

"The influence of the stimulant is exhibited therefore not on one particular function. It does not merely promote secretion, but it stimulates to healthy vital actions in their entirety, viz: secretion, organization, and absorption con-

temporaneously; the granulations are secreted and organized, while the circumvallation of unsound material, the product of years of growth, is gradually absorbed and reduced to the level of the surrounding integument; for the removal of this wall is quite as indispensable to the ultimate result as the obliteration of the cavity by granulation. Without the two surfaces be brought to the same level, the process of cicatrization, or skinning over, will never be perfected.

"If, therefore, we find that a disease like that I have described, and which exhibits so palpably a dormant condition of the remote capillaries, is amenable to this form of stimulant, which can only accomplish the cure by the substitution of healthy for morbid actions, why should we restrict its employment to this class of diseases? Why, as I have elsewhere inquired, may we not experiment with success on any local disease dependent on the same cause, viz: an inert condition of the remote vascular system?

"In claiming for opium the merit of rousing into healthy action the dormant capillary system, to the end of accomplishing the permanent cure of the chronic ulcer of the legs in old persons, I by no means wish you to infer that I consider all other modes of treatment unworthy of trial. Indeed, I attach great value to that recommended by Mr. Baynton, of Bristol, and others; but, having tested their value, I have no hesitation in pronouncing that which I have recommended, so far as I am competent to judge, as by far the most certain and efficacious."—*Lancet*, Feb. 3, 1855.

43. *Vapour of Iodine in Mammary Tumours*.—M. EICHMANN relates the following case: A healthy woman, æt. 47, none of whose relatives had ever had cancer, received a slight blow on the breast. This occasioned a hard and mobile tumour. After an absence of the catamenia for 17 months, the tumour became painful; smaller glandular enlargements appeared on the breast and extended to the axilla; lancinating pains were felt; the skin became adherent to the tumour, and the sebaceous follicles were distended with blackish matter. The patient refusing to submit to extirpation, M. Eichmann applied to the tumour a bag filled with tow, and containing also iodine, which was retained *in situ* by means of adhesive plaster. The iodine was renewed fortnightly. After wearing this bag for a month, the patient was greatly improved, and after the lapse of seventeen weeks the mammary enlargement had completely disappeared.—*Gaz. des Hôpitaux*, 19th Dec. 1855.

44. *Digitalis Pomade in Hydrocele*.—Last spring, an Italian surgeon named Bellucci published accounts of five cases of hydrocele cured by the external use of digitalis. Little attention was paid by surgeons to these results; but very recently M. LAFORGUE, chief surgeon in the *Hôpital de la Grave*, at Toulouse, has made trial of the new mode of treatment, and found it to be as efficacious as was alleged by M. Bellucci. A man, æt. 60, had a large hydrocele of the right testicle, and being unwilling to submit to the usual operation, besought M. Laforgue to try some other method. Daily friction of the tumour was ordered with the following ointment: R.—Pulv. folior. digitalis, 6 grammes; axung. 30 grammes; *misc.* The patient was also ordered to wear a suspensory bandage. In a few days, the man presented himself completely cured. He had enjoyed excellent health during the process, and had used, in all, 18 grammes of the powder of digitalis.—*Ibid.*, from *L'Union Méd.*, 30th Sept. 1854.

45. *Radical Cure of Hydrocele by Injections of Chloroform*.—Prof. LANGENBECK, of Berlin, not being satisfied with the effects of tincture of iodine as an injection in cases of hydrocele, has recently been employing chloroform as a substitute for it with excellent results. He finds that it acts in the same manner as tinct. iodinei, but that it sets up adhesive inflammation more quickly and more surely than that remedy. After withdrawing the fluid of hydrocele, he injects $\mathfrak{z}\text{i}$ or $\mathfrak{z}\text{iss}$ of chloroform through the canula, and after allowing it to remain for a short time in contact with the tunica vaginalis, he allows it to escape, so as to avoid its absorption. The pain occasioned by it is not intense, even less so than that induced by iodine, and it is not of long continuance.

Langenbeck, in his paper, relates the particulars of four cases treated thus, in all of which a complete and radical cure was accomplished in from two to three weeks.—*Dublin Hospital Gazette*, April 1, 1855, from *Deutsche Klinik*, 31, 1835.

46. *Two Cases of Aneurism unsuccessfully treated by Compression.* By JAMES MILLER, Esq.—The author was of opinion that the question as to the treatment of aneurism by compression was still *sub judice*, and must be settled by the production of facts, rather than by mere reasoning. As an advocate for this form of treatment, however, he held it to be necessary that all facts bearing on the question should be adduced, and, acting on this belief, he brought forward these cases of failure.

Case 1 was one which Mr. Miller had had under his own care in the Royal Infirmary. The patient was a shoemaker, aged 42, from Galashiels, and was admitted March 23, 1854. His general health was good, and the popliteal tumour was about the size of a hen's egg. Next day, Carte's compressor was applied on the femoral at its upper and lower portions. The pressure was moderate, and antiphlogistic regimen was enjoined. On the 27th, the upper compressor was removed, in consequence of the complaint of pain; and on the 5th of April it was noted that there was much pain, swelling, and cedema of the limb. A lump of lead, with an elastic band attached, as used in the Dublin hospitals, was now tried. August 1st, seventeen weeks since the commencement of the treatment, the tumour was harder and slightly diminished in size; pulsation was distinct. Flannel was ordered to be applied to the limb. The lead weights had up to this time been kept constantly applied above, and Carte's apparatus to the lower part of the limb.—Aug. 3, the patient suffered much; anxious to have the artery tied. Carte's apparatus alone was now kept applied at night. Aug. 10, pressure was abandoned; the tumour was harder, but still pulsating, becoming caudate towards its head, and increased in size towards the inner side. Aug. 24, five months after admission, the artery was tied. The parts in its neighbourhood were found more matted together than usual. At 8 P. M. a slight thrill was perceptible in the tumour. There was sickness from the chloroform. Pain of the back was complained of. Aug. 25, the thrill was still less. Colchicum was given internally; and chloroform was applied locally to the back. Aug. 26, the thrill was scarcely to be felt; and on the 27th it was gone. Aug. 30, the thrill had returned; and on the 15th Sept. it was noted that the thrill was still present; the ligature was yet in the wound; it came away on the 18th. October 10, the tumour was at length silent, and diminished in size. The patient was dismissed on the 14th.

Case 2 had been under the care of Dr. Johnston, of Montrose, and was admitted into the Montrose Infirmary March 24, 1854. The tumour was about the size of a small orange, and had been first noticed in the previous July, and gradually increased in size. The pain, at the time of admission, was severe, the symptoms well marked. The health was good, and the heart sound. Low diet, etc., was ordered, and digitalis given internally. Carte's apparatus was applied at intervals, and soon another instrument alternately with it. Up to the 16th of August, the tumour had become flattened, and diminished in size to that of a walnut. The pulsation was weakened, but was still apparent. The health now began to flag, and on the 22d a ligature was applied. Slight pulsation was felt in the tumour on the 28th, but never returned. The ligature came away on September 14th; and on the 15th of October the patient was dismissed from the hospital. Dr. Johnston attributed the successful issue to the fact of the pressure employed having developed the collateral circulation of the limb.

Mr. Miller drew attention first to the fact of the pressure not having been easily borne. He believed that a still simpler method of applying it would yet be discovered. Secondly, careful watching was requisite during its application, as the patient, under the mistaken idea of expediting the cure, was apt by undue pressure to excoriate the parts and induce erysipelas. Any swelling of the limb, etc., demands attention, but cessation of the pressure appeared to suffice to dispel such dangers. Care to be taken that the pressure was not applied too low down. Third, it was to be noted that the collateral circulation was much affected. In the first case, the tumour first became harder, etc.,

and then pulsation appeared in it from the collateral circulation. In such circumstances, Mr. Miller would recommend instant application of the ligature, and if this was not done, difficulties were met with afterwards, even if the artery was tied. As it was, in this case the pulsation continued for six weeks. In Dr. Johnston's case, however, the collateral circulation gave no trouble after the deligation of the vessel. In future, if after long-continued pressure the collateral circulation increased greatly, Mr. Miller would cease compression, and, after an interval of rest, would apply a ligature, without, however, any risk of gangrene supervening. Fourth, as to the effect on compressed parts. In the first case, pain was always complained of, and a hardness could certainly be felt. On dissection, the parts were found much condensed, and difficulty was experienced in passing the needle; but this was to be avoided by cautious dissection. Fifth, after prolonged pressure, the state in which the system is, is one very favourable for a subsequent operation. In the first case, from accidental exposure while on the operating table, an attack of lumbago came on. Lastly, Mr. Miller pointed out the odd parallelism which existed between the two cases as to the dates of admission and of their general progress. These histories furnished the following arguments for and against the treatment by compression: *Against*. 1. The pressure was not well borne. 2. The effect produced on the collateral circulation, and on the parts immediately subjected to the pressure, was very troublesome in the first case. 3. In both cases, the plan failed after a trial of five months. *For*. 1. If compression can be maintained so long, it must be a comparatively safe procedure. 2. The free collateral circulation induced was favourable to any subsequent operation, as there is less risk of gangrene of the limb. 3. The constitution was better prepared for the operation by ligature, should that become necessary.—*Proceedings of Med.-Chirurg. Soc., Edinburgh, Dec. 6, 1854, in Monthly Journ. Med. Sci., Jan. 1855.*

47. *Stricture of the Urethra.* By JAMES SYME, Prof. Clin. Surgery. (Extracted from Clinical Lectures.)—I will now bring before you a case of stricture of the urethra of great interest. The patient, Daniel M——, aged thirty-seven, a seaman, has long suffered severely from the disease. He was treated by dilatation, eight years ago, in the hospital at Plymouth; but, even then, small instruments alone could be passed, and soon after leaving the hospital he relapsed into his former condition. At present, micturition is very laborious and frequent, sometimes hourly, and the stream of urine extremely slender. On examination, I found a bougie of moderate size arrested, an inch and a half from the orifice, by an obstruction, which could not be passed by the smallest bougie; I then tried a common probe, but this also could not be passed. I now had recourse to the instrument which I hold in my hand, which was made for cases of this description. You see it is considerably finer than a common probe at its extremity, and gradually increases in thickness towards the handle. This instrument passed without the slightest difficulty. If the stricture had been seated at the bulb, it might have been said that the fact of my being able to pass only so small an instrument was no certain evidence of an extreme degree of contraction, as there is difficulty in guiding the instrument with precision at that part of the canal; here, however, there is no such source of fallacy, for we can direct the instrument with certainty in the course of the urethra, and, small as it is, it is firmly grasped. The first remark I would make regarding this case has reference to what is called impermeability of strictures. Now, the No. 1 of our set of bougies is the smallest size that is made, while many sets have none so small; so that this stricture would be impermeable to all ordinary bougies. On the other hand, the very small instrument I have just shown you passed without the slightest difficulty; and this case, therefore, furnishes a good example of the principle that all strictures that allow urine to pass out, will admit of an instrument, sufficiently small, being insinuated through them. The next point to which I would direct your attention, is the situation of this stricture, anterior to the scrotum. It is here that the tightest strictures have been observed to occur, which is fortunate, for if there should be a stricture of the same degree of tightness behind the scrotum, its remedy would be attended with extreme difficulty. In the present

case, I feel the induration of the stricture like a small pea, somewhat elongated. Now, experience shows that of all strictures of the urethra, this condition is the most unmanageable by bougies; the patient tells us that so far from having experienced benefit from the introduction of instruments, his symptoms have rather become more aggravated, and I believe we might continue dilatation for any length of time without advantage. Instead, therefore, of pursuing this unsatisfactory course, I propose to divide the stricture at once. But you will say, How can it be divided? We must use a proportionately small director, the employment of which would be attended with great difficulty at the bulb, but will be very easy here. In this part of the canal there is no occasion for a long incision in the skin; the point of the knife being entered into the groove of the director, anterior to the stricture, will be pushed backwards through it, while the end of the penis is held firmly; the same principle being followed in cutting backwards here, as in cutting forwards for a stricture at the bulb—viz., that of proceeding from the side where there is greatest resistance. After this, I shall pass a full-sized instrument, and I expect it will go freely into the bladder. Of this we cannot be certain till the stricture at the anterior part of the canal has been divided; but, as far as I remember, I never met with a tight stricture behind the scrotum along with an extreme degree of contraction anterior to it. There will be no need to retain a catheter in the bladder: the urine will flow freely, and there will be no risk of extravasation; even if it did occur to the small extent which is alone possible, I should regard it as a favourable circumstance, on account of the effect it would have in preventing the urethra from uniting by first intention.

[The patient was now brought into the theatre, and Mr. Syme, having shown that the No. 1 bougie was arrested an inch and a half from the orifice, while the delicate instrument above described passed the obstruction without difficulty, endeavoured to introduce a very fine grooved steel director. This, however, could not be passed beyond a certain distance; but Mr. Syme, on feeling the instrument through the tissues of the penis, found that the point of the director had passed the stricture, which he felt like a pea upon it, and that it was only in consequence of the instrument being tightly grasped that it refused to pass further. He therefore proceeded to operate as he had proposed, after which he introduced successively three different sized bougies through the seat of stricture, and pushed on the largest (No. 8) with ease into the bladder, thus proving that no obstruction existed behind the one divided. Scarcely a drop of blood escaped during the operation. Mr Syme then made the following remarks:—]

This, then, gentlemen, is, as you have seen, a very simple process; yet the patient may be almost said to be already cured. I think it likely that, instead of leaving the hospital very imperfectly relieved, after several months of dilatation, he will in a few days go away, without requiring any further precaution than the occasional introduction by himself of a steel bougie, two inches in length, to prevent the risk of contraction during the healing process. You will, I trust, not soon forget that remarkable fact which you have just witnessed—viz., that, after I had introduced the very small director fairly through the stricture, the firm grasp of the contracted part made it impossible to push it further without a degree of force, which I did not feel justified in using.

[The operation was performed on the 15th of January. On the night after the operation the patient retained his urine for seven hours, and never afterwards voided it with unnatural frequency. It flowed in a full stream by the urethra, but for a few days issued also to a small extent by the wound, in the neighbourhood of which the cellular substance showed some signs of irritation by swelling and tenderness. Full-sized bougies were occasionally introduced, and he left the hospital to join his ship on the 6th of February.]

In his lecture on the 25th of January, Mr. Syme said:—]

There are two patients who have been in the hospital for the last seven weeks, with stricture of the urethra at the bulb, as tight as you ever meet with in that situation. It was at first difficult to say which of these cases was the worse; both had extreme difficulty of micturition, and both incontinence of

urine, and both required, in the first instance, the smallest-sized bougies, which I experienced some difficulty in introducing. By passing instruments regularly at intervals of three or four days I was soon able to introduce a large sized bougie in both cases, but neither appeared at first to improve in their symptoms under the dilatation. By and by, however, one of them did derive benefit; he lost the incontinence, and could pass his urine in a much better stream—in short, he is now so much improved, that I think in his case we may be satisfied with dilatation. You observe I express myself with some hesitation; for the more I see of these complaints in this obstinate form, the more doubt I entertain as to the expediency of trusting to dilatation for their cure. The other patient, however, has not experienced the relief that we desired; his difficulty of micturition is almost as great as ever, and his clothes are still wet both by day and night from incontinence of urine. I also found, after leaving him for a few days longer than usual without the introduction of instruments, that, instead of going on with the large-sized bougie I had used on the last occasion, I was obliged to begin again with No. 1. On my next examination of the urethra, ten days later, I was unable to pass even the smallest size. In this case, therefore, if we had no means except dilatation, we should be puzzled and at a loss what to do next; but I need hardly say that we shall feel no such perplexity, but shall proceed to the other mode of relief for stricture—viz., dividing it by external incision. But before having the patient brought in, I will mention to you a striking case, at present under my care in private. During my short residence in London, this gentleman came up from Aberdeen, and placed himself under my care, in order to obtain relief from a stricture, which had been treated for some time as impermeable by several surgeons. I at once succeeded in introducing a small instrument, and, after employing dilatation for three weeks, the urethra admitted a full-sized bougie, and he went away apparently well, and with the hope of continuing free from his symptoms, with the occasional introduction of an instrument. Instead of this, however, he has ever since—that is to say, for the last seven years—been perpetually under the care of surgeons in different parts of Great Britain. On one occasion he had complete retention, and several surgeons then failed to pass a catheter. But the remarkable circumstance is, that he has been during this period repeatedly treated by dilatation to the full extent. And now comes the most important point of all: this patient, though in full general health, not above forty years of age, married, and leading a regular life, and therefore not in danger of contracting the disease afresh, yet, even when the stricture was fully dilated, has never passed urine with ease; he has even felt an insecurity of being able to void it at all, and has been frequently obliged to go out of church or to leave company in consequence of the importunate desire for micturition which has accompanied his complaint. His life has, in short, been miserable, and this, you observe, at times when bougies of full size could be passed into the bladder. This is an example of that form of the disease which first led me to aim at some other means of effecting that for which dilatation was evidently inadequate. These cases were formerly the opprobria of surgery. It is only three days since I divided the stricture in this gentleman; yet he told me yesterday (I have not seen him to-day) that he felt as if something was taken away that had made life miserable; an incubus had been removed that had been weighing him down during many long years of suffering. (The relief in this case proved permanent.)

[The patient was now brought into the theatre, and chloroform having been administered, Mr. Syme succeeded, after some little difficulty, in passing the thin part of his staff through the stricture. The rest of the operation was easily completed in the usual way. Mr. Syme, while he had his finger in the wound, observed that he could feel the artery of the bulb beating at the side, and remarked that this showed forcibly the danger of deviating from the middle line in making the deep part of the incision. Mr. Syme then easily introduced into the bladder a No. 11 catheter provided with a stop-cock. The patient having been removed, Mr. Syme made the following remarks:—]

You had, not long ago, an opportunity of seeing the operation performed in a case complicated with fistulas, and great thickening and induration of the

perineum; those are the cases in which the operation presents the greatest difficulty. Again, ten days ago, you saw me divide a stricture anterior to the scrotum, in which the operation presents the greatest simplicity, and is easiest of performance. That patient, I may remark, continues well, passing his urine without pain in a good stream, and the urethra admitting a full-sized instrument. The operation you have just seen holds a middle place as regards difficulty of the operation, the stricture being seated as far back as it is ever met with, but the perineum free from induration. You see here on the table the blood that was lost during the operation; though he is a young man of thirty, in full health, and with rather greater disposition to bleed than is usual, yet two teaspoonfuls would be a liberal allowance for the amount of the hemorrhage.

[Everything afterwards went on favourably; on the removal of the catheter at the end of forty-eight hours, the urine passed in a perfectly full stream by the urethra, and continued to do so, none having escaped by the wound. In a few days he was going about the ward, and might then have been dismissed, but remained under observation until the 27th of February, nearly five weeks after the operation, when he got his discharge after a full-sized bougie had been passed with perfect facility into the bladder.]—*Lancet*, March 10, 1855.

48. *Notes on Lithotripsy, with an Account of the Results of the Operation in the Author's Practice.*—By SIR BENJAMIN BRODIE.

The author announced that his chief objects were—to communicate, in a series of notes, some observations as to those circumstances which are especially deserving of attention, with a view to the success of the operation: to give a brief but accurate account of the actual results of his own practice; and to add some observations as to the amount of danger involved in the operation by crushing, compared with that which belongs to lithotomy. He adduced reasons why this operation was not applicable to the period of youth, nor generally common or necessary in females. He preferred the forceps worked by a screw to that in which the force is applied by means of a rack, since the latter, though affording some advantages in the way of greater expedition, must be manifestly deficient in power as compared with the screw. The author remarked that no prudent surgeon would willingly undertake this operation unless the bladder admitted of the injection and retention of from four to six ounces of tepid water; and that where this power had been lost, it had in all his cases but one been restored by keeping the patient in the recumbent position for seven or fourteen days, and injecting the bladder daily. He then described the steps of the operation, urging the great necessity for the gentlest possible manipulation of the forceps, that injury may be avoided, pain diminished, and, by consequence, the bladder rendered less prone to contract upon its contents; that, with these points in view, the forceps should never be used as a sound for the purpose of exploring the bladder or ascertaining the position of the calculus, but that the convex part of its curved extremity should be brought into contact with, and gently pressed against, the posterior and lower surface of the bladder by the elevation of its handle; that if, when in this position, on the blades being separated, the stone does not fall into it by its own weight, the instrument may be slightly struck on one of its sides, which slight concussion will probably dislodge the calculus from its fixed position and cause it to fall; if unsuccessful in this, the forceps may be very cautiously turned to one side or the other, and the same rules followed in that position as before; but a freer use of the instrument should never be made, not even in cases of enlarged prostate gland. For such cases the author advocated the value of any apparatus by which the shoulders may be suddenly lowered and the pelvis elevated, the calculus being thus thrown into the fundus of the bladder. With the same object, the patient may be directed to change his position from side to side, or to walk about, the bladder being emptied and again injected. Caution was given against the use of any kind of forceps which retains a considerable portion of the detritus within its blades, as being liable to stretch and tear the urethra, induce rigors, and even infiltration of urine and abscess. This happened to the author in four instances, two of

which died. The extent to which a calculus may be crushed at any one operation, the author stated, must depend upon the amount of inconvenience suffered by the patient, which varies materially; but, upon the whole, he thought it more expeditious, and generally much more safe, to be content with a moderate progress; and, with a view to form a better opinion of what the patient can bear without risk, he preferred not to put him under the influence of chloroform. Again, an operation too much prolonged is liable to be followed by rigors—an accident which is not uncommon—is a most formidable complication, especially when combined with other disease, and against the occurrence of which too much care cannot be taken. The best precaution the author had found to be, the placing of the patient in a warmed bed immediately after the operation, the encouragement of perspiration by extra clothing, and the administration of a tumbler of wine or brandy and water. The writer had never found it necessary to cut down upon portions of calculus when lodged in the urethra; he had always found a slight manipulation with a small gum-elastic catheter sufficient to change the position of the fragment and to cause its expulsion. He cautioned members against cutting down upon the urethra in front of the scrotum, as he had seen it followed by troublesome abscesses from infiltration of urine in two instances, notwithstanding that every precaution was used to guard against it. Elderly persons with enlarged prostatic glands, should be assisted in expelling the detritus by washing out the bladder daily. They may be directed to void their urine in a stooping or recumbent posture, with the face downwards. The author gave it as his opinion that the inflammation of the mucous membrane of the bladder, induced by calculus, and indicated by great irritability of the organ, and the copious secretion of mucus, does not form an absolute objection to the operation, although it is doubtless a reason for proceeding with more caution; for, on the contrary, it often happens under such circumstances that the crushing of the calculus is followed by an alleviation of all the bad symptoms. The patient should not be dismissed as cured while any symptoms of calculus remain; a portion may be discovered after patient and repeated examinations, which, if left, would have formed the nucleus for the rapid formation of another stone. The author then referred to the difference in the density, and therefore the force required for the crushing of different calculi, as dependant upon their chemical composition. In cases of copious deposit of the soft mortar-like material of phosphate of lime, the author had made use of a forceps, of which the fixed blade was made concave, so as to answer the purpose of a scoop, with great advantage. He had never met with a case of hemorrhage to any amount in his own practice. He stated that, although the merits of this operation are best shown by cases of small calculi, when cure results after one or more crushings, yet very large calculi indeed, even such as have a diameter of an inch and a half, may be readily and safely broken up with an instrument sufficiently strong, and without any other inconvenience than the frequent repetition of the operation. The author then communicated the result of his practice in 115 cases of lithotomy. In 1 case he had had occasion to perform it eight different times, in 3 others twice, but always for fresh formations. Of these 115 cases, 9 had an unfavourable result; in 5 instances death resulted from the operation; in 4 the fatal result was to be attributed to the co-existence of other disease brought into activity by the shock of the operation; in 3 of the 5 fatal cases death resulted from the formation of urinous abscesses; in 1 from fever and general constitutional irritation; in 1 from inflammation of the mucous lining of the bladder and copious mucous secretion, prostrating the patient; in 3 of the 4 fatal cases the co-existing disease was seated in the kidneys; in 1 it was diarrhoea, and not positively connected with the operation, although inserted in the list. The author now again urged the great necessity for gentle and careful use of the forceps in this operation, showing that many of the imperfect recoveries are due not so much to disease of the prostate gland, to persistence of the original symptoms, or to undetected fragments, as they are to rough manipulations and too prolonged operations on the part of the surgeon. The success of the operation was thus shown to have been as somewhat more than $12\frac{1}{2}$ to 1; while from various statistics quoted by the author, it would appear

that, even admitting young children (so notoriously favourable for the operation of lithotomy, the recoveries being as 14 to 1), the proportion of recoveries after lithotomy to deaths stands as about $5\frac{1}{2}$ to 1. The author concluded his paper by stating that his experience had certainly led him to the conclusion that lithotripsy, if prudently and carefully performed, with a due attention to minute circumstances, is liable to smaller objections than almost any other of the capital operations of surgery, the cases to which it is not applicable being very few indeed, and chiefly those in which, from the calculus having attained an unusual size, the danger and difficulty of lithotomy are so great, that no surgeon would willingly, nor otherwise than as a matter of duty, undertake it.

Mr. Coulson said that all must feel obliged to Sir Benjamin Brodie for having brought the subject of lithotripsy under the consideration of the Society; but there was one point on which he ventured to differ from the author, relating to the most difficult part of the operation—viz., seizing the stone. Sir Benjamin Brodie had stated that the handle of the instrument should be raised, and the floor of the bladder depressed, so that the stone might fall into the hollow thus formed, and thence between the blades of the instrument. This was the plan generally recommended in this country for seizing the stone. Now he (Mr. Coulson) considered the reverse of this to be the right proceeding. The instrument should be kept as nearly as possible on a level with the urethra, and no hollow or depression should be made by it in the floor of the bladder. As soon as the instrument touched the stone, it should be placed lightly over it, the bladder opened, and the calculus seized. It was difficult to depress the floor of the bladder towards the rectum, especially when the prostate was enlarged, without pressing on the neck, and giving pain to the patient. And this was mentioned as having occurred in the cases related in the paper. He (Mr. Coulson) believed that this mode of proceeding not only caused pain at the time of the operation, but by producing inflammation and swelling of the prostate and neck of the bladder, gave rise to many of the serious results that followed. In cases of enlargement of the prostate, it had been very properly recommended in the paper to elevate the pelvis so as to dislodge the stone, and throw it towards the fundus. But cases occasionally occurred in which the stone or fragment could not be dislodged from this situation. Now no suggestion was made in the paper to meet this difficulty. In such a case he (Mr. Coulson) would recommend that the handle should be depressed so as to raise the instrument from the floor of the bladder, and whilst in this situation, it should be completely rotated, the concave part being turned downwards. The handle should then be raised, so that the instrument should incline to the floor of the bladder, and the male blade be drawn towards or as far as the prostate. By this manipulation, when carefully done, a fragment, or a stone, could be easily and safely extracted from that situation. There was also another point connected with the operation, on which he thought that too much stress could not be laid—viz: to do little or nothing more than simply crush the stone at the first sitting. It was desirable to let the shock of the first manipulation be as slight as possible, as it was impossible to foretell the susceptibility of the patient, and how he would bear the operation. He was glad to hear the use of chloroform discountenanced in lithotripsy. It was absolutely necessary that the operator should know what he was doing at every step of the operation, and the sensations of his patient were a material guide for determining at least when the operation was going wrong. The important question, however, to solve, was, to what cases lithotripsy is applicable; for in the proper selection of the cases, the success of the operation mainly depended. In general terms it might be stated that when the stone was large, the bladder thickened, contracted, and irritable, the prostate much enlarged, and the bar-like ridge described by Mr. Guthrie present, and the urethra irritable and contracted, lithotripsy was contra-indicated; on the other hand, when the stone was of moderate size, the bladder sufficiently capacious, and not irritable, the prostate not much enlarged, and the urethra healthy, then lithotripsy was peculiarly suitable, and might be performed with every chance of success. He (Mr. Coulson) said it was of the utmost importance to make a proper selection of cases; for even M. Civiale, with all his partiality for lithotripsy, rejected out

of 838 calculous patients which presented themselves to him in a given period, 290 as unfit for the operation. In properly selected cases, lithotripsy might be considered as one of the greatest improvements in modern surgery.

Mr. Brooke said that he had many years ago paid much attention to the subject, and had watched the progress of many more cases in the hands of Baron Heurteloup and the late Mr. Anthony White, than had occurred in his own practice. He thought that the convenience of Heurteloup's "lit rectangle" had not been duly appreciated by English surgeons, as it affords such an easy means of depressing the shoulders of the patient, or, in other words, elevating the pelvis, whenever any difficulty is found in seizing the stone in the horizontal position of the trunk. He protested against the method of seizing the stone, recommended by Mr. Coulson—namely, that of traversing the bladder, and even the space behind the prostate, by the open blades of the forceps, as being dangerous, and adhered to that mentioned by Sir Benjamin Brodie—namely, gently to depress the posterior branch against the posterior surface of the bladder, and allow the stone to fall in between the blades. He (Mr. Brooke) thought that the use of percussion had been too universally abandoned in lithotripsy. A moderately soft and friable stone may probably be as well crushed by screw-pressure, but a large and hard stone may be comminuted by percussion, that would not yield to the screw. Its laminated texture is partially disintegrated by repeated slight taps with the hammer, and a few smarter blows will then suffice to comminute it. According to his experiments, a hard stone is broken into large fragments by the screw, but is much more completely comminuted by percussion. An instrument with opposed blades comprises a much larger portion of the stone than that in which the anterior blade is made to pass through the posterior; and such may be completely closed by gentle percussion, the intervening *débris* being forced out in the form of mud, or paste, although it cannot be closed entirely by the screw, and is therefore very likely to lacerate the urethra on its withdrawal, as Sir Benjamin Brodie had mentioned in one or two of his first cases. In regard to dislodging fragments impacted in the urethra, he (Mr. Brooke) was satisfied of the validity of Heurteloup's maxim, that fragments lodged posteriorly to the aperture in the triangular ligament were generally more readily replaced in the bladder than extracted. This might be effected by passing a moderately full-sized sound down to the fragment, and directing the patient to make an effort at micturition. At the moment when the success of the effort is evinced by the escape of a few drops of urine by the side of the sound, the neck of the bladder is relaxed, and the fragment may be readily pushed back by the sound. He also alluded to the utility of Heurteloup's evacuating sound in cases of enlarged prostate, or feeble bladder, as well as to the facility of detecting small fragments, even when buried between prominent rugæ, by his vesical sounding-board, described some years since in the "*Transactions*" of the British Association.

Mr. Charles Hawkins thought that the great value of the paper consisted in its having given the profession a better opportunity than they had previously had of judging fairly between the merits of lithotomy and lithotripsy. Lithotripsy had been regarded originally as applicable to only a very small number of cases; but his (Mr. Hawkins') belief was, that it was really available in most of the cases in which lithotomy might be resorted to. Mr. Coulson had spoken of the danger which sometimes occurred from the escape of small fragments of stone from the lithotrite; he (Mr. Hawkins) had in several such cases succeeded in removing these fragments by the scoop, having previously elevated the patient. He thought chloroform should never be resorted to in lithotripsy, as it was an axiom in this operation, not to do more at one time than the patient could bear. Chloroform would prevent our being warned of this. In one case, however, he had been compelled, from the excessive irritability of the patient, to resort to this agent. The patient was operated upon four times, the bladder being materially relieved after each sitting. The case proved very successful. It had been said that when great irritability of the bladder existed, lithotripsy should not be resorted to; but this could not be regarded as a fixed rule. In one case in which the bladder was so irritable that the patient

passed water every quarter of an hour, and bloody urine followed any exercise, he had succeeded in getting the bladder to hold two ounces of urine, and in using the lithotrite twice. The case was cured; but even in forty-eight hours the patient had become quite comfortable as regarded pain. With a view to show that the large size of the stone did not necessarily prohibit lithotrity, he exhibited the fragments of a calculus, which had been broken up after twenty or thirty sittings, in a gentleman, seventy-five years of age. The fragments weighed two ounces and a half. The patient recovered. In this case the stone would have been difficult of removal by lithotomy. His own opinion in regard to the operation of lithotrity was, that if the patient was not in a state to bear it, he would have little chance from lithotomy. Mr. Aston Key many years since remarked, in *Guy's Hospital Reports*, that lithotomy would leave lithotrity far behind as a successful operative procedure.

Sir Benjamin Brodie could not enter into all the points which had been brought forward in the discussion. He would, however, speak on one or two of them. With regard to his mode of seizing the stone, he thought Mr. Coulson had misunderstood him. His practice was to raise the handle of the forceps, and seize the stone when it lay in that part of the bladder which was in contact with the rectum. In this proceeding all depended on the gentleness of the manipulation; no roughness must be resorted to; a rough hand might produce much evil. A reference to his own practice would prove his position. In the first few cases in which he had operated, from not employing sufficient care, inflammation of the bladder had followed; but after that, in consequence of his operating in a more gentle manner, no harm had followed in a single instance. There had also been no difficulty in seizing the stone, nor even the fragments, except on one or two occasions. He objected to the mode recommended by Mr. Coulson, of turning the forceps round, with the view of seizing the stone, as dangerous. He, himself, had never seen the bladder injured by lithotrity; but Sir Astley Cooper had mentioned a case to him, in which the forceps had been passed through the walls of the bladder into the belly. By the plan he had recommended there was no difficulty in seizing the stone, and he had found no necessity to lower the shoulders of the patient to effect this, except in a very few instances. With respect to the employment of the hammer, as advocated by Mr. Brooke, he (Sir Benjamin Brodie) contended that Weiss's screw crushed the stone as effectually and completely as did the combined screw and hammer of Heurteloup. All the chief French surgeons now used the screw: it was a less formidable operation in appearance, and occupied a smaller space of time. Allusion had been made to the statement of Mr. Key respecting lithotomy; but he (Sir Benjamin Brodie) knew that long after that was made, Mr. Key had repeatedly confessed the great comfort he had derived from having been able, in many cases, to substitute lithotrity for lithotomy. Lithotomy was an operation attended with the greatest anxiety to the surgeon, and he was better without it when his patients were in his private practice, and in affluent circumstances, for they rarely consented to an operation, until the kidneys had become diseased, or some other formidable complication had taken place. He mentioned that Cheselden had performed 280 cases of lithotomy, and had only lost twenty. It was probable that three-fourths of them were children. Cheselden, in later life, had met with a number of unfavourable cases; and it might be questioned how far his early retirement from practice might have been influenced by the mortification attending these failures.—*Lancet*; March 24, 1855.

49. *Large Prolapsus of Rectum successfully treated with Nitric Acid.*—Dr. CHRISTOPHER FLEMING, of Dublin, records (*Dublin Medical Press*, April 25, 1855) the following interesting example of this:—

A man, 24 years of age, of an unhealthy, strumous aspect, was admitted into the Richmond Hospital, March, 1853, labouring under an enormous protrusion of the anus and rectum, which rendered his life miserable. His countenance was pallid, his frame attenuated, and the smell from his person most offensive, his clothes being saturated with the profuse discharge from the anus, which escaped involuntarily, even when there was no protrusion. From childhood he

was delicate, having had repeated diarrhoeal and dysenteric attacks, to the last of which he attributed the date of his present disease, when about 12 years of age. At this period, and up to that of 15, he was apprentice to a tobacco manufacturer, subsequent to which his occupation was uncertain, but always laborious, being latterly that of a hodman to a bricklayer. During all this period his habits have been most irregular, his sufferings from hardship and from his disease extreme, and his diet uncertain and innutritious. At the age of 15, he contracted syphilis, and underwent protracted mercurial treatment. For his present disease he has been in hospital, but left without any appreciable benefit. He cannot remain for any time in the erect posture without the escape of the protrusion at the anus. He has constant and urgent desire to go to stool, and passes large quantities of fecid discharge, tinged with blood. The escape of feculent matter is most irregular, both as to amount and consistence, and is always accompanied with a sensation as if the whole contents of the abdomen were escaping, and with protrusion at the anus of a considerable tumour, which he is obliged to replace, or subject himself to extreme agony. There is a persistent, dull pain and uneasiness in the region of the rectum and pelvis, and his sufferings are less acute when the feculent discharges are solid than otherwise. He has little or no appetite, often nausea and vomiting, and is much tormented with flatulence—in fact, all the symptoms of aggravated dyspepsia are present. Under no circumstances has he irritability of the bladder. In the absence of any protrusion, the anus is patulous, and its circumference, covered with muco-sanguineous discharge, presents numerous ulcerated chinks in the direction of its rugæ. It is so wide, and the sphincter so relaxed, that a gaping, funnel-shaped cavity is exposed, within which the fingers, placed conically, passed with ease. It, and the greater portion of the rectum, protruded on the slightest exertion, and constituted a tumour of considerable size, presenting somewhat the appearance of a prolapsed uterus. The circumference at its base exceeded, by measurement, nine inches; and a line drawn from the base to the free end, where the orifice of the bowel existed, reached beyond four. The mucous membrane hung in loose, pendulous, and concentric folds, coated over with the ordinary tenacious and slimy secretion seen in such cases, and its sub-mucous tissue was thickened with copious infiltration. Its surface when first exposed was of a bright red, which, in proportion to the duration of the prolapsus, assumed a dark congested modena colour. It presented irregular patches of ecchymoses, of adherent lymph, and of extensive ulceration. It was by no means sensitive to pressure. On introducing the two forefingers within the intestinal orifice, grasping the protruded intestine between them and the thumb, and measuring the intervening depth from the anus, there was so much of thickness as to give the sensation of the complete prolapse of the bowel. It was curious to remark how he restored this vast protrusion, and sometimes he did so by muscular efforts alone, without the slightest manual help. He bent forward, pressed his hands on his knees, when a gradual diminution took place in the bulk of the protruded bowel, and with a sudden gulp it disappeared, all being apparently effected by a combined and violent muscular effort. Again, he failed in this, and was obliged to assist it with his hands. It is unnecessary to specify the details of treatment during the months of March and April, further than to state that the horizontal position was enjoined, diet properly regulated, the regular action of the bowels insured, and all the ordinary local means adopted to restore the healthy condition of the protruded bowel. On the 16th of April, although much dissatisfied, he acknowledged some improvement as to the amount of protrusion, and as to the discharge from the bowel, and the latter was obvious from the altered state of his linen, which was considerably less stained. The ulcers around the anus had a much more healthy appearance, and the rectal mucous surface was less loose and pendulous, much less congested, and the excoriated and ulcerated patches healed; yet the protrusion was considerable. Any operative measures appeared to me quite unsuited to his case, as well from the nature of his constitution as from the large extent of surfaces which should be necessarily implicated in them. It occurred to me that the object sought by such could be accomplished by another expedient; and with that view I formed four equidistant radiated tracts with concentrated nitric

acid, commencing them near the intestinal opening, at the apex of the protrusion, and passing them as near as I could calculate to the upper margin of the inner sphincter. The breadth of these tracts was about a quarter of an inch, and the length two inches or so. They were well smeared with oil, and the bowel carefully returned. No pain was complained of during or subsequent to the application of the acid, which could be justly attributable to it—it was similar to that always complained of—it was relievable by the same expedients—it was not more severe during the act of defecation. There was no vesical, no abdominal irritation. From day to day there was noticed a diminution in the amount and in the frequency of the protrusion, and the irritability and uneasiness about the rectum and verge of the anus were less.

April 30. Fourth day from the application of the acid; report most favourable as to the progress of the case, and the sensations of patient; all prominent symptoms improved. I made a careful examination of the rectum, and found in the nitric acid tracts many deep, ulcerated sulci, with defined edges; the lateral ulcerations were much more marked than the anterior or the posterior; there was no tenderness of any amount worth noting, and there was decidedly less of laxity of the mucous membrane. The ulcers at the edge of the anus were more healthy; I touched them over with sulphate of copper.

25th. Progress good; discharge from the rectum and anus much less; local uneasiness comparatively slight; general health greatly improved, and feeling of great comfort and relief experienced in consequence of the absence of tenesmus and frequent prolapse, as heretofore. Feculent discharges once daily, and without pain, and subsequent easy returns of bowel.

28th. Improvement in every particular progressive; obvious diminution in bulk and shape of protrusion, and mucous membrane much more tensely spread over it; ulcerated patches visible, but more limited and superficial; proportionate decrease in discharge. Two ounces of an astringent lotion to be thrown into rectum morning and evening.

May 3. Fifteenth day from first application of nitric acid; report most satisfactory; protrusion of bowel less frequent, occurring only once in twenty-four hours; mucous discharge also much diminished; defecation natural and without pain: can cough, sneeze, and use any exertion without any protrusion; also walks about without any annoyance; outlines of previous ulcerated patches merely visible, and intervening portions of mucous membrane much more fixed, but yet loose and flabby. Pencilled those spaces with nitric acid, as in the first instance, but less extensively; effect apparently now more productive of pain, yet not very acute, and easily controlled by hip-bath and anodyne enema. Complete subsidence of all uneasiness after a few days; subsequent steady and progressive improvement in tone and in functions of bowel, so much so that on the 14th of May the patient was very much improved. The bowel has not now descended for many days; its contents are discharged regularly once in the twenty-four hours, and without pain, and without any appreciable amount of prolapsus, and the discharge from the anus is much less.

Further more minute details of progress or of treatment would be here inappropriate. The case was witnessed whilst in hospital, at its different stages, by a very intelligent and observant class of pupils. The man left of his own accord on the 4th of June, having then had full control over the prolapsus, and the most healthy action of the bowel being established. He moved about without any inconvenience; was recommended to go to the sea-side, to bathe, and to attend to the state of his bowels. In August, he called on me, on his way with his fellow-workmen to see the Industrial Exhibition, and expressed himself as being perfectly free from the prolapsus or any other annoyance. I then examined the rectum, with Dr. McKenzie, of Edinburgh, who happened to be in my study at the time, and heard his report of his condition. The anus was loosely contracted, yet not patulous, and the ulcerations around appeared to have perfectly healed.

There is every reason to assume that the cure has been permanent.

The very exaggerated form of prolapsus in this case is of rare occurrence in adult man, in the absence of injury or of disease, causing an inefficient, if not complete suspension of the action of the sphincters. Doubtless, very large pro-

trusions of the anus and of the rectum do occur, singly or conjointly, arising from causes not now under consideration. The operative proceedings suggested by different authors may be suitable to special examples of those cases; but they are in very few, indeed, that I am aware of, persistently beneficial, and in all they are more or less hazardous. Those operations have for their object, mainly, the more perfect and the more complete closing of the sphincter, either by its shortened and divided fibres, or by the consolidation of such with the anal orifice, tegumentary or mucous. I am of opinion, however, that the result of the above case may lead to the hope that a milder mode of proceeding may be adopted, and that the agglutination of the mucous wall of the rectum to its muscular, by the establishment of healthy inflammatory action, can be effected in the manner I have detailed, so as to enable the special muscles about the lower aperture of the rectum to perform the natural and requisite offices. Moreover, the cicatrization of the several ulcerated chinks, the necessary result of the procedure, will tighten up the mucous membrane, so as to render permanent such union. In the particular locality I have marked, away from the external orifice, the mucous membrane appears to be as little sensitive as the tracheal mucous membrane is distant from the larynx; and hence the comparative freedom from pain in the application of the remedy.

50. *Nitrate of Silver for the Cure of Prolapsus Ani.*—Mr. LLOYD treats prolapsus ani by smearing the whole surface of the protruded bowel with solid caustic, and then returning the bowel. The application is repeated once in a week or fortnight, as may be requisite. Mr. Lloyd states that he rarely found it necessary to employ it more than three or four times; and further, that although the plan had been one invariable resort with him, for a long series of years, that he had never known any untoward consequences to result. In cases in which the protruded bowel has become swollen, and is difficult of reduction, the effect of the caustic is surprising. In one such case, the mass could be easily seen to diminish in size under its influence. Mr. Lloyd does not limit the use of this remedy solely to prolapsus, but adopts it also in cases of hæmorrhoidal congestion, and thickening of the mucous membrane about the verge of the anus.—*Med. Times and Gazette.*

51. *Particular Method of Applying Cauterization for the Reunion of Anomalous Fissures, and especially those of the Palate.*—M. CLOQUET proposes, in divisions of the *velum palati*, to take advantage of the great amount of retraction which occurs in the cicatrix consequent on burns.

We need not in such cases, says M. Cloquet, cauterize the edges of the fissure throughout their whole length, converting them into a granulating sore, the cicatrization of which must be afterwards assisted by sutures, appropriate bandages and the maintenance of correct adaption. This method, long known to the profession, sometimes succeeds, but often entirely fails. The one which I propose, adds M. Cloquet, consists in applying the cautery to the angle of the fissure, and that only to a limited extent; leaving the contraction of the cicatricial tissue to operate, and then, practising a similar cauterization, and waiting for some time to renew the application in such a way as by repeated operations to bring the edges of the division towards each other, and to unite them by a succession of cicatrizations which may be regarded as so many successive points of suture. The double advantage is thereby gained of being thus enabled to watch, step by step, the results of the treatment, and to obtain unions of the most difficult nature by an operation, simple, scarcely painful, and exempt from all danger. It is especially in fissure of the palate that M. Cloquet considers the advantages of this mode of operating as incontestable, and he mentions four cases in all of which the operation had been attended with equally successful results. There had been no pain felt, no change in habits or regimen necessary, and no complications had arisen. The operation was of the most simple nature, every surgeon could perform it. It required the aid of no assistant, an advantage of great importance in country practice; and, lastly, it could be practised on very young children. One objection urged against this method was the length of time required for completion of the cure, but the

slowness of its action constituted its safety, and the inconvenience arising in this way was very small, as the patient experienced no alteration in his health or habits during its progress.

The cauterization may be effected by two different means; namely, either by caustics or the actual cautery. M. Cloquet states that in the first of those cases where he attempted this method, he used, as the cauterizing agent, the acid nitrate of mercury, and succeeded completely. However, he prefers the actual cautery, its action being deeper, almost instantaneous and consequently less painful, while it occasions a more firm cicatrix, and one which becomes more rapidly organized. The three other patients were treated in this manner, and the results obtained confirmed his opinion on this point. An almost insurmountable obstacle to its employment might be, however, occasionally met with in the terror of the patient. But, fortunately, science provides us with a means of obviating this inconvenience; as a platina wire introduced within the mouth, before the electric circuit is completed, cannot excite the patient's alarm, and as it can afterwards by this means be brought to a white heat, and be kept incandescent for any length of time, the surgeon is enabled to act with all the calmness and precision desirable.—*Monthly Journal of Medicine*, May, 1855, from *Gazette Médicale*, March 3.

52. *On the Employment of Tracheotomy in Croup.*—M. TROUSSEAU loses no opportunity of bringing before the Profession the claims of this operation, which he believes have not met with due acknowledgment, especially in Britain. He states that his employment of it has been more successful than ever during the last year, for of nine operations he has performed, recovery has been the result in seven. During the last four years he has operated 24 times in private practice with 14 recoveries; and at the Hôpital des Enfants Malades tracheotomy was performed 216 times, with 47 recoveries, almost a fourth. This is a considerable result when we consider the social condition of the children brought to the hospital, the injudicious treatment they had usually already been submitted to, and the disastrous condition in which they are placed after the operation, surrounded by various foci of contagion, so that when all seems going on well, scarlatina, variola, or pertussis may induce the most dangerous complications. M. Trousseau feels convinced that in civil practice success will attend full one-half of the operations, provided they be undertaken under conditions rendering success possible. This qualification is important, for if diphtheritic inflammation has deeply contaminated the system, so that the skin, and especially the nasal fossæ, exhibit the special phlegmasia, if the frequency of pulse, delirium, and prostration indicate a complete poisoning of the system—the peril being rather in this general condition than in the local lesion—the operation should never be attempted, as it is then always fatal. But if the local lesion constitutes the principal danger, at whatever degree the asphyxia may have arrived, the child having but a few minutes to live, tracheotomy will succeed almost as well as if performed three or four hours sooner.

M. Trousseau has now performed the operation above 200 times, and he particularly insists upon its being executed with due deliberation, without any attempt at display. The double canula must always be employed, and as large a one as can conveniently enter the trachea. The operation completed, the most urgent thing to attend to is the feeding of the child, for, under the influence of abstinence the absorption of external miasmata, and of the vicious secretions fabricated within the body is favoured, and the power of resistance is enfeebled. Without gorging the child with food its appetite when present must be satisfied, while when there is none it must still be forced to eat, and by feigning intimidation M. Trousseau has got children to eat who otherwise would have been lost. Milk, eggs, chocolate, and broths form the most suitable diet.

The much greater success which has attended his operations in late compared with former years, M. Trousseau attributes in part to practitioners not previously exhausting the patient's strength by bleeding and blistering so much as formerly. After the operation, all medicinal treatment must be dis-

continued, as interfering with due alimentation. If blisters have been already applied, they must be healed up by means of rhatany or Goulard ointment, pencilling the surface with nitrate of silver if diphtheritic exudations be present.

Apologizing for the apparent minutiae to which he calls attention, he observes, that the longer he lives the more he is convinced of the importance of such details in therapeutics. Between the canula and the skin a small strip of oiled silk or caoutchouc should be interposed, and the relatives should be taught to remove and cleanse the inner canula every two or three hours. The neck should be surrounded by a knitted comforter or a large piece of muslin, and the infant should breathe into this, so that the inspired air may become impregnated with some of the warm vapour furnished by expiration. This precept is very important; for by its aid we prevent the drying of the cavity of the canula and the trachea, and thus avoid irritating the mucous membrane and the formation of coriaceous crusts, which, becoming detached, may cause dangerous suffocative paroxysms, by obstructing the tube. Before the author and M. P. Guersant adopted this plan, they lost several patients by catarrhal pneumonia, which is now of much rarer occurrence. Another practice, in the neglect of which a cure is rare, consists in thoroughly pencilling the entire surface of the wound daily with nitrate of silver. We in this way prevent the dangerous formation of thick, fetid, false membranes on its surface. This specific inflammation may also become propagated to the cellular tissue and develop phlegmonous erysipelas, leading to local gangrene, or at least violent symptomatic fever and a general infection of the economy which rarely spares the patient. By the fifth day, the surface of the wound has become so modified that these accidents are no longer to be feared.

Finally, the removal of the canula and definitive closure of the wound require attention. The canula is rarely removable before the sixth day or later than the tenth, and in some cases the larynx remains quite closed for fifteen, twenty, or even forty days. At the end of the first week we should take it out with great care, so as to avoid making the child cry. The infant having become accustomed to breathe by the artificial mode, may be seized with a paroxysm of fear and difficult respiration on the first removal. There may be some obstruction of the larynx, by slightly adherent false membranes, mucus, or tumefaction; and the laryngeal muscles may have somewhat lost the power of harmoniously contracting. The difficulty of breathing usually soon disappears if the child can be kept quiet, and, according to the degree in which the laryngeal passage seems re-established, the wound may be strapped up with court-plaster, or left for a day longer covered with ointment or lint. If the air does not pass at all, the canula must be replaced for a while longer. When respiration is re-established, the opening in the trachea is usually closed in four or five days, and the external wound heals soon after.

Sometimes after the operation there is a difficulty in deglutition, consisting in the passage of fluids through the glottis, and its penetration into the trachea and bronchi, creating great irritation. Besides this irritating effect, the child acquires an invincible disgust for its food, and will die rather than take nourishment. The best means of treatment is to avoid liquid diet, giving solid or semi-solid substances, allaying thirst by a little cold water, given just before or long after the repast, so as to avoid exciting vomiting. The inconvenience usually commences three or four days after the operation, and rarely continues longer than from the tenth to the twelfth day. It would seem that the larynx, which thus permits liquid aliments to pass, should allow the passage of the air also; but it is not so, for if we remove the canula, the passage will be found insufficient. M. Archambault, who has paid much attention to this complication, believes that it results from the child having, by the use of the canula, lost the habit of moving the muscles which close the larynx, in harmony with those which propel the food; and he has found it advantageous to temporarily close the canula with the finger during the attempt at deglutition, the child then being obliged to bring the laryngeal muscles into action, and the harmony becoming re-established. This stratagem, however, sometimes completely fails.—*Med. Times and Gaz.*, April 7, 1855, from *Archives Gén. de Méd.*, March.

53. *Paracentesis Thoracis*.—M. TROUSSEAU records the following interesting case: A man, æt. 16, was admitted on the fifth day of an acute disease, with severe frontal headache, lassitude, and præcordial pain. There was intense dyspnœa, a little cough, a very quick pulse, prominence in the cardiac region, increased percussion, dulness at the same point, extending to the second rib, and to the right of the sternum; the heart's sounds were feeble and distant. No mention is made of rheumatism, and it is to be presumed that it was absent. For a month the effusion continued the same; only once, for two days, did it seem to diminish, and then there was a little friction at the base. Afterwards, the dyspnœa increased and the fluid augmented as the dulness now reached to the clavicle. There was also pleuritic effusion. It was determined to puncture the pericardium: this was done by an incision in the fifth intercostal space, three centimetres ($= 1\frac{1}{4}$ inch) from the sternum; thirteen ounces of fluid slowly escaped, the cardiac dulness decreased in amount, respiration could be heard in the lung as low as the fourth rib. A day or two afterwards, the pleuritic effusion was found to have increased; the heart was displaced to the right. Thoracentesis was practised in the sixth space in the axillary line, and although at first the canula was blocked up by false membrane, eventually sixteen ounces of fluid were removed. Neither pleural nor pericardiac effusion reappeared, but, soon after, signs of tuberculosis of the left lung came on.

The authors refer to the other (7) cases of paracentesis pericardii on record.—*Brit. and For. Med.-Chirurg. Rev.*, Jan. 1855, from *Archives Gén.*, Nov. 1854.

54. *Enormous Fibrous Tumour of the Neck extirpated by the method of Morcellement—Recovery*.—On the 8th Aug. last, M. MAISONNEUVE presented to the Imperial Academy of Medicine, a woman, æt. 35, from whom he had removed a very large fibrous tumour of the neck, which other eminent surgeons had pronounced beyond the reach of art. He extirpated it by the method he terms *morcellement*, which consists in dividing it into pieces. He had first employed this method in the case of large fibrous tumours of the uterus, which were not extricable on account of their enormous size.

The tumour was of two years' standing, and had latterly increased in bulk so much as to render suffocation imminent. It occupied the whole of the left side of the neck, and extended vertically from the mastoid process to below the clavicle, and transversely from the spinous processes of the vertebræ to behind the larynx and trachea, which were considerably displaced by it to the right side. The carotid artery and internal jugular vein were visible on its external surface. It was slightly nodulated on its surface, and it had the firm resistant feeling of fibrous texture. The skin glided with ease over its surface; but the tumour could not be moved over the deep-seated textures. It was not painful to touch. The arm was neither painful nor swelled, but retained perfect freedom of motion. M. Maisonneuve diagnosed the existence of a fibrous tumour adherent to the transverse processes of the vertebræ, and he considered that the important vessels and nerves of the neck were not involved in its substance, for two reasons: 1st. The unimpaired condition of the sensory and motory functions of the head and arm; and 2d. The fibrous character of the growth, and its intimate attachment to the vertebræ. His anatomico-pathological researches had long satisfied him that *tumours which have a periosteal origin always remain isolated and distinct from the soft parts in the midst of which they are developed*. He, therefore, resolved to attempt its removal, and the poor patient, on learning his decision, wept tears of joy. Chloroform having been administered, a vertical incision was carried from the mastoid process to the clavicle, and another transversely from the larynx to the spinous processes of the vertebræ. The flaps of this crucial incision were laid down, and the tumour was extirpated by a long, laborious, and most delicate dissection, in the course of which, the carotid, the internal jugular vein, the pneumogastric nerve, and the cervical and brachial plexuses were exposed. On account of the firm adherence of the tumour to the adjacent parts, it was found impossible to extract it *en masse*; M. Maisonneuve therefore resolved to divide it into two equal portions by a transverse incision. This greatly facilitated his operations; and saved the important structures from injury. Portions of the scaleni muscles were unavoid-

ably removed in the dissection. Maisonneuve had to subdivide one of the halves of the tumour, but he succeeded in removing the whole of it, and completing the operation in three-quarters of an hour. There was very little loss of blood, owing to the extreme care taken to avoid wounding large vessels; and the patient was under the influence of chloroform the whole time.

The wound left by the operation was a deep and ghastly excavation, at the bottom of which lay the lower six cervical vertebrae, the first rib, the cervical and brachial plexuses, the larynx, trachea, pharynx, and œsophagus—all exposed to view! M. Maisonneuve, anxious to avoid suppuration and obtain union by the first intention, brought the tissues together by means of adhesive plaster, *serres-fines*, and properly regulated pressure. In three days, four-fifths of the wound had cicatrized, and the remaining portion was filled with healthy granulations. In a month the patient was quite well; the parts around preserved their normal site; the arm had no impairment of its sensibility or motion; the voice was clear, and deglutition was easily performed.

The tumour was purely fibrous, and of a dense hard texture. It weighed 475 grammes (*i. e.*, nearly 8 lbs).—*Month. Journ. of Med. Sci.*, Dec. 1854; from *L'Union Médicale*, 10th August, 1854.

55. *Extirpation of the Uterus*.—Dr. REICHE states that he has extirpated the entire uterus seven times; in all cases the result was fatal. He, however, advocates the operation in cases of cancer confined to the organ. He then describes the method of operating; this presents nothing calling for analysis. Partial extirpation he represents as a painful operation, but one free from danger. It is indicated in all degenerations limited to the neck of the uterus.—*Brit. and For. Med.-Chirurg. Rev.*, April, 1855, from *Deutsche Klin.* 43. 1854.

56. *Excision of the Head of the Humerus, by a New Method*.—M. BAUDENS, in a memoir recently (Feb. 26, 1855,) read before the Imperial Academy of Sciences, insists on the duty of endeavouring to enlarge the sphere of conservative surgery, and to seek to substitute excision for amputation, the *ultima ratio*. This object he has always had in view. In 1836, he announced, in his *Clinique on Gun-shot Wounds*, that “excision should be the rule, and amputation the exception, in cases of comminuted fracture of the upper extremities.” “Since the additional observations made on the hundreds of wounded admitted into Val de Grace, in February and June, 1848,” adds M. Baudens, “we are able to go farther, and to prove that the majority of fractures of the leg, the result of gun-shots, may be preserved from amputation by removing the splinters, and by the application of ice and of our fracture apparatus.” Conservative surgery is not merely limited to the avoidance of mutilations, it diminishes the mortality (*son necrologe est moins chargé que celui de l'amputation*).

The advantages claimed for excision by M. Baudens are, that it produces less shock, leaves a less extensive surface for traumatic inflammation, and is more likely to be followed by speedy union.

M. Baudens has employed excision of the head of the humerus in preference to amputation at the shoulder-joint, with the gratifying result of thirteen recoveries and but one death; hence, he feels authorized to reverse an established rule, and to say that “*excision, when a ball has broken the head of the humerus, ought to be the rule, and amputation the exception*.”

This operation dates back more than a century, having been first performed by Thomas, a surgeon of Pezenas (Languedoc), in 1740, for necrosis of the head of the humerus. Subsequently, Boucher, in a memoir on gun-shot wounds, and after him, Percy and Larrey, showed that by removing the shattered head of the humerus, the arm might be saved. This operation has been most successfully performed by White, Vigars, Moreau, father and son, etc., in cases of disease.

According to M. Baudens, the real value of the operation has not as yet been fully decided. Larrey, he says, thinks that the use of the arm is greatly im-

paired, and that the subjects of this operation are unable to raise the arm on the scapula.¹

In 1854, M. Gimelle gave the following opinion, in a report to the academy: "Resection of the head of the humerus reduces the arm to impotence; it falls like the pendulum of a clock, and is often of more annoyance than use." M. Sedillot gives the same opinion. (*Médecine Opératoire*, p. 497.) From the results of M. Bauden's experience, he disagrees entirely with these opinions. "On all occasions," he says, "in which excision is limited to the head of the humerus, we have always found the motions of the arm to be restored; but the new scapulo-humeral articulation becomes a ginglymoid instead of an arthrodial joint."

I. Means of obtaining a New Articulation.

In order to obtain a new articulation, two indications must be rigorously carried out. 1. To keep the humerus in *immediate contact* with the glenoid cavity. 2. To take as much care as possible of the muscles and nerves of the shoulder. Hence, M. Baudens says, that of the two methods recommended of performing this operation, viz: by flap or single incision, he has no hesitation in selecting the latter. He objects, therefore, to the operation as performed by Moreau, Manne, Sebatier, Bent (of Newcastle), Morel, Syme, etc., all of whom employed modifications of the flap operation. The method by single incision was that originally performed by White. The difficulties of this mode of procedure, he admits, are greater. These difficulties are stated to be twofold: the first, arising from the energetic contractions of the deltoid muscle, which render it difficult to expose the head of the bone; the second, from the head of the bone being drawn high up into the coraco-acromial vault by the contractions of the scapular muscles. These muscular contractions are the "Gordian knot" of the operation. To overcome them, says M. Baudens, the tuberosities are by rotation to be alternately brought into the centre of the incision, and the muscles divided, and with them the capsule which is confounded with their insertions, by which an opening is made sufficiently large to give exit to the head of the humerus. These peculiarities, which, according to the author, were not previously known, led M. Baudens, in 1833, to modify materially the method of White. Instead of making the incision externally (or nearer the acromion), as White did, M. Baudens makes it *internally*, for three reasons: first, because the head of the bone is here more superficial; secondly, because the incision can be prolonged as high up as is necessary to expose all the bone; whilst, thirdly, it enables the operator to come down at once upon the bicipital groove, and thus upon the muscles (capsular), the division of which forms the key to the whole proceeding. "The advice given by authors to cut the capsule first, and the four rotator muscles afterwards, is precisely the reverse of what ought to be done." The capsule need not be specially sought after; "all that is necessary is to make the section of the muscles, at the same moment the capsule will be found to be largely opened; then the head of the humerus, which had been drawn up into the arch beneath the acromion and coracoid processes, and held there as if in a vice, immediately descends, and from that moment can easily be made to project through the opening in the capsule."

Mode of operating, as performed by M. Baudens. The operation is divisible into four stages or periods:—

First Stage.—The arm being everted, the point of a small amputating knife is to be passed directly down to the head of the bone, on the outside of the coronoid process, and carried downwards for about five inches, keeping the knife in contact with the bone.

¹ We scarcely think M. Baudens has done justice to Larrey in this matter. Larrey, we are of opinion, has fully established the propriety of excising the head of the humerus when practicable. In his work on *Military Surgery*, he says: "I have had the good fortune, on ten different occasions, to supersede the necessity for amputation at the shoulder, by the complete and immediate extraction of the head of the humerus, or its splinters, without delay; seven were cured, three died." "In some, the arm became ankylosed to the shoulder, and in others an artificial joint, allowing of motion, was formed."—*Translator*.

Second Stage.—If necessary, cut a few fibres of the deltoid transversely, without wounding the skin; open up the bicipital groove, and cut across the tendon of the biceps.

Third Stage.—By rotating the arm alternately inwards and outwards, we are able to expose, first, the greater, then the lesser tuberosity, and freely to cut across the muscles inserted into them.

Fourth Stage.—The capsule having been thus opened, the elbow is to be carried backwards and upwards, so as to luxate the head of the humerus, and make it protrude through the wound; then, with the saw, remove the bone, sparing, as much as possible, the posterior attachment of the capsule and the periosteum. [M. Baudens attaches much importance to this point, in consequence of the *belles decouvertes* of M. Flourens, on the regeneration of bone by the periosteum.]

Fifth Stage.—Tie the vessels, cover the extremity of the humerus with the preserved periosteum, as with a hood, keep it in direct contact with the glenoid cavity by propping up the elbow with a bandage, and endeavour to obtain union of the soft parts, except at the lower angle of the wound, where a tent is to be inserted to allow matter to escape.

II. *Within what limits is the incision to be confined?*

M. Baudens enters at length into this question. The operation admits of several modifications; but the grand rule is to save as much bone as possible. Thus, 1. When the head of the bone is but partially injured, he has been able to remove the half only. This, he says, has never been previously done. 2. All the head may require to be removed; but it is very important not to encroach on the anatomical neck, and, at all events, to take care of the attachments of the capsule posteriorly. 3. If the anatomical neck of the humerus must be divided, the insertions of the muscles are to be preserved as much as possible. 4. In one case, where the humerus was greatly comminuted, I removed, says M. Baudens, the humerus below the insertion of the deltoid. "Here, the bone being connected by soft parts only, was deprived of the movements of the scapula, but the motions of the forearm and hand were preserved intact. Even here, the result was far more satisfactory (*magnifique*) than is ever obtained by amputation at the shoulder-joint." 5. In one case, M. Baudens removed, with success, the head of the humerus, the glenoid cavity, the acromion process, and all the spine of the scapula.

III. *When the fracture of the head of the humerus extends downwards into the shaft [split fracture], is this a case for amputation?*

Larrey does not hesitate to answer in the affirmative. "Amputation," he says, "is in that case indispensable." [*Cl. Chirurg.*, tome v. p. 183.] This opinion, which is likewise that of many other surgeons, is not the opinion of M. Baudens. "In four cases," he says, "we were content to remove only the head of the humerus, without minding the fissures which ran, more or less, down the shaft of the bone into the medullary canal, and recovery took place just as if these fissures had never existed." M. Baudens adds that he has often seen, in amputation for gun-shot wounds, longitudinal fractures in the bone which formed the stump, from which no unpleasant consequences resulted. "The fissures got well as simple fractures do."

In amputation of the thigh for comminuted fractures, this is a point of much importance, as the mortality is much influenced by the proximity of the amputation to the trunk. M. Baudens says, the limb should be removed close to the fracture; and adds, "that he has more than thirty times carried this rule into practice, and never regretted leaving a fissure in the osseous tissue." Longitudinal fractures, he adds, are so frequent, as the result of gun-shot wounds, "that it would be very difficult to avoid them in amputating, as we are often in ignorance of their precise extent; but fortunately they remain unobserved as to their results."¹

¹ This opinion, we think, should be received with some reserve. In a case of comminuted fracture, near the centre of the tibia, we have seen a fissure to extend up into the knee-joint, which, becoming inflamed and subsequently disorganized, rendered amputation of the thigh necessary. Had the injury been so severe as to

IV. *Is it absolutely necessary to excise the head of the humerus when it has been broken by a ball?*

M. Baudens says, from his experience, the operation is *always* requisite. When this is not done, we learn that "one of three results are observed: 1, death from purulent infection; 2, the necessity for secondary excision; or, 3, the patient survives with ankylosis, or fistulous openings, in a state of suffering and peril, the issue of which is uncertain."

V. *Is immediate excision to be preferred to secondary excision?*

M. Baudens furnishes the following statistics:—

"In twenty-six cases, the head of the humerus was thus injured. In eleven, excision was *immediately* performed; ten recovered, one died; in fifteen, the operation was deferred in the hope that, because their wounds perhaps were less severe, they might recover without an operation. Of these fifteen, eight died of purulent infection; three underwent secondary excision (all of whom recovered), and four survived with fistulous openings.

"Hence, we see the success of three secondary excisions was dearly bought, since from not having operated at once, eight of the wounded died of purulent infection."

The remainder of the memoir is occupied with illustrative cases, which are of extreme interest, and which corroborate the principles laid down in the preceding pages.

57. *Dislocation of the Astragalus Backwards and Inwards.*—Prof. WILLIAMS communicated the following case of this accident:—

"Stephen Phillips, labourer, aged 54, of spare habit and sallow complexion, who has uniformly enjoyed good health, and lived well and temperately, was admitted into the City of Dublin Hospital on the 12th of April, 1852.

"He states that on the previous day he had been at work, along with some other men, beneath an overhanging bank of earth and gravel in the granite quarry at Kingstown, which they were undermining and removing in order to expose the granite strata underneath. At the time the accident occurred, the patient and another man were engaged shovelling the loose earth, as it was detached, into a truck or wagon, which was placed behind them on the "tram way." Whilst thus employed, another man ascended the bank, and without any warning to those below, struck a heavy iron bar, called a "clay bar," into its upper surface, when it suddenly gave way, and being precipitated over, struck the patient violently on the left side of the thorax (he was on the side of the wagon next the falling earth), he standing with his chest parallel with the front of the bank previous to its fall; but *when struck*, his body was partially rotated backwards and to the right side, as he was in the act of throwing a shovelful of earth into the wagon, which stood behind and to the right side of him. The force of the concussion threw him violently backwards across the roadway. The remainder of the earth fell over the lower part of his body, fixing his feet and legs to the ground. Though very much hurt, he says he was not at all confused, and describes the accident as having taken place exactly as above stated. He also says that he does not think the mere weight of the falling earth was sufficient to produce the injuries received, as he was easily able to withdraw the injured limb without assistance; the right leg having been more heavily covered, had to be dug out before he could be completely extricated. He says he suffered acute pain in the left ankle from the moment of the accident, and it soon became swollen, and he also had severe pain in the left side of the chest. He was carried to a house in the neighbourhood, and the next day was admitted to the City of Dublin Hospital under the care of Dr. Williams.

"He complained of pain and a stitch on taking a full inspiration in the left side of the chest, and on examination the fifth and sixth ribs of the left side

require amputation, in the first instance, according to M. Baudens' rule, it should have been performed just above the seat of fracture, and in that case, secondary amputation would have been required for the subsequent disorganization of the knee-joint. The case alluded to was treated in the Richmond Hospital by Dr. Hutton.—*Tr.*

were found to be fractured about their centres. The left ankle-joint presents some curious and unusual appearances. The anterior relations of the tibia are very little disturbed, the anterior aspect of the foot being free from deformity, except indeed a nearly imperceptible shortening of the foot, which is a little everted. He has free motion of his toes and some slight motion of the ankle-joint. Motion of the latter increases the pain, which is of a "burning" character. There is no fracture of either the tibia or the fibula, and there is some swelling and ecchymosis, with a hard tumour of an irregularly convex shape, lying between the inferior extremity of the tibia, the tendo-Achillis, and os calcis. Professor Williams came to the conclusion that this tumour was formed by the astragalus, which was dislocated backwards and inwards, and also so rotated on its antero-posterior axis that its superior articulating surface looked almost directly inwards. A slight attempt was made to replace the bone, but was speedily abandoned, both because of the impediment to reduction presented by the above-mentioned rotation, and of the risk of further injury to the swollen and ecchymosed soft parts covering the astragalus. The limb was then placed on a double inclined plane, so arranged that the leg lay horizontally. Leeches were applied to the joint, and subsequently cold water irrigation."

Professor Williams said he would not detain the Society by reading the details of the progress of the case, which had been fully and accurately taken by Mr. Taylor. It would be enough to say that the soft parts covering the displaced bone inflamed, and on the eighth day, when it was evident that their destruction was inevitable, they were divided by a crucial incision, which gave exit to a little sanious discharge, and exposed the bone with its superior articular surface looking inwards. On the fourteenth day the bone was removed (some strong ligamentous attachments, which still held it, being divided with a probe-pointed bistoury guided by the forefinger), and it was then found that the astragalus had been fractured as well as dislocated, its head and a portion of the inferior surface having been broken, or rather *'ground off'*, and a quantity of the resulting small fragments were removed. The limb was then replaced in its previous position. Nothing requiring to be particularly noticed occurred until a fortnight after the removal of the bone, when an abscess formed below and behind the external malleolus, and was opened. The discharge both from this abscess and from the cavity from which the astragalus had been removed, now rapidly diminished, and the parts presented a very healthy appearance. Matters went on favourably till about 6th of May, when some trouble was occasioned by stripping of the integuments over the sacrum and os calcis, in spite of every precaution that had been taken to guard against both. This, however, was remedied by attention to position, and on the 7th of June the limb was replaced in its original posture. From this time he went on steadily, but very slowly, improving; and on the 10th of August the cavity whence the bone had been removed had cicatrized, leaving a deep depression.

The foot was in an exceedingly slight degree extended, but not permanently so, for he possessed some power of moving the ankle and was able to bring the foot to a right angle with the leg. He now began to move about on crutches, and at first the foot, when allowed to depend for some time, became painful and œdematous, but that inconvenience was relieved by careful bandaging, and soon ceased. Towards the latter end of August he left the hospital to go to the country, and was then able to walk pretty well with the aid of a stick.

Nothing was seen or heard of the patient until ten months after he had left the hospital, when he returned and stated that he had resumed work, though not of so laborious a kind as before, but that the extension of the foot had gradually increased, so as to cause considerable inconvenience in walking. The tendo-Achillis was now divided subcutaneously, and the foot was brought to about the same position it had been in when he first left the hospital. He was then provided with a high-heeled shoe, and left the hospital considerably improved, but using the help of a stick in walking.

Professor Williams said he was chiefly induced to bring this case before the Society, because it was rather curious so little attention has been directed to dislocation of the astragalus backward; in fact, it has been scarcely noticed by systematic writers on surgery. Benjamin Bell, indeed, says that the astragalus

may be displaced backwards, but he apparently does so wholly on theoretical grounds, and merely as contemplating the possibility of the occurrence; but Professor Williams could not then remember any other systematic writers who alluded to the subject, except Mr. Lizars and Mr. Liston, who have each very briefly mentioned a case that occurred in their own practice respectively, and Mr. Liston observes that he never expected to see another. On the other hand, this dislocation was not noticed by Miller, Fergusson, Bransby Cooper, Skey, Pirrie, or Erichsen, and it had even escaped the great experience and research of Mr. South. The French systematic writers were quite silent on the subject. MM. Vidal de Cassis and Nelaton, for example, the most recent of them, say nothing about it; and yet it was scarcely necessary to say before that Society, that there are several cases of dislocation of the astragalus on record. The accident, however, was undoubtedly a rare one, as only six cases of it, he believed, had hitherto been published; and the fact that so few cases of the kind had yet been recorded had chiefly induced him to bring before the Society the present case, which would make the published examples of dislocation of the astragalus backwards amount to seven in number. There were, however, one or two points respecting which he would say a few words.

The six cases already known are collected by Mr. Turner in his valuable monograph on "Dislocations of the Astragalus" (together with a case inadvertently quoted from Boyer as an example of that accident); and in two of those cases (Mr. Phillips') the bone appears to have been thrown directly backwards, as it is stated that in one the tendo-Achillis was forced back by the bone, so as to form an angle of 45 degrees, and that the appearance in the second case was exactly similar. In one case (Mr. Turner's) the displacement was backwards, outwards, and downwards; and in three (those of Mr. Lizars, Mr. Liston, and one recorded in the *Lancet*, that was admitted to University College Hospital), the astragalus was dislocated backwards and inwards. In the case read that evening, the displacement was also in the latter direction, so that in four of the seven cases now known, the astragalus has been backwards and inwards.

In only one of these seven cases (that recorded in the *Lancet*) was either the tibia or the fibula fractured; and in that case both those bones were broken at the level of the ankle-joint. This case is also the only one of the seven in which reduction was effected; and as Mr. Turner observes, the existence of fracture of the bones of the leg no doubt facilitated the reduction.

In one case (Mr. Turner's) the bone was removed, the dislocation having been compound, complete, and irreducible; and in four (Messrs. Phillips, Lizars, and Liston's) reduction being impossible, the bone was left in its new situation; in all these cases the patients did well, and had ultimately a useful limb, without death of the bone, suppuration, ulceration, or sloughing of the integuments. In the case read that night the result was different, as had appeared from the report of the case. This, then, was the first case of dislocation of the astragalus backwards, in which the soft parts inflamed, and sloughed, and exposed the bone.

It is stated in Mr. Taylor's notes of the case that the attempts made to reduce the bone were not forcible, and were soon abandoned. The reasons for not making much or persevering effort at reduction were (Professor Williams said), that as soon as he had satisfied himself the bone had sustained about a quarter of a complete revolution inwards on its antero-posterior axis, so that its superior articulating surface looked directly inwards, he saw little prospect of effecting reduction, inasmuch as that deviation could scarcely be rectified by any force that could have been brought to bear on the bone, and even if it were practicable, it could not have been effected without inflicting an unjustifiable amount of injury upon the soft parts covering the bone; the attempt at reduction, therefore, was very slight; so slight, indeed, as scarcely to be called an attempt at reduction; and, moreover, the history of previous cases of the accident tended to show that, on the one hand, there was little hope of replacing the astragalus when the tibia and fibula were unbroken; and on the other, that in every instance in which the bone had been left in its new situation the issue had been satisfactory. As to the rotation of the astragalus, that bone could be rotated either on its antero-posterior or transverse axis. In Mr. Liston's case,

the astragalus was rotated in the latter direction; and in Mr. Turner's it was rotated on its antero-posterior axis outwards, the superior articular surface presenting at the wound. In the case just read the bone had made a quarter of a revolution inwards, Professor Williams said he need not dwell upon the mechanism of the various rotations of the astragalus in dislocation; for whether the rotation was partial or complete; whether on the antero-posterior or transverse axis; or whether it occurred in dislocation forwards or backwards, the mode of its production was analogous in each case, and explicable on the same principles. But the recognition of the existence of such a rotation was of great importance, for when it existed to any considerable extent, Professor Williams thought it rendered reduction impracticable. Now, the outlines of the astragalus are so well marked and recognizable by the touch, and the exact position was so clearly made out in this case of dislocation *backwards*, where the soft parts covering the displaced bone are so much thicker than in dislocation forwards, that he could not but think the existence and extent or non-existence of rotation could be easily determined, at all events in most cases, and especially in anterior dislocation; and thus an important guide as to the treatment to be adopted would be obtained.

The notes of the case were illustrated by a cast, showing the appearance of the ankle and foot on the day the patient was admitted into the hospital, and by a drawing and coloured wax cast, taken by Dr. Alexander Carte, exhibiting the position of the astragalus, and the condition of the surrounding soft parts on the day the astragalus was removed.

Mr. Tufnell has in his possession a cast of the patient's foot in his present condition, and perhaps he would have no objection to exhibit it to the Society at their next meeting.

Mr. TUFNELL.—I will be able, I think, to do more than that, for I may possibly be able to bring you the man himself. I met him the other day in the street when he was walking with one stick; he formerly required two, but latterly he said he had discontinued one of them. I asked him whether he was able to carry a basket on his arm and to work for his living, and he replied that he could not. Now, this is an important point to bear in mind.

Professor WILLIAMS.—I have no hesitation in saying that on several points the treatment of dislocation of the astragalus requires to be very carefully reviewed, and that Mr. Turner's statistics are not sufficiently extended to justify some of the conclusions which have been drawn from them. I have myself collected a considerable number of cases in addition to those that are reported in his work, and I hope on some future occasion to bring the subject at greater length under the notice of this Society. As regards the question of leaving the bone *in situ*, this much is to be said, that if authority deserves to have any weight, we have in favour of doing so the great names of Sir Astley Cooper, Dupuytren, and other surgeons of eminence, who did so even in some cases where it was anticipated that the parts covering the bone were likely to slough. —*Dublin Medical Press*, April 4, 1855.

OPHTHALMOLOGY.

58. *Protrusion of the Eyeballs, with Enlargement of the Thyroid Gland and Anæmic Palpitation.* By JAMES BEGBIE, M. D.—J. K., aged 32, by occupation a gentleman's servant, many years ago had a fall from horseback, by which he sustained a severe wound on the occiput, from which a profuse and continued hemorrhage took place. He has never been quite well since that occurrence. In the beginning of 1845, he suffered from bilious fever, and made a slow and imperfect recovery. In August, 1851, had an attack of jaundice, which continued more or less for a whole year; and during its progress the symptoms which first attracted attention in connection with the present history developed themselves. For more than a twelvemonth he has been subject to palpitation, breathlessness,

and giddiness. These symptoms were soon followed by enlargement of the thyroid gland, and by increased prominence and distension of the eyeballs, so as to give him a remarkable appearance of staring, which was noticed by all his friends.

In the spring of 1853, he first came under my observation. He was a man of middle size and well formed. His countenance was pallid and sickly; but under excitement, either mental or bodily, his face quickly flushed, and his manner became nervous and embarrassed. At all times, but especially under excitement, the action of the heart was forcible and rapid, and this action was communicated to the vessels of the neck and head. The eyeballs were enlarged and prominent, presenting the appearance of great distension. The thyroid gland was also much enlarged throughout its whole body, varying in size according to the force of the heart's action. This enlargement was accompanied by a strong pulsation over the tumour, and in the vessels of the neck; and a peculiar thrill was felt, and a loud murmur heard, over the whole extent of the gland. The action of the heart was violent and jerking; and a loud bellows murmur attended the first sound, and was heard most distinctly over the region of the aortic valves. In a state of quiet and rest these symptoms became moderated, and the patient was able to continue his domestic service, in the enjoyment of comparative health. He was directed to take persistently for months the milder preparations of iron, and to use a diet chiefly of animal food. Under this plan he improved in health, and all his more urgent symptoms subsided by degrees. In the autumn of 1853 he went to England, and continued, I understand, to improve in health; but I lost sight of him at this time. Early in the spring of 1854, he had engaged to accompany a distinguished officer to the Crimea, as his body-servant. Before the time arrived, however, when he was to have entered on his duties, he was, after exposure to cold and fatigue, seized with inflammation of the chest, and obliged to relinquish the undertaking. His illness was severe and continued, and he never recovered from it. It appears to have aggravated greatly the peculiar symptoms under which he had so long laboured, and complicated the aspect of his case. He was able to return to Scotland, however, and in March, 1854, he again, after an interval of several months, came under my notice. At this time, he had enlargement of the liver, with jaundice, and the signs of organic disease of the heart, accompanied with general dropsy. His eyes were still prominent, and the thyroid gland enlarged, but neither of these now maintained the striking character which they possessed previously to his leaving Scotland. No remedy was of any avail, and he sank, on the 28th of March, worn out with the sufferings of complicated disease in the thorax and abdomen. The body was opened on the 30th by Mr. Johnston, in presence of Drs. W. T. Gairdner and Warburton Begbie, and myself.

Sectio Cadaveris.—March 20, 4 P. M. Body of a moderately stout and middle-sized man. The linens in which the body was shrouded were in many parts (as the neck, axilla, scrotum and legs) quite soaked with serous fluid exuded from the body. The cuticle was in many parts loose and easily detached from the *cutis vera*, and in every organ of the body examined, signs of decomposition were generally met with—frequently so marked as to obscure the proper pathological conditions.

The subcutaneous tissue, and indeed the cellular tissue generally, contained very little fat, and was in every part more or less infiltrated with serum.

On opening the thorax, the *pericardium* was found of large size, and overlapped, at its sides only, by the margins of the lungs. It contained about six ounces of a transparent yellow-coloured fluid. Upon the anterior surface of the heart, near to its base, a "milky spot" was observed, about the size of a florin-piece, and another upon the opposite surface of the pericardium.

All the cavities of the heart were filled with dark-coloured blood in a more than usually fluid condition; one well-formed decolorized clot was found in the right ventricle. The heart was large (might have weighed 16 oz.), soft, and flaccid. All its chambers, but more especially the *ventricles*, were considerably dilated; the tricuspid orifice admitted four fingers, the mitral three. The tricuspid and mitral valves were large, but otherwise normal; the sigmoid valves were also normal. The *vena cava* inferior was unusually large; and the aorta was

small when compared with the size of the pulmonary artery. The endocardium and inner surface of the aorta were stained of a deep red colour.

Both *pleuræ* contained turbid fluid of a dark red colour, computed at about eight or ten ounces in each. The posterior surface of the upper lobe of the *right* lung was firmly adherent to the costal pleura by strong short bands of lymph. The lower lobe of this lung was crepitant, and infiltrated with bloody serum; the posterior part of the upper lobe was condensed, non-crepitant, and friable, as if hepatized; but the advanced state of decomposition in which it was, prevented a decided opinion being formed concerning it. The left lung was crepitant, with the exception of its posterior part; and from the surface of a section a considerable quantity of bloody serosity was readily expressed.

The sterno-hyoid and sterno-thyroid muscles were much thinner and broader than natural, from being stretched over the thyroid body, which were of large size. The external jugular vein was normal; the internal jugulars were large—the left one, when slit open, measured an inch and a half across at a level with the cricoid cartilage. The thyroid body was of large size, but was not weighed; its weight may, however, be computed at being four or five times greater than natural. Each lateral lobe measured an inch and a half in breadth, and was of a corresponding thickness. This great increase in size was not partial but general, and although the *isthmus* was comparatively larger than the lateral lobes, there was complete symmetry of both sides. It was of a dusky-red colour, smooth, and well-defined, and slightly irregular on its anterior surface, but still retained the natural convex and semi-lunar form of the organ when in a state of health.

The peritoneum contained about a pint and a half, or two pints, of a clear fluid, tinged of a bright yellow. The *spleen* was enlarged in all its diameters, and was computed to weigh about twenty ounces. It was of very firm consistence, and on section presented the trabeculæ well-marked, and also the Malpighian bodies, which were of an opaque yellowish-white appearance. The *kidneys* were both very large and very soft. The cortical was to the medullary substance, relatively, increased in amount, and the great size of both organs seemed to arise from this circumstance; otherwise their actual pathological condition could not be ascertained on account of the advanced state of decomposition which they were in. The *liver* was certainly not enlarged, perhaps rather small; its surface was somewhat irregular, slightly and superficially fissured at points; no rounded nodules, however (as of cirrhosis), could be observed. On section, the tissue was (considering the decomposed state of all the organs) rather hard and dense, and seemed partially atrophied; its colour was deep-orange, and in some places there was an approach to "nutmeg" congestion.

This case presents a well-marked example of the disease first described by Dr. Graves, of Dublin, and afterwards noticed by Sir Henry Marsh, Dr. Stokes, and other Irish physicians; and whose true pathological character was, I believe, first pointed out by me in a paper read to the Medico-Chirurgical Society of Edinburgh, in January, 1849, and subsequently published in the *Monthly Journal of Medical Science*. The affection has since been illustrated by Romberg and Hensch, and other German physicians, and has attracted the notice of some of our best writers on diseases of the eye. The history is interesting, as having occurred in a male, the cases on record, with few exceptions, having been seen in females. It is more particularly interesting as affording an opportunity of examining the morbid appearances after death, the only record of which that has yet appeared being that communicated to the Pathological Society of Dublin by Sir H. Marsh, and by Basedow in Germany. In the case now related, as well as in that of a lady who had long laboured under this peculiar affection, and in whom it proved fatal also, by supervening pneumonia (the only instances which have occurred to myself of instituting *post-mortem* examinations), there exists a remarkable similarity in the chief morbid appearances with those described by Sir H. Marsh. These appearances chiefly consist in the very fluid state of the blood found in the heart and great vessels, in the dilatation of the cavities of the heart, and of the venous trunks, in the enlargement of the spleen and disease of the liver, and in serous effusion into the different cavities, the result of vascular obstruction.

Since the publication of the three cases related by me in 1849, I have, through the kindness of my professional brethren, seen many additional examples of this affection, a large proportion of which have gradually undergone a cure, while the remainder have benefited, or are now benefiting, by the use of iron, animal food, and fresh air. It is of great consequence to impress those suffering from this affection with the belief of its curable nature, and to urge upon them the persistent employment of the means of restoring the red particles of the impoverished blood, and improving the general health; for we have now examples before us to show that the neglect of these rules must lead, from functional disorder of the heart, to dilatation of its cavities, and to the usual train of consequences resulting from such a morbid change.

The more extended our experience of the phenomena constituting this peculiar affection becomes, the more convinced shall we be that the point of departure from health is not in the heart itself, but in the impoverished condition of the blood, which, after a time, affects the heart and vessels functionally, and, by long continuance, involves them ultimately in fatal organic change.—*Edinburgh Medical and Surgical Journal*, April, 1855.

59. *Affection of the Heart, Thyroid Gland, and Eyeballs.* By Drs. ROMBERG and HENOCH, of Berlin.—This disease, to which attention has been drawn by Marsh, Begbie, Cooper, &c., in Great Britain, seems also to be well known in Germany, and many examples of it have been observed by Pauli, Brueck, Basedow, and lastly by the authors whose interesting paper we have now before us—Romberg and Henoch. Though differing in regard to the etiology of the disease as a whole, and disagreeing to a certain extent in the account given of the rise and occasion of its individual symptoms; still, in the descriptions of all the writers now named, there exists so remarkable a uniformity, as to satisfy us of the identity of the disease which each has observed.

We shall, in the first place, make our readers acquainted with some of the cases in an abridged form, and the remarks of Romberg and Henoch, and then add a few observations of our own, which the perusal of the former have called forth.

Case 1.—A. S., aged fourteen, who had never menstruated, was treated, in the clinical ward, for anæmia, and cured by a preparation of iron. In October, 1849, she again became a patient, her former disease having returned. At that time, the extraordinary paleness of her skin revealed her anæmic condition. The right lobe of the thyroid was swollen, and the jugular vessels were seen pulsating. The anæmic sound was clearly audible in the neck. There was the evidence of an enlargement of the heart, and its first sound was at the base accompanied by a bellows murmur. The patient suffered from dyspnœa, increased by motion and from great weariness. The bowels were irregular. On the 12th November she was ordered to take iron, which, with a short interruption, she continued to do till January, 1850. At that date, a decided improvement in her whole system was visible.

Case 2.—A girl of eighteen, who had first menstruated a year previously, began to complain of violent palpitation and uneasiness in the region of the heart, brought on chiefly by exertion, especially in ascending stairs. At the same time, a swelling had appeared in the front of the neck, and at times she expectorated blood. The diagnosis, after examination of the heart, was that something more than functional disorder existed, and that valvular disease was present. In this patient, as in the former one, the enlargement of the thyroid and the affection of the heart existed, but the eyes were not implicated. She differed from the former case in not presenting an anæmic appearance.

Case 3.—Mrs. R., aged forty-seven, subject for many years to numerous hysterical complaints, was admitted into the Clinic May 18, 1849. She complained especially of violent palpitation of the heart, and consequent agitation. During the attacks of palpitation she experienced a feeling of tightness in the throat, and a glimmering before the eyes. The thyroid was evidently enlarged, particularly in its right lobe, and in it she experienced a sense of pulsation and of pain during the occurrence of the palpitation. Then, also, the eyes became unusually large, and appeared starting from their sockets in such a manner as

to expose her to the laughter of bystanders. The catamenia were regular, but scanty. The pulsations of the heart were increased to 100 in the minute, but otherwise there was no change detected. She was hysterical, suffered from weariness and from irritability of temper, with inclination to weep. Although the symptoms had existed for nearly two years, they had considerably increased during the previous four months. In this patient the manifestation of anæmia was very clear. A mixture of digitalis, with phosphoric acid and valerian, were the remedial means employed, and amendment was so speedy that in July of the same year she was able to take a situation as lady's maid, and went to Dobberan, where she experienced great benefit from the use of the sea baths. In March, 1850, she was seen entirely freed from her former ailments.

Case 4.—C. L., aged twenty, first menstruated at the age of thirteen, and suffered thereafter from complete amenorrhœa for a whole year. During this period she had a trifling swelling on the front of the neck. Menstruation again returned, but very irregularly, and the girl, from being blooming and robust, became pale and weak; the thyroid swelling increased; palpitation of the heart succeeded, and then followed a strange largeness of the eyes. Considerable benefit was obtained in this case from the steady use of iron.

Case 5.—Mrs. B., aged twenty-five, presented herself at the Clinic on July 7, 1848. Always healthy, and having regularly menstruated. She had for three weeks been occasionally exposed to a draught, when washing, with her neck uncovered. She had felt pain in the front of the neck, and had latterly noticed a slight swelling in the situation of the thyroid. On examination at the Clinic, the same remarkable prominence of the eyes as noticed in the other cases was observed, still the sight was not affected. Violent action of the heart existed; and in the thyroidal tumour, now greatly increased, and very large, as also in the head, the pulsation was inordinate. The pulse was 144. Three cups of blood were taken, and in the following week leeches were applied to the enlarged thyroid; from these measures an evident, though temporary, amelioration followed. Afterwards (early in 1850), from the use of digitalis, and due in great measure to the regular return of the menses, which had been much interrupted, restoration to sound health occurred.

Case 6.—A. B., a young girl, aged seventeen, had suffered from violent palpitations for two years; had also an attack of typhus fever, and had been neglected, owing to living in a country village. Was admitted into the Clinic November, 1847, when her appearance was most striking. The eyes were protruded; the sight, however, unaffected. The thyroid was greatly enlarged, and appeared throbbing; a loud systolic murmur was heard, and peculiar thrill felt over it. In this case, the occurrence of the palpitation was evidently followed by increase in the size of both thyroid and eyes. Pulse equal and regular—116. Bloodletting was adopted three times in the treatment of this case, and after a little time a leech was applied every fourth day to the enlarged thyroid; as in the former case, a temporary benefit resulted. The girl appears to have ultimately fallen a victim to tubercular disease in the chest, surviving the time of her first illness for nearly three years.

Taking the cases which have occurred in their own experience, and those which have been elsewhere recorded, there are in all twenty-seven which form the subject of the following interesting remarks by Drs. Romberg and Henoch. They acknowledge twenty-seven to be too small a number of observations to warrant any very decided opinions being drawn from them, but, at the same time, believe it sufficiently large to afford the groundwork for much useful study. By far the larger number of the patients were females, only four of the twenty-seven were males. All with one exception were young, the most common age being between twenty and thirty. In the larger number of the cases there existed the combination of the three symptoms of palpitation of the heart—enlargement of the thyroid and prominence of the eyes; while in six of the twenty-seven cases one or other of the three was absent. In the two cases first treated by Dr. Romberg in the Clinic, the prominence of the eyes was not observable. Undoubtedly of these phenomena the palpitation of the heart is the one best understood, and corresponds most readily with the view taken of the

whole disease. Almost always the cardiac symptoms are those first discovered and first complained of; then, after a longer or shorter period, the swelling in the neck commences, and the prominence of the eyes follows. Only a few of the recorded cases lead to the supposition that the three diseased appearances arose at about the same time, certainly in the fifth case treated in the Clinic the enlarged thyroid was the first symptom noticed. It is well to inquire wherein the original affection of the heart consists. That in some instances there exists organic disease is proved as well by examination during life as by *post-mortem* examination (Basedow and Marsh); but, again, in others it is equally certain that the cardiac symptoms depend merely on an increased irritability of the organ (Cooper, Begbie, Lubarsch). Cases explicable on both these grounds, and on these only, have been treated by the Clinic. In regard to the thyroïdal swelling, it is interesting to note its increase and subsidence after the violence of the cardiac palpitation—this fact is expressly stated by Sir Henry Marsh, and mentioned by Begbie as existing in his third case, and also noticeable at times in the fourth; this, of course, points to an intimate relation between the two symptoms. Marsh and Heusinger describe the condition of the thyroid as a true hypertrophy, and it has been noticed by Graves that, after the lapse of years, the consistence of the gland has been much increased.

As regards the remarkable prominence of the eyes, this symptom comes on gradually, and so far as vision is concerned it is not of much importance, seeing that only in one case (Lubarsch) was it at all seriously impaired; but, though sight is not much affected, the prominence of the eyes produces a singular disfigurement, causing the sufferer to be not unfrequently avoided in company. Acknowledging the great difficulty of determining upon what peculiar condition the prominence of the eyes depends, our authors set aside the view of the increase of the aqueous humour causing a true enlargement of the eye (Begbie—the theory also adopted by Dr. Stokes), also that which attempts a solution by reference to an hypertrophy of the post-ocular cellular tissue (Basedow). They look upon the idea of the prominence being due to a want of tone in the ocular muscles, and an accompanying congestion in the posterior parts of the eye (Cooper, Dalrymple) as more likely; but they appear to think still more favourably of the view of Heusinger, who found in two cases an extraordinary accumulation of fat in the cellular tissue behind the eyes, and regards it as the probable cause of the exophthalmos.

Again, regarding the disease as a whole, our authors proceed to remark that certainly the larger number of the individuals so affected exhibited evident symptoms of anæmia, such as a remarkable paleness of the skin, the peculiar sound audible in the bloodvessels of the neck; headaches often very violent; giddiness, especially when in the upright posture; humming sound in the ears; attacks of fainting; small frequent pulse, &c. Irregularity of the catamenia also is commonly present, while fluor albus, and sometimes complete amenorrhœa, have been found. Symptoms of an hysterical nature further distinguished not a few of the cases, the globus hystericus, neuralgic pains in different parts, coldness of the extremities, and strange wanderings of the mind. Basedow describes a remarkable calmness and a great desire for pleasure as characteristic features of the mental condition. In some of the cases it is clear that if the disease was not originated, at all events it was furthered by the occurrence of a severe hemorrhage or flux, which reduced the system (Begbie); also a depressed state of both body and mind seemed connected with its first occurrence (Graves). But though anæmia was present in a large number of the cases, there are others whose commencement could not be traced to it, and anæmia cannot therefore be regarded as an essential requisite towards the explanation of the complex phenomena. The irregularity in the uterine system, too, cannot be regarded as altogether explanatory of the disease; for, independently of males being subject to it, these uterine derangements, though marked, were of very varying nature. Heusinger directs attention to the condition of the spleen, which he found after death much increased in volume and manifestly diseased.—*Edinburgh Med. and Surg. Journ.*, April, 1855, from *Klinische Wahrnehmungen und Beobachtungen*.

60. *Change of Colour of the Iris independent of Inflammation of its Texture.*—It is a familiar fact, that one of the ordinary results of inflammation of the iris is a conspicuous change both in its colour and texture. But it is not so generally known. Dr. ROBT. TAYLOR states (*Med. Times and Gaz.*, March 17, 1855) that “the colour may be completely changed, *without any indication of diseased action in its texture*,” and he gives the following cases as examples of this.

Case 1.—The first is that of a lady, who received an accidental blow on the right side of the forehead, nearly twelve months ago, in consequence of which the sight of the corresponding eye failed slowly and painlessly, until, when she consulted me a short time since, she was unable to read the largest print. There was a deep-seated, mottled opacity, situated, apparently, in the posterior capsule of the lens. The veins from the interior of the globe were enlarged and tortuous, and the point of exit of one or two of them was tinged by a slight deposit of pigment; but this increased vascularity was equally conspicuous in the other eye, the vision of which was unimpaired. About six weeks ago, or ten months after the accident, she observed the first indications of change in the colour of the iris. It had formerly, like that of the left eye, been of a dark brown colour; it is now of a bright blue-gray, without the slightest admixture of brown, and presents a striking contrast to the dark hue of the other. The change was completed in four weeks.

Case 2.—The patient, a woman, 29 years of age, is still under my care at the Central London Ophthalmic Hospital, for deep-seated disease of the right eye, identical, I believe, with the affection described by Dr. Mackenzie in the last edition of his work on Diseases of the Eye, as *Retinitis Lactantium*. When she commenced her attendance at the Hospital, vision was so far destroyed that she could scarcely distinguish between light and darkness; but it has since been completely restored.

Five weeks ago, the change in the appearance of the iris was first observed by her friends. Formerly, like that of the left eye, it was of a deep brown colour, mottled by a few minute specks of gray; within the time specified, these specks have gradually enlarged, until their conjoined area now occupies nearly one-half of the iris, to the exclusion of the original dark hue. The change is still in progress.

Case 3.—The third instance was observed in a man, who presented himself, on one occasion only, at the hospital, on account of capsulo-lenticular cataract of the right eye. I was at once struck with the difference in colour of the irides. That of the left eye was dark hazel, the gray being in minute specks, and in very small proportion to the brown; that of the right, in which the disease existed, was bright blue-gray, with only a very few minute brown dots. He was very deficient in intelligence, and could not give any clear account of the length of time the difference had existed; but it was too striking to admit of the possibility of its having been congenital, or even of very long continuance; as in such a case he could not have failed to have heard it constantly remarked upon.

In each of these instances the surface of the iris retained its brilliancy; the superficial fibres were as sharply and clearly defined as in the unaffected eye; the pupil was free from adhesions, and dilated actively and fully under the influence of atropine. Had the progress of the change not been observed, at least in the first two cases, it would at once have been concluded that the difference in colour was congenital.

My colleague, Mr. Haynes Walton, informs me that he has witnessed one example of a similar nature in connection with capsulo-lenticular cataract, and another in which a gray iris was permanently stained with rust-coloured spots. In the latter instance, however, there is reason to believe that the eye had suffered from syphilitic iritis.

[The change of colour of the iris in these cases must have certainly resulted from some derangement of the nutritive functions of the part, and though this may not have been of an inflammatory character, it certainly was an abnormal action, it can scarcely be said not to have been a “diseased” one.]

61. *Diphtheritic Conjunctivitis, and the application of Caustic in acute Inflammation.* By DR. A. VON GRAEFE.—The occurrence of fibrinous exudations upon the conjunctiva has been long known. They differ from layers of coherent mucus in being more adherent, elastic, apt to coil up, and capable of splitting into fibres. Under the microscope they are seen to consist of coagulated fibrin; the latter are composed of a convolution of young cells.

The exudation first appears as a jelly-like coagulated substance. In the latter stages, it disappears before the increasing number of pus corpuscles. If in a case of acute conjunctivitis the upper lid be everted for a few minutes, the mucous membrane exposed to the air will be forthwith covered by such a layer. In very young infants, the blood is not sufficiently formed that it may yield a firm layer of fibrin; at least, such an occurrence is the exception.

The characteristics of this form of disease depend less on the exudation of fibrin than on the condition of the mucous membrane itself. In blennorrhagic inflammation, the conjunctiva is looser, succulent, and infiltrated by a fluid exudation. In diphtheritic inflammation it is stiff, and filled with a firm substance. The eyelid is therefore prominent and immovable, instead of being only swollen and soft. In blennorrhœa the conjunctiva is very vascular, and the circulation is so far free that the greater number of the vessels contain fluid noncoagulated blood. The diphtheritic conjunctiva is, in consequence of a high degree of stasis of the blood, but sparingly supplied with the circulating fluid. The blennorrhagic secretion consists of yellow-coloured homogeneous pus; that of diphtheritis is brownish gray and semi-transparent. In this latter disease there is a great sensation of heat experienced by the patient.

The symptoms occurs as follow: In an eye previously sound, there suddenly comes on swelling of the upper lid, attended by an increased flow of tears, and by a marked sense of heat. The integument of the lid loses its folds, and becomes smooth and glistening. There is also incipient chemosis; but the conjunctiva is not very red; the network of vessels forming large meshes; the membrane between them being of yellow colour, studded with red points. The upper lid is remarkably stiff; its eversion is attended with both pain and difficulty. The appearance of the conjunctiva palpebrarum would not strike the inexperienced observer; but the smooth yellow surface is not in truth the membrane; it is a thick layer of fibrin, which arrests the circulation in the part, and threatens destruction to the eye.

After a time the lids lose their stiffness; the conjunctiva becomes more spongy and looser; the fibrin separates; the vessels fill, and pour forth discharge; the chemosis loses its yellow colour, and the peculiarities of the diphtheritis are lost. The duration of the different stages varies extremely. The author has seen the stage of fibrinous exudation last from six to ten days; that of succulence and spongy alteration of the mucous membrane coming on afterwards. But this latter may be complicated by relapses of the exudative inflammation.

The dangers of diphtheritic inflammation are those which refer to the cornea. A part becomes turbid, and loses its epithelium; the colour changes to a muddy yellow; a sore or ulcer forms, which becomes deeper and deeper; while upon its surface, by the aid of a magnifying glass, numerous points are seen, which indicate molecular necrosis.

It is mostly an epidemic, may proceed from a variety of causes, and is decidedly contagious. The prognosis is worse in adults than in children. Of 40 young patients suffering from this disease, the eye was lost in nine cases. Three had leucoma, with adherent iris; 21 recovered without any affection of the cornea; 7 had slight corneal affections at later periods.

As regards treatment, the author recommends abstraction of blood, the application of ice, and the administration of mercury. He, however, attaches especial importance to the application of caustic. He affirms that this method of treatment is not only permissible, but is indicated in all forms, and from the commencement; that it controls the worst and most dangerous changes; that ulcerations of the cornea do not contra-indicate its use. For the acute forms he uses a solution of ten grains nitrate silver to the ounce of water, or a solid stick of nitrate of silver and nitrate of potash fused together. The caustic

must be applied to all the diseased surface, and must be proportioned to the severity of the attack.—*Med. Times and Gaz.*, 17th Feb., 1855, from *Archiv für Ophthalmologie*.

62. *Trephining the Os Unguis for Fistula Lachrymalis*.—M. DEMARQUAY, of Paris, has revived this operation, which he claims as *new*, and takes credit to himself for devising it. What is more remarkable, the English Journals give it as a novelty without comment, and thus sanction the claim.

The operation was performed by Mr. Pott, who used a trocar for the purpose, and by Mr. Hunter, who had constructed an instrument like a shoe-maker's punch, by which a circular piece of bone could be cut out completely. The late Dr. Physick used to mention the operation in his lectures, and we are under the impression that he performed it in a few cases. Like most French discoveries in ophthalmic surgery, it is an old practice long since abandoned.

MIDWIFERY.

63. *Case of Presentation of the Bladder in Labour*. By ALEX. HARVEY, M. D.—Mrs. M——, æt. 28, was taken in labour of her first child on the evening of Tuesday, the 4th July last, some weeks before the expected time of her delivery. She sent for me the same evening, when I found her complaining of pains in the lower part of the abdomen, both behind and in front, and likewise round the hips. She had an opiate given her, which had the effect of relieving, indeed, of removing the pains, and procuring for her a comfortable night's rest.

The pains returned next morning. On examining her per vaginam, the os uteri was found slightly dilated. In the evening the pains had become more decided and regular, and the os uteri more fully dilated. No bag of waters could be discovered, and the patient was not aware of its having broken. The presentation was ascertained negatively. It was not the head, but its real nature remained doubtful.

At 3 A. M. of the following day, the os uteri was pretty fully dilated, and the pains were good. The presentation, however, was still uncertain. It seemed impossible to doubt that what had all the characters of feet could be aught else; but the limbs connected with them, besides being themselves extremely small and tiny, ended in what seemed certainly not a breech, and very exactly resembled the shoulder. No anus could be felt, nor organs of generation. The limbs were brought down, and proved to be the feet and lower extremities.

The pains continued vigorous, but the child descended very slowly till about 1 o'clock in the afternoon, after which the labour advanced steadily, although still very slowly, till about 3 o'clock, when the entire fœtus was expelled by one strong pain.

Altogether, the labour occupied above thirty-six hours.

For some time before the delivery of the woman, and after the full dilatation of the os uteri, the presentation was as follows: The front of the child was to the spine of the mother; the trochanters and head of the thigh bones, which were in connection with a bony surface of very limited extent, and (as before observed) very closely resembling the top of the shoulder, lay behind the pubis. Filling the hollow of the sacrum was the front of the abdomen, lying remarkably low down and pressing on the perineum, occupying besides, to a great extent, the cavity of the pelvis.

At the moment of delivery, and for some time previous to this, this abdominal mass, in the form and shape of a large round swelling, which was tense, elastic, and unyielding, filled up the whole outlet; the breech, or what appeared to be the breech, being pushed upwards behind the pubis. The mass in question passed through with extreme difficulty, and with corresponding anguish to the mother.

The child was stillborn. On examining it, it was found that there were no buttocks, and that the bony pelvis was but imperfectly developed, small and insignificant. There was no anus, nor any trace of one. A projecting fold of skin of a livid colour occupied the site of the scrotum or labia, and had a shallow imperforate fissure in the centre of it.

The abdomen (speaking comparatively) was of enormous size, and was distended by what was manifestly fluid in its interior. It encroached upwards on the chest, which was preternaturally short and narrow. The child's neck was apparently wanting, and the head was small, elongated, and livid, but not otherwise abnormal.

The general surface presented marks of incipient decomposition.

On laying open the abdomen, the urinary bladder was found occupying the greater part of its cavity. It formed a round or oval tumour, considerably larger than the average size of the foetal head. Unfortunately, it was accidentally punctured, and its contents escaping, neither the real nature nor the exact amount of the fluid it contained could be ascertained; but it had the colour and odour of urine; and, judging as well from the apparent capacity of the bladder as from what was seen to escape, the fluid might have amounted to about a pint and a half.

The bladder had no outlet, but the ureters entered it in the usual way, and were pervious throughout. Moreover, the rectum also opened into it, but no indications of the presence of meconium were discovered in the bladder. The condition of the generative organs was not minutely inquired into; but, on a cursory examination, none could be seen, and the sex of the infant was not made out.

The striking feature in this case was the extreme difficulty with which the delivery was accomplished, a circumstance now manifestly referable to the unyielding nature of the fluid in the urinary bladder, which was actually the presenting part, and which, from the action of the uterus upon it, had to make its way by its broadest possible diameter.—*Edinburgh Med. and Surg. Journ.*, April, 1855.

64. *Abnormal Quantity of Liquor Amnii*.—Dr. JAMES A. SIDNEY related the following examples of this at a recent meeting of the Edinburgh Obstetrical Society:—

Case 1.—Mrs. B., Stevenlaw's Close, pregnant of her third child, when I saw her in December, 1852, with Dr. Aiken. She said she was six months gone; at that time she was as large as other women at full time. The os uteri was open to about the size of a shilling; the foetal pulse could not be heard, although examined several times. On the 1st January, 1853, I was sent for, and found her in labour; the abdomen was extremely large, but not pendulous, and the parietes so thin that distinct fluctuation could be felt. I left her, and was again sent for on the morning of the 2d, about 5 o'clock, when I found the os uteri nearly fully dilated, and the pains very rapid and strong, but not the slightest effect produced on the bag of waters, which was tightly stretched across the os. Believing that there was superabundance of liquor amnii, and over distension of uterus, I gave her a dose of ergot, had her brought to the edge of the bed, and ruptured the membranes with a quill, when about *four gallons and a half* of liquor amnii were caught in basins, and a great quantity besides flowed on the bed and over the floor. The child, evidently about the seventh month, was soon born, but dead; had been so for some days; placenta came away of itself. Uterus contracted well, and no hemorrhage followed. Woman made a good recovery.

Case 2.—Mrs. McK., Richmond Street, abdomen pendulous, pregnant of third child, at full time. Said she was much larger than on previous occasions, and thought she would have twins.

Nov. 19, 1854, 10 A. M. Labour was tedious during the first stage, from indurated os from ulcerations; but got great benefit from venes. and tart. ant. and a bandage. The os uteri at length was fully dilated, but after that the pains made no progress; membranes were never pressed down. I had her brought to the edge of the bed, and caught in basins *three gallons* of liquor amnii, a

good quantity flowing on the bed and floor; the child was born about two hours after; woman and child did well.—*Monthly Journ. Med. Sci.*, April, 1855.

65. *Cases of Placenta Prævia*.—The following cases of placenta prævia were communicated to the East Kent and Canterbury Medical Society, during the sessions 1852–55.

Mr. F. F. GIRAUD was summoned about a month before the full term of pregnancy to attend the wife of a tradesman in Faversham, who had already given birth to nine children, on account of slight hemorrhage having occurred from the womb during the act of relieving the bowels. He could not determine by examination whether the placenta was over the os uteri, from that part being too much closed, and too far back to admit the finger. As she was habitually costive, and had been straining at the water-closet when the hemorrhage came on, he thought it possible that the blood might have escaped from a congested state of the vessels within the vagina; and, after having emptied the bowels with castor oil, he desired her to be more attentive to the state of her bowels. No more hemorrhage ensued until the 28th February, exactly a month from its first occurrence. The patient then again perceived a discharge of blood on relieving the bowels; on looking into the night-stool, he found a small coagulum, together with a very offensive motion. She felt slightly faint, which Mr. Giraud attributed to alarm more than to the loss of blood; the pulse being firm, and no hemorrhage continuing. There were no indications of approaching labour, and the os uteri would scarcely admit the end of the finger, and not sufficiently to ascertain the state of the parts within. Under these circumstances, he gave the patient a little castor oil, and directed the nurse, if the slightest return of bleeding should take place, to send to him immediately, as it might be necessary to have recourse to artificial delivery. In about four hours, an alarming message was sent; and in a few minutes Mr. Giraud was at the bedside of the patient, where he also found his partner, Mr. Garraway. It appeared that, on using the night-stool, an enormous quantity of blood had passed away; and her nurse, finding her very faint, had some difficulty in getting her into the bed. He immediately passed his hand into the vagina, and had great difficulty in overcoming the resistance of the os uteri. The placenta was completely over the mouth of the womb; but in his eagerness to accomplish the delivery, he was not aware whether his hand passed through any part of it or by its side. The pressure of the fingers easily ruptured the membranes; Mr. Giraud obtained firm hold of the feet of the child, and extracted it without any material difficulty, in about a quarter of an hour from the first introduction of the hand into the vagina. During this process, he was not aware of any hemorrhage going on; but the patient became very faint as soon as it was over, the pulse small and feeble, her breathing short, and, in spite of brandy, ether, and ammonia, she expired within half an hour of the birth of her child.

Remarks.—The unusual circumstances attending this case were these: 1. The hemorrhage occurred each time during the action of the bowels, ceasing altogether when that effort was over. 2. The second attack of bleeding at the full period of gestation was so profuse as to sink the patient irrevocably, although no time was lost in the delivery of the child, and the first bleeding on the same day was of small amount. 3. In the sixty-two cases of placental presentation, recorded by Dr. Robert Lee, in his *Clinical Midwifery*, the bleeding, for the most part, came on in repeated gushes, and when recurring at the full period of gestation, each sudden and considerable escape of blood was more or less followed by continuous hemorrhage of less moment, but which was not the case in this instance. This was only the fourth case of placenta prævia which had happened in Mr. Giraud's practice during a period of twenty-seven years. The three former terminated favourably under the usual treatment, viz: introducing the hand and delivering the child.

Mr. RIDGEN reported four cases of placenta prævia which had occurred in his own practice. The first happened in January, 1842, in the mother of several children, who was advanced to the eighth month of her pregnancy. The vertex of the child presented and pressed upon the placenta, which was

thus expanded over it. An opening was made in the placenta, and the dead child was expelled through it. The mother recovered well. The second occurred in November, 1844; the mother was about six months and a half advanced in pregnancy; the child was delivered by turning, and was still-born. The mother rallied at the time, but died from puerperal peritonitis, six days after delivery. The third took place in March, 1846, and was also at the period of six months and a half. Delivery was effected by turning, and the child was stillborn. The mother recovered, but suffered from neuralgic pain in one leg for several months. In the fourth case, April, 1847, a stillborn child was delivered by turning, at six months and a half; the mother recovered.

Mr. REID related a case which had been under his care in October, 1848; the patient was the mother of eleven children. During the two months preceding the full period of pregnancy, there had been, at intervals, several considerable losses of blood. When first seen, the patient was prostrated by a profuse hemorrhage which had taken place suddenly. The vagina was immediately plugged, and stimulants were freely administered; after a time the hand was introduced, and the placenta, which was directly over the os uteri, was detached by the fingers, the os not being sufficiently dilated to admit the hand. After this, the prostration of strength became so great, although there was no further hemorrhage, that for three hours it was necessary to give the whole attention to the restoration of the patient's vital powers. During the whole of this period no contraction of the uterus was perceived; though at last, when the hand was introduced, the placenta was found projecting in the form of a cone through the os uteri; by gentle traction, which excited slight uterine pains, it was removed. After waiting some time, and apparently no effort being made by the uterus to expel the child, the head, which presented, was perforated, and the child was delivered. Uterine pains returned with the traction that was used, and the uterus contracted firmly after the child was withdrawn; the patient subsequently had a slow but good recovery. As far as one case could, this confirmed the rule which Dr. Murphy had established in considering the alternative of separating the placenta or turning, in cases of placenta prævia; that, in instances of extreme exhaustion, the plan of separating the placenta was not only advisable, but would be found a valuable mode of treatment. In this case there was the greatest probability that any forcible introduction of the hand, in order to turn, would have proved fatal.—*Assoc. Med. Journ.*, Feb. 9, 1855.

66. *State of the Fœtal Pulse as an Indication for Artificial Delivery.*—Professor SIMPSON made some remarks at a recent meeting of the Edinburgh Obstetrical Society on the indications afforded by the stethoscope for expediting delivery, and specially directed the attention of the Society to the fact that, while danger was usually indicated to the mother during labour by the increased rapidity of her pulse, the death of the child was most frequently threatened when the fetal pulse became slower and slower. It was known that in cases where, during labour, pressure was exercised upon the cord, the pulsations of the foetal heart became feebler, and were at length suspended by the continuous pressure. This was most probably the way in which the fœtus perished during severe and prolonged labours; the aeration of the blood by the placenta being imperfect, or entirely suspended. There were, however, cases in which danger was indicated to the child by the fetal pulse becoming much more rapid than ordinary, reaching 150 or 160 beats in the minute, and at the same time very irregular. Dr. S. believed the danger in these cases did not result from pressure on the umbilical cord, as in the cases where the pulsations became slower and slower, but from pressure, or some source of irritation acting on the brain.—*Monthly Journ. Med. Sci.*, April, 1855.

67. *Epidemic Puerperal Fever which recently prevailed in the Dublin Lying-in Hospital.*—Dr. MCCLINCKOCK read before the Dublin Obstetrical Society, March 2, 1855, the following interesting account of this epidemic:—

"The epidemic, whose history I am about to bring forward, unequivocally declared itself in the first week of last December [1854], and subsided in the

middle of February [1855]. Unlike the one of 1845, its outbreak cannot be said to have been either sudden or unexpected, inasmuch as twelve or fourteen cases of puerperal peritonitis and phlebitis, together with a few isolated examples of typhus and scarlatina, had occurred in the house during the preceding nine months.

"From the beginning of December to the 14th February, 182 women were confined in the hospital. This, I may just remark, is not half the average number of deliveries in the same period, and was owing to a stop having been put, in the latter part of December, and during all January, to the admission of patients, except such as were so near delivery that it would have been attended with imminent risk to send them away.

"Now, of these 182 women, 38—that is, 1 in every 5—were unequivocally affected with the symptoms of the disease; and out of these 38 so affected, 17 recovered, and 21 died, making the proportion of fatalities nearly 1 in 8 of all admitted; a frightful rate of mortality, and more than tenfold the average of this hospital.

"In three of the above cases, the puerperal disease was complicated with scarlatina. Two of these died, and the third made an excellent recovery, though the metritic attack was a marked one, and the scarlatina very severe, showing itself so early as the second day after delivery, and presenting in its course a truly formidable array of symptoms. On two occasions, this woman seemed to owe her preservation solely to the liberal exhibition of wine and brandy, and this too at the very time when we had every reason to fear the existence of uterine inflammation.

"It would be wearisome and tedious were I to give the individual history of all these cases, and yet I am quite at a loss how to classify or arrange them, not knowing what to take as the basis of any such classification, as they presented considerable variety in their symptoms, course, and morbid appearances. For example, in many cases, including some of the most malignant, there was no initiatory rigour whatsoever. Again, intense abdominal pain was a prominent feature of some cases from the onset to the termination; whilst in others, equally fatal, there was *no* complaint of the belly. Vomiting, likewise, was an early and constant attendant upon the disease in not a few instances, whilst in some it did not appear at all, or only at the close. And so on with the morbid appearances; some cases presenting intense peritonitis, others phlebitis, and a few putrescence of the uterus, and these either separately or conjointly. There were two features, however, common to them all, namely, a very rapid circulation, the pulse ranging from 120 to 140, and a marked adynamic type; so marked, indeed, that in two cases only did I feel justified in making trial of phlebotomy, and these, as you may suppose, were selected cases. Yet, in each of them, the supervention of syncope rendered it necessary to discontinue the bleeding before ten ounces of blood had been abstracted, one losing about seven, and the other nine fluidounces; and what is still more worthy of attention is the fact that in neither of these instances did the blood exhibit, after some hours' standing, any of the characters indicative of inflammation. Both these patients died.

"In nothing did the various cases differ so much as the manner in which the disease made its invasion. In the majority, a rigour announced its first onset, this being speedily followed by pain or uneasiness in the uterus; except in three or four instances, the pain was not by any means intolerable or severe at the commencement, or even for some hours afterwards. Tenderness of the uterus to pressure, however, with perceptible augmentation of its bulb, was almost invariably found to be present from an early period of each case.

"The first approaches of the disease, when not ushered in by rigour, were sometimes remarkably slow and insidious—the only deviations from normal convalescence being a trivial acceleration of the pulse and a slightly furred state of the tongue, with, perhaps, diminished secretion of milk. On two or three occasions the attack began apparently with after-pains, or at least with pains of an intermitting character, commencing almost immediately after delivery, and so equivocal in their nature that it was impossible to say when they ceased to be purely spasmodic and became inflammatory. Mr. Hey, of

Leeds, in his *Treatise on Puerperal Fever*, makes the remark 'that, during the epidemic season, lying-in women were unusually subject to after-pains, and those of a more violent kind than ordinary.' My recent experience is quite in accordance with this observation.

"The patient's own representation of her state we found could not always be relied on, owing to her unconsciousness of the presence or the progress of the malady. Frequently her statements on this head, though made with confidence and complacency, were yet so utterly at variance with the symptoms and actual condition of the patient, that the most inexperienced observer could scarcely have been deceived by them for one moment. Whether this apparent ignorance of her real state arose from an unwillingness to believe she was affected with illness, or formed part of the disorder, I cannot take upon me to say; but certain it is that no less than four or five of these poor creatures have assured me, in language of gratitude and self-satisfaction, that they felt perfectly well, and this too when their general symptoms plainly forbade all hope of recovery.

"This complete unconsciousness of danger, however remote, at a time when the hand of death was almost upon the patient, was a curious and distressing feature of the disease; and is the more remarkable from the fact that these women were apparently in full and perfect possession of their mental faculties. I have once or twice before observed the same in women dying of pure metro-phlebitis.

"Vomiting was not by any means a very prominent or constant symptom, except in the marked peritonitic cases; though in nearly all the fatal cases it came on some hours before death. Several of those who recovered had sickness of stomach, and a few of them even vomited large quantities of the dark-green tenacious fluid which has been aptly compared to green paint.

"Guided by the experience of this epidemic, I feel disposed to regard the state of the tongue as a more reliable prognostic than any other *single* symptom. With only one or two partial exceptions, I never saw a patient recover when the tongue had become dry, or brown, or glazed; I have observed this symptom before any of the others had assumed a mortal or even threatening character; nor was it absent in any of the fatal cases of the disease.

"At the outset of an attack the tongue was usually white, slightly furred, and somewhat less moist than natural. In many cases, this state of the organ has been the very first symptom to excite alarm, and to apprise us of the coming storm.

"As the disease made progress, the next unfavourable change observed in the state of the tongue was a dry, brownish streak down its centre, and more remarkable towards the base. This condition gradually extended until the entire dorsal surface of the organ was involved.

"I think I am justified in asserting that the prevailing character of the tongue in the late epidemic was a close approximation to what is usually called the 'typhoid tongue,' and this is one symptom wherein it differed from the epidemic of 1845, in which the tongue presented most usually a broad, soft, creamy appearance. Mr. Hey, in his *Account of the Puerperal Fever as it visited Leeds*, makes the following remarks, which are pertinent to our present subject, as marking the contrast, in this symptom, between the two epidemics:—

"The tongue was never incrustated with the dry brown fur of typhus, except the disease was of long continuance, or had been improperly treated. It was generally moist and soft, and though it was not unfrequently covered with a thick white or brownish fur, yet it was often but little altered from its natural appearance to the last, even in bad cases."

"Diarrhoea was present in most of our cases, but was not so conspicuous or so formidable a complication as in the epidemic of 1845. I cannot but think that its first production was often attributable to the mercury and some of the other remedies which were used to subdue the disease; had it been otherwise, it is probable we should have experienced more difficulty in restraining it.

"Fulness of the belly, with tympanitis to a greater or less extent, was almost universal; but in the individual cases this condition did not become remarkable till an advanced stage of the complaint, except in those which showed

from an early period a preponderance of the symptoms referable to inflammation of the peritoneum.

"The extreme rarity of cerebral disturbance in the course of puerperal fever is attested by nearly all observers, and the general tenor of my own experience agrees therewith. Nevertheless, I saw four cases which were exceptions to this rule. Two women, some hours after the first appearance of the disease, became quite lethargic, insomuch that it was only with great difficulty they could be roused to any degree of consciousness; and in this state, closely bordering on coma, they remained till their death, not many hours after. Another patient was affected in quite an opposite way. She was very restless and excited, wanting to get out of bed, and with difficulty restrained from doing so. Along with this she had a kind of noisy delirium, bearing a very close resemblance to one form of puerperal mania. These three women exhibited in a marked degree the same morbid appearance, namely, putrescence of the interior of the uterus and sloughing of the vagina. The fourth patient actually became maniacal three or four days after the development of puerperal fever. In the course of a week, however, she regained possession of her reason, but was very near dying of the puerperal fever.

"There seemed to exist throughout the epidemic a strong tendency to putrescence or sloughing of the uterus and vagina, and this, too, quite irrespective of the length or character of the labour. In six cases, we had direct proof of the existence of this gangrenous condition; two of these were patients that recovered, and had sloughing of the vagina.

"This constitutes an important feature in the late epidemic, and places it in strong contrast with the disease as it presented itself to Dr. Joseph Clarke and Dr. Collins; for neither of these authors makes any mention of such having occurred in their experience.

"Dr. Collins, in describing the morbid changes which he met with in the uterus, as a result of puerperal fever, thus expresses himself: 'The uterus, in the great majority, was quite natural in appearance; in some it was soft and flabby; and in a few, unhealthy matter was found in the sinuses.'

"Elsewhere in his report he states that only one case of sloughing of the urethra occurred during his seven years' mastership.

"Dr. Clarke, in his account of the epidemic of 1787-8, distinctly says that no unequivocal marks of putrescency in any part of the system appeared in the disease.

"It has already been stated that in every instance the pulse was found to be very rapid. At the commencement of an attack it was rarely below 112, occasionally much higher; and as the symptoms became more developed, and the disease made progress, the pulse commonly rose to 130, 140, and even 160. The other characters of the pulse were sufficiently remarkable to render them deserving of notice. In no one instance could we have applied to it the epithet 'incompressible;' on the contrary, it was invariably soft and yielding, and gave to the finger a sensation that is best described by calling it 'liquid or undulating.'

"During the epidemic of 1845, and I believe in former epidemics also, trismus and convulsions prevailed to an unusual extent among the children born in the hospital. It is a fact, however, worth recording, that not a single example of either of these complaints presented itself during the entire period of the late visitation.

"It rarely happens that puerperal fever breaks out in the hospital without its contemporary appearance in private practice; and it never happens, I believe, that it prevails to any extent outside of the hospital without appearing among the patients within its walls. On the late occasion, I had reason to know that several deaths had occurred from the disease amongst women confined at their own homes, and lacking neither comfort nor attention, before it visited the hospital. Nor since then were its ravages confined to the poor inmates of our wards; for many women among even the upper classes of society were carried off under its fatal influence. During the months of December and January, no less than twelve of such deaths, in and about Dublin, came to my

own knowledge; and I have heard of four or five more occurring in the beginning of last month.

"On the outset of the fever in the hospital it displayed uncommon virulence, and the first seven patients who were attacked fell victims to its deadly malignity.

"It may not, perhaps, be uninteresting to mention the number attacked on different successive days, as marking, to a certain extent, the progress of the epidemic. Thus, on each of the following days, viz: the 1st, 3d, 4th, 6th, 9th, 10th, 11th, and 12th of December, there was *one* woman seized with the disorder; *three* on the 13th; and *three* on the 15th: on no subsequent day of this month was there more than one; and on many days not one was attacked; but in February *two* were attacked on the 8th; *two* on the 9th; and *two* on the 10th.

"With respect to the period after delivery at which the patient was seized, the following are the general results: 2 were attacked in three hours from the completion of labour; 1 in four hours; 1 in twelve; 1 in fourteen; 1 in seventeen; and 1 in twenty-two hours. Each of these seven cases terminated fatally.

"One was seized in twenty-two hours after delivery, and 1 in twenty-three; the former recovered, and the latter died. Thus, we see, of 9 patients, in whom the complaint manifested itself on the first day of childbed, 8 died; 12 were affected on the second day, 6 of whom died; 10 were attacked on the third day, and of this number the disease proved fatal to 3.

"One woman, who was slowly recovering from an attack of scarlatina, which came on soon after delivery, was seized with symptoms of peritonitis on the tenth day, under which she rapidly sank. There are yet five cases to be accounted for, but in these we could not fix the precise day on which the disease attacked them, so stealthy and imperceptible were its incipient advances.

"This low, insidious manner in which the disorder not unfrequently crept into the system (if I may so say), taking hold upon the vitals without giving any unequivocal evidence of its presence, constitutes, I think, a remarkable feature of the epidemic, and places it in strong contrast with the epidemics described by Gordon, Hey, Armstrong, Joseph Clarke, Collins, and others.

"From Dr. Collins's report of the hospital, it would appear that between one-third and one-fourth of all the patients admitted were primiparæ, and this exactly corresponds with Dr. Hardy's and my report. But amongst the patients attacked with puerperal fever, in the late epidemic, a much larger proportion than the above were confined of first children; in fact, 19, or one-half of the 38, had been pregnant for the first time. Curious to say, Dr. Collins's experience on this point is exactly the same as my own; for, of his 88 cases of puerperal fever, 44, the one-half, we perceive, were women in their first labours.

"Although I have not made it a matter of special statistical investigation, still I think I am correct in saying that those women who were in bad health, or suffering any chronic complaint at the time of admission, as well as those who had tedious or difficult labours, were more liable than others to become the subjects of puerperal fever.

"Dr. Joseph Clarke's experience upon this point agrees with mine. He observes: 'Most of our patients attacked in the year 1787 were admitted in a weakly state, or had tedious and fatiguing labours.'

"With reference to the important and much debated question of the contagiousness of puerperal fever, my late experience does not enable me to say anything decisive. Two facts, however, I may be permitted to mention as being, in some degree, relevant to this point. On four different occasions it happened that the two patients in adjoining beds were seized with the disorder. I do not attach any weight to this circumstance myself, but think it right to mention it.

"The other fact easily admits of being construed into a proof of the contagious nature of the disease.

"In two opposite wards (Nos. 7 and 8), on the same corridor, there were nine fatal cases, nearly one-half of the entire number of fatalities, and more than occurred in any other three wards. Now, the only way in which I can

account for this is, that a mother and daughter are respectively the nurses of these two wards, and having, on this account, more intercommunication, would be very likely to convey infection from one ward to the other.

"The duration of the disease in individual cases varied a good deal. One patient died in fifty hours from the period of invasion; 1 in sixty hours; and 2 in seventy-two hours. These were our most rapid cases. Four or five days was the average length of time that patients lived after being seized with a fatal attack of the fever.

"The influence of the seasons has been sometimes alluded to as a cause of childbed fever. In the *Mémoires sur les Hôpitaux de Paris*, M. Tenon has given a series of tables exhibiting the number of births, and the mortality of lying-in women and children at the Hôtel-Dieu, in the several months of each year of the decade from 1776 to 1786 inclusive. These statistics show December to be the most fatal month, and June, July, August, September, and October the least so.

"Of the mode of treatment pursued with the different patients who were attacked with the disease, I cannot here give a detailed account; but a brief outline of the general principles on which it was conducted, and of the comparative utility of the principal remedies employed, may prove not unacceptable to the Society.

"I believe it may with truth be affirmed that bleeding, in this epidemic, was inadmissible. The only cases in which it was tried proved it so, and both of them died, the disease seeming to be wholly unaffected, if not aggravated, by the measure. My opinion on this point is not in the least shaken by the dictum of Gordon, 'That puerperal fever is inflammatory at the commencement, and putrid only in its progress;' backed though it be by the experience of Hey, Armstrong, and Professor Meigs, the latest and most voluminous author on puerperal fever. Gordon himself states that, unless he could abstract twenty-four ounces of blood at the first depletion, he despaired of the patient's recovery; and this very statement explains the secret of his success. His cases were nearly all examples of the sthenic, synochal, sporadic form of the disease, which, as we all know, is by far the most manageable form. But the cases which would not bear bleeding, and which, in other words, approached to the low typhoid puerperal fever that is chiefly met with in hospitals, he found to be the most intractable and the most fatal. We find Dr. Meigs, too, saying, 'Very few persons can be expected to survive these childbed fever inflammations, whether accidental or unavoidable, when the circumstances forbid a resort to bloodletting.' In support of the supposition above thrown out, I would beg to draw attention to the significant fact that each of these four authors, Gordon, Hey, Armstrong, and Meigs, the great champions for the lancet in the treatment of puerperal fever, derived their experience of the disease from *private practice*; and it is now well established that a strict parallel as to the mode and results of treatment can be rarely instituted between the disease as it presents itself in hospital and in general practice.

"To return, however: although general bleeding was found so wholly useless, yet local depletion deserves to be mentioned in more qualified, if not more encouraging terms.

"Most of our cases that recovered were leeches over the hypogastrium at the very beginning of the attack, and, so far as I am capable of judging, with decided benefit. No doubt the same means was likewise used with some that died; nevertheless, this does not alter my opinion.

"Epithems of spirits of turpentine, hot-water fomentations, and linseed-meal poultices, and hot salt, were external applications in constant use; and, though not in themselves of a powerful nature, were, nevertheless, found to be indispensable auxiliaries in the treatment.

"*Mercury* was tried in a large proportion of cases, and in various doses, but I cannot say I ever observed any decided improvement to have been traceable to its specific action on the system. In some instances, the disease progressed with such frightful rapidity that absolutely there was not time for the drug to make an impression upon the constitution. In other cases the mercury seemed to have produced diarrhoea, and had, therefore, to be laid aside. In two cases

death occurred, notwithstanding that pyalism had been excited. As a purgative, it was in constant requisition, but always combined with, or followed by, other cathartics, and in this way it was found, as it always is, an efficient and useful agent. Many of our cases that recovered got repeated doses of calomel or blue pill, but in one instance only were the gums touched, so that, if it cured the disease, it did so without affecting the system. When exhibited with this intention, it was always combined with opium, and occasionally with camphor also.

"*Rectified oil of turpentine* is another remedy that was largely employed, but in only two, or at most in three cases, did it seem to have been decidedly productive of benefit; and in all these cases wine, and in two of them camphor, was given at the same time. Combined with an equal quantity (three or four drachms) of castor oil, the turpentine proved to be a most valuable anti-flatulent purgative. On other occasions it was exhibited in one or two drachm doses every hour or second hour. It never sickened the stomach, and patients made no complaint of taking it.

"In one case I tried the *opium* treatment, giving a grain every hour till unequivocal indications of narcotism came on (which happened after six grains of the drug had been taken), but without any amelioration of the symptoms. This woman had been bled before the opium treatment commenced.

"If we might judge from this solitary instance, the disease in question does not seem to engender any very apparent tolerance to this medicine.

"*Wine* was allowed to all our cases; and in some from a very early period of the disorder. All the patients who recovered from a bad attack of the complaint got wine to the extent of eight, ten, or twelve fluidounces in the twenty-four hours; and this from the second or third day of their illness. Some of them, too, got brandy along with the wine. In forming an estimate of the utility of this stimulant, I would wish to express myself with the strictest caution and reserve; but I can with truth say, that on no occasion did I see reason to regret its exhibition; whilst in some cases its good effects did not admit of doubt. If I had to encounter another outbreak of puerperal fever, similar to that just subsided, I should, with my present knowledge, give wine much more freely to my patients.

"After a calm and deliberate survey of the symptoms, treatment, and other attendant circumstances of the late epidemic—viewed in relation to this all-important question of treatment—the practical conclusion at which I arrive is embodied in this short precept: To leech promptly—to purge actively—and to stimulate freely. Such, at least, are the leading principles that would guide me, and the treatment of all our successful cases was based upon them. In making this statement, I am fully aware of the facts that the same line of treatment may not be adapted to different epidemics, or even to the same epidemic as treated in hospital and in private practice.

"The proportion of fatal cases in this epidemic is, I believe, somewhat below the average mortality in puerperal fever when occurring in hospital patients, 21 having died out of 38, which is exactly at the rate of 55 deaths per cent.¹ If this result is in any degree attributable (and I am far from asserting that it is so) to the treatment employed, I would feel inclined to ascribe it to the fact of stimulants having been systematically used from an earlier period of each individual case, and given with more freedom than has been heretofore recommended by any author that I know of, excepting, perhaps, Dr. Copeland; and even he did not go beyond camphor and turpentine.

"It is but justice to remark here that, in adopting this, comparatively speaking, stimulant line of treatment, I only carried into effect a suggestion that had been previously thrown out by Dr. H. Kennedy, and which was embodied in a

¹ Thus, Dr. Joseph Clarke lost 21 out of 28 patients seized with puerperal fever in this hospital; Dr. Collins lost 56 out of 88; and Dr. Johnson 10 out of 14; William Hunter, in his hospital practice, had 31 deaths out of 32 cases; and Dr. Leake, in the Westminster Lying-in Hospital, lost 18 out of 19 patients affected with this fever. Thus, to sum up, in 181 cases there were 131 deaths, which is at the rate of nearly 72½ per cent.

paper he read before this Society some years ago, wherein he traced a resemblance between puerperal fever and typhus, and referred them both to the same group or family of diseases."—*Dublin Quarterly Journal of Medical Science*, May, 1855.

68. *Resorption of the Placenta.* Dr. SABATIER, of Bédarieux (France), has published in *L'Union Médicale* of the 24th of April, 1855, three very interesting cases, which would tend to show that portions of retained placenta may be absorbed, and be rejected from the economy, partly by way of the bronchi. The author thinks that the veins take up the products of decomposition, which, entering the torrent of the circulation, are eliminated in the shape of the various excretions—such as the perspiration, saliva, tears, &c.; and he holds that the very fetid expectoration, which was noticed in his three patients, shows that the putrid substances are also taken up by the lymphatics. M. Sabatier does not, however, dogmatize on the subject, but calls attention to the facts, and these certainly point very clearly to an abundant casting off of sphacelated matter from the lungs, without any of the signs which would accompany gangrenous inflammation of these organs. We would beg our readers to refer to a case of this kind brought before the Medical Society of London, by Dr. Winn.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

69. *Prolonged Retention of Life by Infants who have not Breathed.*—The long period during which life may, under certain circumstances, be retained by the infant who has never breathed, is a fact full of interest to the physiologist, the medical jurist, and the accoucheur. The experiments of Legallois show that, in the mammalia, the foetus which has not breathed can resist death from submersion much longer than the foetus in which respiration has been carried on. Puppies and kittens, immediately after birth, may be kept under water for twenty-eight minutes with impunity; when five days old, they perish after sixteen minutes' submersion; and, when fifteen days old, they die as rapidly as other warm-blooded animals of any age, from deprivation of air. The human stillborn foetus can probably live longer without respiration than any other mammalian foetus. The following cases are collected in the *Gazette Hebdomadaire* for December 1, 1854, from different sources. They are very striking, and very suggestive to the practical accoucheur.

Case 1.—A woman, aged 25, who had tried to conceal her pregnancy, was delivered when seated on a tub. The infant, born without any signs of life, was buried in a sand-pit, and, after remaining there for half an hour, was removed, and lived. This case is described by Dr. WEESE in 1845, in *Badisch. Ann. f. Staatsärz.*, x, 2.

Case 2.—In 1850, a young woman was tried by the tribunals of Berlin, who had buried her new-born male infant, believing it to be dead. After an hour, the infant was disinterred, and recalled to life.

Case 3.—T. P., a servant, aged 23, was delivered in a stable, when leaning against the wall, alone, and in a state of unconsciousness, about half past 4 A. M., on the 16th of October. When she came to herself, she found the in-

¹ Since writing the above, Dr. Sinclair has informed me that the mode of treatment pursued here in puerperal fever by my predecessor, Dr. Shekleton, differed in only one point from that above described: Dr. Shekleton did not purge as actively as I did. The results of his experience in the disease I do not know; but I am happy to be able to state that a clinical report is in course of preparation, which will furnish us with full particulars not alone on this point, but of the practice and statistics of the hospital during the entire period of Dr. Shekleton's mastership. Such a work must prove of immense value. And I have no doubt but that the two gentlemen, Dr. E. B. Sinclair and Dr. George Johnston, to whose hands its preparation has been intrusted, will fulfil this duty in an able and efficient manner.

fant on the ground, having a spade lying upon it, with its cutting edge turned to the body. She took the infant, which was perfectly cold, believing it to be dead, and, with the placenta attached, wrapped it up in her apron, and buried it in the garden. Suspicions arose that she had been confined: she confessed; and, at half past nine, the infant was dug up from a depth of thirty *centimetres*. It was found lying on its face, with the placenta under the abdomen. Though cold, apparently dead, and pulseless, the cord was tied. For two hours, P., a surgeon, used means to reanimate it, when at last it began to breathe feebly, gradually signs of life became more evident, and it cried. Some slight wounds were observed on its body: wounds in the neck, which did not bleed at first, bled when the infant was restored. It took the breast greedily. On the 17th and 18th, the wounds suppurated; and, on the 19th, it died of convulsions. The physicians intrusted with the judicial autopsy reported that it had been inhumed before it had breathed; and that it had not breathed till after it was exhumed, and that the statement of the mother was possibly true. She was therefore acquitted of the charge of infanticide, but was found guilty of concealment of pregnancy. This case is reported by Dr. МАСШКА.

Case 4.—A. gave birth about noon, in a private house, to an infant, who gave no signs of life. For an hour, different unsuccessful attempts were made to animate it. The skin became blue, and gradually warmth left it. It was considered dead, and in three hours it was removed to a cold room. [This occurred in January, and the weather was very cold.] Towards evening, it was closed up in a coffin. During the night, the windows of the room remained open. At 11 A. M. on the following day—that is, twenty-three hours after the birth—Dr. Maschka, being accidentally at the house, was asked to look at the body. It was perfectly cold and blue; the eyes and mouth were shut; the joints and the extremities were flexible; there was neither rigidity nor cadaveric discoloration. Astonished at this latter circumstance, but not really doubting that the death of the infant was real, Dr. Maschka placed the stethoscope upon the region of the heart, when, to his amazement, he distinctly heard the sounds of the heart: they were feeble, and at long intervals. It was impossible to appreciate the impulse of the heart against the walls of the chest, or to perceive any movement in the corresponding intercostal space. Attempts at resuscitation were made, but without any effect. At the autopsy on the following day, cadaveric discolorations and rigidity were present. The lungs were of a deep red, and contained no air; they were heavier than water. There was blood in the left, but none in the right side of the heart. It must be admitted—provided the observation of the reporter be correct—that this infant lived twenty-three hours after birth, and never breathed.

Case 5.—The following case is extracted from the *Gazette des Tribunaux* of Feb. 20, 1850: A woman was tried for attempting infanticide; she had buried her infant, but it was dug up, breathed, and lived. The following are the facts: Marie and Renée lived with their father at Vernantes, in the *arrondissement* of Baugé (Maine-et-Loire). On the 16th of May, 1849, Marie was alone in the house with her father. About half past six P. M., Renée came home, found Marie in a swoon, and called the neighbours to her assistance. Marie soon regained consciousness. One of the female neighbours, having observed numerous spots of blood, asked Marie if she had been confined. She replied, "No, it is not yet time." Her father, however, having observed the earth disturbed in a place in the garden, asked her to explain it, when she replied, "I have been confined; but, as my child was stillborn, I buried it in the garden." The child was then disinterred. It was found five *centimetres* [about two inches] below the surface, with its face downwards, and with the placenta attached. Means were used to restore the infant, and it was ere long recalled to life. It was calculated that the infant had been three-quarters of an hour under ground. The accused was acquitted.

The elaborate memoir of Dr. Maschka terminates with the following summary:—

1. New-born infants can live without breathing, not merely for half an hour or an hour, but for a very considerably longer period, even in circumstances the most unfavourable.

2. In such cases, there is obviously not only an arrest of blood in the capillaries of the skin, but the vessels of the different organs are either in a state of permanent contraction, or are filled with a column of stagnant blood.

3. The movements of the heart must gradually become very slow. Here, a very important question presents itself. How can there be circulation in the infant without respiration, and therefore without the production of arterial blood? This condition of passive life can only exist when assimilation is at a minimum, and when the oxygen of the maternal blood is consumed very slowly. The maternal blood must in such cases be sufficient to maintain life.

Dr. Maschka's essay is contained in the *Vierteljahrsschrift f. Hoprakt. Heilkunde*, t. iii, 1854. It contains interesting physiological discussions, which will repay perusal.—*Assoc. Med. Journ.*, Dec. 8, 1854.

70. *Lead Poisoning; the Artisans who are most Exposed to it.*—In Henke's *Zeitschrift für die Staatsarznei Kunde* (Hft. I. 1854), Dr. BIERBAUM has recently published a lengthy paper on this subject, which contains much valuable information on the relative prevalence of this affection among certain kinds of artisans. There are two great classes of men very liable to the lead disease, viz:—

1. Those who are engaged in obtaining the metal, and in manufacturing its preparations.

2. Those who use lead and its compounds for industrial and artistic purposes. Among the first class are, *a*, the miners who dig the ore, and the foundry-men who fuse it thereafter; both are very often martyrs to the affection. *b*, Also, those engaged in the fabrication of leaden articles. Tanquerel des Planches found that 406 out of 1213 cases of lead poisoning occurred in individuals thus employed. *c*, Those who work among salts of lead, as cinnabar, litharge, etc.

In the second class are found, *a*, Artists. They used to be affected more in former times than they are now, as they were then obliged to grind their own colours in a dry state, and thus they unavoidably inhaled the dust. *b*, Heraldic and decorative painters are not very subject to the disease. Among 1213 cases, 33 were decorative painters (Tanquerel des Planches). *c*, Varnishers of carriages and metal are very often thus affected. *d*, Manufacturers of pottery and earthenware, and porcelain, are also liable on account of the salts of lead used for the process of enamelling. *e*, Artisans in glass-works are not very liable to the disease, although the oxide of lead is used to facilitate the vitrefying process. *f*, Manufacturers of cards and fancy paper are also not much affected, although they use the preparations of lead very extensively. *g*, Type-founders, compositors, and printers, as also, *h*, tinsmiths, etc., are very little subject to it. *i*, Lapidaries are not very much affected by lead colic, for although the stones are subjected to the friction of a leaden wheel, the particles of the metal liberated thereby are not inhaled, as the stones are covered with water and emery powder during the process. Tanquerel des Planches found 35 cases among 1213 of lead disease occurring among lapidaries. *j*, Copper and bronze foundries are occasionally affected, on account of the lead which is mixed with these metals. *k*, Workmen in shot manufactories contract lead disease occasionally. Des Planches found 11 cases out of 1213, among those employed in such works.—*Monthly Journ. Med. Sci.*, April, 1855.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Hæmoptysis successfully treated by the Horizontal Position and Cold to the Chest. By WM. H. BYFORD, M. D., of Evansville, Ind.—The report of several cases of fatal hæmoptysis, in Dr. Moreland's extracts from the Records of the Boston Society for Medical Improvement, reminded me of a very severe case that recently came under my notice, and which, on account of success attending what I suppose to be rather unusual management, I propose, with your permission, to place on record in your valuable journal. As I was passing the drug store in which Dr. F. was a partner, he called to me to stop. Upon observing him first, I saw that he was expectorating blood with so much rapidity that he scarcely had time to breathe, and could speak no more than one word at a time, and that with great difficulty. I supported him to the counting-room, which he reached with the utmost exertion only, and placed him in a chair. I then opened the door, undid his bosom and collar, and ordered one of the young men to get me some medicine; but before this last could be done the hæmorrhage had increased so much that it was impossible for the patient to expectorate it as fast as effused into the bronchia, and swallowing was out of the question. The livid colour of asphyxia rapidly manifested itself in the hands and face until the surface seemed entirely black. The respiration became convulsive, imperfect, and finally entirely ceased. The pulse was feeble, slow, almost imperceptible, and at last could not be felt at the wrist; general, but weak convulsions occurred, and all seemed to be over. In fact, I thought he was dead. Assisted by his partner, I lifted him from the chair, and placed him in the horizontal position upon the floor in such a manner as that the head and thorax were considerably lower than the rest of the body, and applied cold water liberally to his neck, face, and chest. Immediately after getting him in this position, a gush of blood to the amount of several ounces took place from the mouth, and partially freed the bronchia from this fluid. Again short convulsive efforts were succeeded by more perfect respiration, the pulse became perceptible, and the patient gradually revived, so that in fifteen or twenty minutes he was able to speak and expectorate the blood with more freedom. I did not venture, however, to move him from his position for nearly an hour. At the end of an hour and a half he was removed to his residence, about two squares distant, in a large rocking-chair. The hæmorrhage seemed to cease very soon after commencing the use of cold water freely to the surface, and no more blood was discharged than I supposed had been effused into the bronchia, when he was first placed upon the floor. The livid colour did not entirely disappear from the surface until the lapse of eight or ten hours after the attack. Under the use of appropriate remedies, in the selection of which I had the judicious advice of Dr. D. Morgan, he has gradually recovered from his prostrate condition so far as to be able to walk about his room. Judging from auscultation, we supposed the blood was effused from a cavity of some extent, or its neighbourhood, situated in

the apex of the left lung. We also thought that unmistakable evidence was afforded by auscultation, that the fluid occupied the bronchia of almost the whole of both lungs; it probably having been diffused by imperfect respiratory efforts and gravity throughout that great extent.

In reviewing this case, and taking into consideration all its symptoms, it is apparent that asphyxia had supervened, and would have resulted in death, had the sitting or ordinary recumbent posture been persevered in even for a few moments. The *besoin de respirer* had lost entirely its control over the medulla oblongata, and complete unconsciousness prevented the patient from making any of those instinctive or intelligent efforts that could have resulted in the expulsion of the blood which had shut out the air from the bronchia and air-cells. But so soon as the blood was partially evacuated by the force of gravity, the air rushed into the lungs, aerated a part of the blood in the pulmonary capillaries, sent it forward to the heart, stimulated that organ to action, and thus renewed the circulation. Although most cases of asphyxia—and I think this is the way in which pulmonary hemorrhage generally proves fatal—from pulmonary hemorrhage occur in the absence of medical aid, yet sometimes an opportunity occurs that will enable us to act with sufficient promptness to save life; and I think that the position in which the thorax and head are dependent is the only sure means of evacuating the blood from the bronchia and air-cells, and admitting the air. I cannot refrain from remarking also that cold, applied to a sedative degree to the surface of the chest, has in many instances under my observation contributed very materially in arresting this hemorrhage.

Hydrocele successfully treated. By BEDFORD BROWN, M. D., of Salem, Fauquier Co.—A hydrocele of unusually large dimensions, containing at least a pint and a half of fluid, was subjected to the following operation for its radical cure. All the coats of the scrotum were laid open, by two longitudinal incisions, measuring about two-thirds the length of the tumour, and giving vent to the fluid contents in a full stream. When the vaginal sac had been thoroughly evacuated, the testicle examined and found healthy, a tent of cotton cloth, measuring two and a half inches in length and half an inch in width, was inserted into the vaginal sac, under the most dependent end of the testicle, and left hanging out of that cavity.

The idea was to permit the tent to remain until adhesive inflammation should be excited by its influence, and then remove it before the least indication of suppuration supervened.

In about three days from the operation, and when the vaginal tunics had become enormously thickened by effusion on their surfaces of coagulable lymph, the tent was removed. The two vaginal tunics were perfectly united by adhesion, forever obliterating the vaginal sac.

The above mode of treatment of hydrocele I have found invariably successful, without involving the least danger, and giving but little pain or trouble, with the exception of the subsequent swelling of the coats of the testicle, which can be rapidly reduced by the use of warm-water applications.

Emmenagogue Properties of Chamomile Flowers.—By H. T. BROWN, M. D.—The emmenagogue properties of these flowers seems to have been overlooked by all writers on materia medica, and also by the profession in general, until within the last few years. My attention was first directed to the emmenagogue properties of this article in the spring of 1853, while treating a young lady, who had already arrived at the age of eighteen years, without

having any appearance of the menstrual flow. I was called to remedy this unnatural delay of menstruation, which I endeavoured to do by using such means as had been most efficient in bringing about the natural discharge. Having pursued the usual course of treatment for several days, without any apparent benefit, I was induced to try the tincture of chamomile flowers, which I prepared by infusing two ounces of the flowers in a pint of dilute alcohol. Of this tincture, I directed a teaspoonful to be taken every three hours, using at the same time a liniment composed of tinct. camphor ℥vi , oil turpentine ℥i , spts. ammonia ℥iii , as an external application to the pelvic and lumbar regions. After pursuing this course of treatment about twenty-four hours, there was a very copious catamenial discharge which lasted a few days, giving entire relief to the existing symptoms, and also restoring to the system its former health and vigour.

I have had numerous opportunities of witnessing the effects of the chamomile in obstinate cases of amenorrhœa, and have found it to be one of the most effectual remedies in such cases, both for regulating the recurrence of the flow, and also the amount eliminated at each menstrual period. Whether chamomile possesses the power of determining the flow of blood to the pelvic viscera, or whether its emmenagogue properties are resident in its tonic principle, will be a matter for future investigation.

Tapeworm expelled by Infusion of Pumpkin-seeds.—By D. LEASURE, M. D., of New Castle, Pa.—In this Journal for July, 1854, I reported a case of tænia expelled by infusion of pumpkin-seeds, and mentioned incidentally that the patient's mother had, at an early period of her life, passed a tapeworm under the use of a "secret vegetable remedy, probably male fern."

On the 6th day of April last, I was somewhat surprised to receive from the old lady a tapeworm, twenty-one feet eight inches long, in every respect similar to the one passed by her daughter a little less than a year previously. The head in this instance also was attached to the worm, showing its entire expulsion.

She informed me that, having discovered joints of the worm in her dejections, she resorted to the infusion of the pumpkin-seeds on her own responsibility, and took it in the same manner as previously taken on my prescription by her daughter, viz: a pint of the bruised seed to be infused in three pints of boiling water, and left over night, the whole to be taken next day, the patient fasting in the mean time."

Rheumatism (Rheumatismus vagus seu dolens) successfully treated with Atropa Belladonna. By B. ROEMER, M. D., of Jackson, Madison Co., Tenn.—On the 25th December, 1854, we were called in consultation to Mr. R. F., who had been suffering for twelve weeks with constant rheumatic pains. Mr. F. is a carpenter, about thirty years old; always had good health, and had suffered the first attack in consequence of unusually severe labour. (We may also add that, to judge from his walk, he suffers from ischiatic lameness.) Mr. F. had been attended by different physicians, who did not check the progress of the disease. When we saw him first, he suffered excruciating pains in the back and the loins, which soon changed towards the knee and toes (right leg), from which parts they again resumed their former position in the course of a few hours. Having taken charge of the case, we administered mass. pilul. hydr. gr. v, followed by an infusion of senna, after which the ammoniated tincture of guaiac was given for several days. Finding that the pains rather increased, we discontinued this plan of

treatment, and began with the liquid extr. of belladonna (containing 10 grs. extract to one ounce water), of which eight drops, gradually augmented to twenty, were taken three times a day. After following this course for three days, we found the pains abating. A relapse taking place in consequence of imprudent eating, we applied acupuncture along the sartorius muscle, and ordered an ointment of belladonna gr. xx, and lard ʒviiij , twelve grains of which were rubbed along the non-punctured parts of the affected leg, the skin having been reddened with diluted sulphuric acid. Besides a severe retention of urine, which was speedily removed by means of pulv. canthar. gr. ss, camphor gr. j; no other obstacle occurred in the treatment, and in four weeks the patient was dismissed.

DOMESTIC SUMMARY.

Extirpation of the Uterus.—Dr. G. KIMBALL, of Lowell, Mass., has, it appears, from a communication in our contemporary, the *Boston Med. and Surg. Journal* (May 3d, 1855), performed this formidable operation three times, and one of the patients recovered.

In the first case "the operation was begun with the view of removing a diseased ovary, and terminated in the extirpation of the uterus. Though feeling well assured in this case as to the correctness of my opinion regarding the nature of the disease I was about to encounter (an opinion, too, which, so far as I know, was concurred in by each of the several medical gentlemen present), my first incision through the abdominal parietes revealed at once the unexpected yet unmistakable fact that the tumour in question was no other than an enormous, irregular, lobulated structure; the uterus itself being the only organ involved. My determination, in this aspect of things, was to desist from further prosecuting the operation; but, upon consultation, another judgment prevailed, and it was finally concluded by a complete extirpation of the diseased mass, and with it also the whole of the organ with which it was connected. This patient survived the operation ten days. For the first six days the symptoms were comparatively mild—so much so as to afford considerable hope of recovery. On the seventh day, however, the aspect of things changed for the worse; and on the tenth day, as before stated, the case terminated in the death of the patient."

The subject of the second case was a Mrs. T., of Vernon, Conn. about 34 years of age, who had a globular, movable tumour in her abdomen of about seven inches in diameter. No great inconvenience attended the size of the tumour, but she was greatly reduced by the profuse hemorrhage during the menstrual period.

"Examined per vaginam, the neck of the uterus was found in its natural condition, both in position and size; the os uteri open rather more than natural; a sound readily passed up some four or five inches. The enlarged and diseased portion of the organ could not be reached by the forefinger—the entire bulk of the tumour lay in the abdominal cavity."

To save the patient from the risk of death from the profuse hemorrhage at the menstrual period, it was decided to remove the uterus, which was assented to by the patient.

"She was now put in readiness for the operation by being placed on a properly elevated table, and brought under the influence of chloroform. Upon exposing the abdomen, and observing the small size of the patient, it appeared quite evident that, in order to dislodge the tumor *entire*, it would be necessary to extend an incision from the ensiform cartilage to the pubis. But rather than do this, it was thought better to expose a part only of the tumor, and see what could be done by way of *enucleating* the diseased portion of it—thus reducing

its bulk so as to allow its being drawn out through a comparatively small opening. Accordingly, an incision was made through the *linea alba* directly over the most prominent portion of the tumour, exposing it to the extent of about four inches. Another cut of less extent, through the uterine walls, brought to view the fibrous mass within. Observing that no bleeding followed this procedure, this last incision was prolonged to an extent corresponding with that through the parietes. Through this opening, a portion of the diseased mass, thus exposed, was suddenly and forcibly extruded, seeming, at first, as if a little additional force would be sufficient to dislodge it entirely from its connections. Attachments, however, firmer and more extensive than had been anticipated, rendered this part of the operation rather difficult; but being finally accomplished, and the uterus becoming at once greatly diminished in bulk, it was readily drawn out from the abdominal cavity, conformably with the plan adopted in the outset, and placed in the hands of an assistant.

"A straight, double-armed needle was now passed through the organ in an antero-posterior direction, as low down as the supposed point of its junction with the neck, this part being, of course, left intact as regards its relation with the vagina. By this plan of appropriating to each lateral half a separate ligature, there was no great difficulty in making sure against all chance of subsequent hemorrhage; a consideration of great importance, in view of what might otherwise be very liable to happen.

"The remaining part of the operation was very simple, and easily accomplished. It consisted of a mere amputation of the diseased structure by a single straight incision, carried across from one side to the other, and as near to the ligatures as was consistent with their secure attachment.

"The parts having now been made as clean as possible, the wound through the parietes was brought together, and its edges secured with four sutures. Adhesive strips, and a compress wet with warm water and laudanum, completed the dressing.

"The operation was somewhat protracted, lasting nearly or quite forty minutes; yet it was not accompanied or followed by any extraordinary or alarming degree of exhaustion. The amount of blood lost did not exceed four ounces.

"After being laid in bed, the patient was troubled with nausea and occasional vomiting, which continued for two or three hours. This, however, was probably the effect of chloroform merely. Upon its ceasing, an urgent desire, without the ability, to evacuate the bladder, came on, together with a severe pain in the lower part of the back. The first difficulty was readily relieved by the use of the catheter, the latter by a half-grain dose of morphine—which seemed not only to quiet the pain, but to induce what was then considered a comfortable night's rest."

The patient was left in the care of Dr. Skinner, and subsequently improved rather satisfactorily, except that the ligatures, which remained, proved a source of irritation.

Early in May following, Dr. Kimball visited the patient, and found her greatly improved in strength and flesh. "The ligatures, however, still remained an annoyance, producing a good deal of discomfort, particularly in the exercise of riding and walking. Another attempt to remove them was again unsuccessful; and, from the pain that always followed these efforts, it was thought advisable rather to allow them to remain attached for an indefinite time longer, than to subject the patient to repeated failures. This conclusion seemed reasonable and safe, from the fact that their presence was looked upon as a mere *inconvenience*, and not implying any danger.

"This visit, as stated above, was made early in May, eight months subsequent to the operation. From that time to the present, my further knowledge of the case has been only of an indirect character, yet quite satisfactory. From several individuals coming from the immediate neighbourhood of the patient (one of them recently), I learn that the operation is spoken of as perfectly successful, and the patient herself restored to health."

In regard to the third case, Dr. K. states that the motives which induced him to operate were substantially the same as decided him in regard to the second

case. The patient died on the third day, and upon *post-mortem* it was shown that a ligature had slipped, and that hemorrhage had been the immediate cause of death.

Gastrotomy successfully performed for the removal of a Bar of Lead from the Stomach.—Dr. JOHN BELL, of Wapello, Iowa, records (*Iowa Medical Journal*, April and May, 1855) the following very remarkable case of this:—

On Christmas day, a man came to Dr. Bell's office, who was said, while performing a favourite feat of running a bar of lead down his throat, to have accidentally let it slip, and that it had descended into the stomach. The man asserted that he had swallowed the bar, and added, "that it was nothing wonderful for him to do, as he had swallowed three or four bars at previous times." This was said in a half wagish manner, and being to all appearance partially intoxicated, and having, withal, the reputation of being an expert at juggling and sleight of hand, Dr. Bell supposed it to be one of his tricks, and this opinion was strengthened from the fact that he seemed to be suffering no inconvenience. Dr. Bell came, therefore, to the conclusion that it was a hoax, but, to satisfy himself further, he passed a sound down the oesophagus, but could not discover anything. A few minutes afterwards, Drs. Cleaver and Bell, after a brief consultation, concluded to introduce the sound again; they did so, but with no better evidences of the presence of lead than before. They told him to go about his usual employments, and, should it trouble him, to send for them. The next day he went to work, and continued at work for three or four days, when, becoming unwell, he went home, some six miles from Wapello, and sent for Dr. Robertson, of Columbus City. On Monday, Jan. 1st, Dr. Robertson requested the physicians of Wapello to meet him forthwith at the residence of the patient. Drs. Taylor and Bell answered the summons promptly. Drs. Robertson, Neal, Cleaves, Graham, and Crawford had arrived before them. The patient was closely examined, and there was found no perceptible external evidence of the presence of any foreign body in the stomach; the patient was comfortable, up and about, and seemed well, except some paleness, which might have been produced by the regimen enjoined. After considerable conference, it was deemed best not to operate at that time; instructions were given to keep the patient on a low diet, and open the bowels by a saline laxative, and should any untoward circumstances or symptoms supervene, to notify the physicians at once.

Dr. Bell was hastily summoned the next day, Jan. 9th, to see the patient, and found him suffering from gastralgia and abdominal soreness; there had been considerable retching and vomiting of a dark watery fluid; pulse small and tense; great anxiety, restlessness, prostration, and apparent sinking of the vital powers. The bowels had not been moved; very sensitive to pressure over the left iliac and inguinal region.

In consultation with Drs. Taylor and Robinson, it was agreed that a bar of lead had been swallowed, and that an operation was advisable for its removal. Accordingly, the next day, Jan. 3d, Dr. Bell operated, assisted by Drs. Robertson, Cleaves, Graham, and Taylor, in the following manner:—

"The patient having been properly placed and secured, chloroform was administered. It produced, at first, some nausea, and the patient threw up a quantity of black, fetid, watery fluid. As soon as insensibility ensued, I made an incision from the point of the second false rib on the left side to the umbilicus, dividing the skin and cellular membrane; thence through the abdominal muscles to the peritoneum; made a minute opening at the lower end of the section through the peritoneum, passed in the director, and, with a probe-pointed bistoury, divided it through the entire length of the incision. The division of the peritoneum produced a spasmodic contraction of the abdominal muscles, and a large quantity of the omentum and bowels were ejected from the orifice; these I replaced as speedily as possible, and at once passed my hand inward and upward through the incision, grasped the stomach, and immediately discovered the lead and its position. It lay in a direction from right to left, the upper end resting against the walls of the stomach, to the right of the cardiac orifice; the lower end in the greater curvature of the stomach, to the left of and

below the pylorus. As it was impracticable to reach the upper end, I seized the bar between my thumb and middle finger, and with the forefinger on the lower end of it, I retracted it upward and backward, for the purpose of making the incision in the stomach as high up as possible. I then passed a scalpel in, along the side of the forefinger, as a guide, and divided the coats of the stomach immediately at the end of the bar, making the incision parallel with the muscular fibres, and not larger than to admit of the removal of the lead. I then introduced a pair of long forceps, seized and drew out the lead. The external orifice was closed with the ordinary interrupted suture and adhesive strips; a compress was applied, and a roller around the body.

"The time of operating was twenty minutes; considerable delay was occasioned by the protrusion of the contents of the abdomen, which had to be replaced before the operation could proceed. As soon as the effects of the chloroform passed off, $\frac{1}{4}$ gr. sulph. morph. was administered, and the patient left in charge of a judicious medical attendant."

We need not give the details of the subsequent progress of the case; it is sufficient to say that convalescence was established as readily as after most of the minor surgical cases. "The patient was discharged on the 15th day after the operation, and has continued well up to this time; he is now residing in this village, working daily at his trade, that of a shoemaker. The orifice in the stomach was made on the left anterior side, and I think about one inch below the pylorus; the opening was just large enough to withdraw the lead. From some cause, probably from the efforts to vomit, a portion of the omentum had been forced out between the sutures, and when the adhesive strips were removed for the first time, it was found protruding from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch. Upon examination with a probe, I found it had formed adhesions on both sides of the orifice. I therefore removed the external portion with a pair of scissors.

"It may be a matter of surprise that an operation was not had sooner. Our reply to a question of that nature is, that an operation of that magnitude was not justifiable, as long as there was any doubt as to the lead being in the stomach; and the evening previous to the operation was the earliest time that all doubts of the fact had vanished, and the operation was performed at the earliest practicable moment thereafter. Although I had seen the patient occasionally for three or four days after this singular feat had been performed, and was called on the 8th to witness an operation, during all this time I had not seen one single symptom that was conclusive evidence of the presence of a bar of lead in the stomach.

"The length of the bar is $10\frac{1}{2}$ inches, and its weight $9\frac{1}{2}$ ounces avoirdupois."

Wound of the Stomach; Recovery.—Dr. C. HAPPOLDT records (*Charleston Med. Journ. and Rev.*, May, 1855) an example of this:—

The subject of it was a lad 17 years of age, who, on the 10th February, received a wound from a bowie-knife, which entered at a spot three inches to the left of the middle of a line drawn from the ensiform cartilage to the umbilicus; and from the testimony of bystanders, the instrument penetrated to the depth of about four inches.

The patient had recently eaten large quantities of pastry and ground-nuts, and had drunk immediately before the accident a pint of sarsaparilla beer. Upon receiving the wound, he closed and wrestled with his opponent for some minutes, when he was taken away and conveyed in a carriage to his residence, where Dr. H. saw him at 5 P. M., one hour after the infliction of the injury.

"The longitudinal direction of the wound was parallel to the axis of the body, and was three-quarters of an inch in length; the edges were gaping widely; and there was no hemorrhage. The garments covering the thorax were saturated with a colourless, inodorous fluid, and the circumference of the wound was incrustated with about an ounce of white lardaceous matter. Having no probe at hand, I could not ascertain the depth or direction of the wound, but, from the fact of what appeared to have been the contents of the stomach having escaped through the external orifice, I concluded that the cavity of that viscus had been entered.

"Trusting that the wound of the stomach would close and adhere by the con-

traction and quiescence of the organ, I brought the edges of the tegumentary wound together by a single suture; and applied adhesive straps and a compress and bandage, to prevent muscular contraction and the access of air into the abdominal cavity. The patient was placed on his back: and the twelfth of a grain of the sulphate of morphine was given every two hours until he fell into a sound sleep."

Some fever supervened, which was subdued by appropriate remedies, and eleven days after the reception of the injury, the wound had closed.

"The case," Dr. H. remarks, "is rendered interesting from a favourable termination having occurred under the very circumstances which are universally regarded as most adverse and unfavourable to recovery. The stomach was full at the time when the perforation occurred, and, from the perturbation and compression to which it was necessarily subjected by the exertions of the patient, some of its contents were expelled not only externally through the parietal wound, but also into the abdominal cavity, as will be presently shown. Effusion of the contents of the stomach into the abdominal cavity constitutes the chief danger in these wounds. Percy, in the *Bulletin de la Faculté de Med.*, vol. i, p. 320 (quotes the writer of the article on wounds of the stomach in the *Dictionnaire de Médecine*) says that of twenty wounds of the stomach produced by the sword, the bayonet, or the knife, in which effusion into the abdominal cavity occurred, he has seen but four recover. All writers that have consulted on the subject attach the same importance to this accident. The portion of the gastric contents which escaped into the abdominal cavity must have been retained by the position of the contiguous organs in one spot, until adhesion of the peritoneal surfaces prevented its diffusion. Besides the inferences deducible from the circumstances related, I am strengthened in my opinion that some of the gastric contents did escape within the abdominal cavity by the nature of the matters which issued from the wound five days after the perforation occurred. Although I did not submit the discharge to a microscopic examination or chemical analysis, yet there were innumerable small particles which, from their feel, consistence, and odor, declared them to be the undigested particles of the ground-nuts which had been eaten a short time before the accident. The discharge was, moreover, whiter than pus or serum, and far more offensive. The gastric wound appeared to be near the pyloric orifice, and as the greater curve of the stomach must have been depressed at the time of the perforation, it is probable that the wound was nearer the upper wall or lesser curve of the organ. It might have been overstrained caution which led me to close the external wound as I did, and I cannot now see that any benefit resulted from it; but I could not, on first seeing the case, determine whether any of the gastric contents were lodged within the abdominal walls."

Luxation of the Head of the Radius backward, with Fracture of the Inner Condyle of the Humerus.—Dr. THOMAS M. MARKOE records (*New York Journ. of Med.*, May, 1855) the following cases of this infrequent accident:—

"Case I.—During the fall of 1849, I was sent for to see J. H., who had a day or two before fallen from a hay-loft, striking upon his left elbow. Great pain and inability to move the limb followed the accident, and soon after very great swelling round the injured joint, so that at the time of my seeing him it was impossible to trace the bony prominences of the articulation, and therefore an accurate diagnosis was out of the question. I was forced, therefore, to content myself with supporting the arm in a sling, with the elbow in a semi-flexed position, which was a comfortable one, and applying a cooling spirit-lotion to the inflamed joint with a view of reducing the swelling. This was sufficiently accomplished, at the end of a week's time, to enable me to ascertain that the head of the radius was dislocated backward, and projected behind the external condyle in such a manner that the finger could very plainly recognize the cup-like depression on its superior surface. At the same time, though there was some deformity about the internal portion of the joint, yet there was not sufficient projection backward of the olecranon for luxation of both bones, and the exact relation of the internal condyle and the olecranon could not be fully made out on account of the swelling, which seemed to be greater on this side than on

the other. There was a slight feeling of crepitus somewhere about this internal portion of the joint. There was no considerable deformity in respect to the position of the forearm and the arm, and no deformity at the wrist. In thinking over the symptoms of this case, I could not satisfy myself that it was possible for the head of the radius to be so completely displaced, without either a corresponding projection of the ulna at the wrist, or else a fracture of either the ulna or its trochlear support, which might permit the radius to slip up, or, finally, so great a degree of bending laterally outward of the forearm on the arm as would amount to an angle of more than 45° . In deciding upon these physical possibilities, the symptoms so evidently pointed to fracture of the internal condyle through the trochlea that I could scarcely avoid making that my diagnosis. It seemed to me clear that the fracture had involved the inner condyle in such a manner as to pass obliquely across the bone, and to include in the broken fragment the larger part of the trochlear articular surface, which being displaced backward would allow the ulna to fall sufficiently back to give the radius a chance to be entirely luxated. This view was confirmed by a very thorough examination of the part, made while the patient was under the influence of chloroform, to which we subjected him to facilitate our attempts at reduction. The displacement was easily reduced by extending the forearm on the arm steadily for a few minutes, and making firm traction in the length of the limb, and then bending the elbow to a right angle over the knee. All deformity ceased, and all the motions seemed to be restored. My impression is that, after the reduction, the crepitus became more distinct, and more clearly referable to the internal condyle; but, as I took no notes of the case at the time, and am now giving it from memory, I cannot insist upon that point. The limb was kept in a curved tin splint at about a right angle for three weeks, to give the fracture time to unite. Passive motion was then begun with great care. The arm was gradually restored to its functions, and by the end of a year was as strong and as useful as the other. Two or three years after the accident I examined the elbow, and the only trace which it presents of the injury is a slight thickening and irregularity about the internal condyle.

"*Case II.*—Patrick Riley, æt. 16, an Irish sailor boy, was brought to the New York Hospital, June 5, 1854, with an injury of the right elbow-joint, received a short time before in a fall from a load of wood. At first sight, the appearance of the joint was that of a luxation backward of the forearm, but upon a more careful examination, it was perceived that the olecranon did not project backward nearly as much as is usual in the complete luxation. The head of the radius was manifestly dislocated backward, and the cup-shaped extremity of it could be felt and even seen projecting behind and below the external condyle of the humerus. On the inner aspect of the joint the olecranon could be felt about half an inch further back than it should be, and, in connection with it and displaced backward with it, could be felt the internal condyle of the humerus, while a little above the point usually occupied by the prominence of the condyle, was a sharp, bony projection, which was evidently the extremity of the inner condyloid ridge from which the condyle had been fractured off. The movements of the joint were very much limited in every direction, and in particular, rotation of the radius was much restrained. The case was regarded as one of fracture through the internal condyle, as it forms the base of support for the trochlear articulation, with such a displacement backward of the fractured portion as would allow of complete luxation of the head of the radius. The accident being recent, and examined under ether before any swelling had commenced, the points on which the diagnosis was founded were very clear and easily recognizable, and the conclusion arrived at was confirmed by two of my colleagues who saw the case. By drawing down firmly, and flexing the forearm on the arm, the reduction of both displacements was easily effected, but slipped out again with equal facility if not carefully handled. After the reduction, crepitus became more manifest, and could be referred with precision to the moving of the fractured condyle. The arm was kept in the semi-flexed position, and supported in a grooved tin splint for the usual time, when passive motion was carefully commenced. The recovery was accomplished without accident, leaving him a limb perfect in all its movements, but showing some

little deformity, perhaps from slight displacement backward of the broken fragment of the condyle. None but a very curious eye, however, would observe any difference between the two limbs.

"Case III.—B. P., æt. about 14 years, was brought to me by a gentleman of this city, under whose care the case had been for about a month, for my opinion on an injury of the left elbow, which he had sustained four weeks previously by a fall, in which the inner part of the joint struck with force against a projecting corner of a trunk. Great swelling had taken place before Dr. — saw the arm. He could detect obscure crepitus, but the great swelling prevented a satisfactory examination from being made. He, however, considered the case as one of luxation backward of the forearm, with fracture through the epiphysis of the humerus, and fracture of the coronoid process. He reduced the luxation and bent the arm at a right angle, and placed upon its anterior surface an angular elbow splint, with two corresponding ones on the arm and forearm behind. These were kept on about three weeks, and then passive motion was commenced.

"I found the limb extended nearly to the straight position. Flexion at the elbow was very painful, and so much constrained that it could not be brought up nearly to a right angle. There was great deformity of the elbow-joint, but the inflammatory swelling had very much disappeared, so that it was easy to make out the following points. The olecranon and the head of the radius were in their proper relation to one another, and their motions upon one another, though somewhat restrained, were easy and gave no pain. The external condyle was prominent toward the outer and anterior aspect of the joint, and behind and to the outside of it was seen and felt the head of the radius, into the cup-shaped cavity of which the finger could be introduced. The relations of the internal condyle with the olecranon were but little altered, but there was nevertheless such a projection backward of the olecranon, as, in connection with the luxation of the radius, to give the joint the appearance of luxation backward of the forearm. That the ulna, however, was not displaced from its articulation with the humerus in the usual way, appeared certain; first, because the projection backward of the olecranon, though marked, was not as great as in the complete luxation backward; and secondly, because the internal condyle and the olecranon were in almost their proper proximity to one another. The humerus, in the region of the internal condyle and of the trochlear cavity, was a good deal thickened, and apparently enlarged by the deposit of new bony matter. The whole lower extremity of the humerus was felt prominent on the anterior aspect of the joint, as it is in luxation of the forearm backward. From these appearances, I made up my mind that the accident had been originally one of those above described, in which a fracturing off of the whole or a part of the trochlear surface had, by its yielding, permitted the peculiar luxation of the radius, and the displacement of the ulna. As far as I could ascertain, union of the broken piece had already taken place, but that union was probably still soft. I therefore recommended that the reduction should be attempted in the usual manner, by traction and flexion over the knee, and that afterward the limb should be treated as an ordinary fracture of the internal condyle, by being bent to less than a right angle, and confined in this position for three weeks, beginning then with cautious passive motion. The reduction was effected as recommended, the next day, when all the deformity ceased, the union of the broken fragment being so soft as to yield without trouble, and the motions of the forearm on the arm became perfect. The after-treatment was fully carried out; and when I saw him, seven weeks after, he was rapidly regaining the motions and power of the limb. A good deal of thickening remained about the region of the fractured condyle, but the bony prominences of the articulation had all regained their normal relations to each other, proving that the bones were in their proper position."

Treatment of Pneumonia.—Dr. JOHN T. METCALFE gives (*New York Med. Times*, May, 1855) an abstract of 12 cases of pneumonia which occurred during his term of service in the New York Hospital, two of which were fatal, and offers the following remarks on the treatment of the disease:—

"Of the two fatal cases, one was fairly moribund when first seen. It occurred in the person of a confirmed inebriate, and attacked the upper part of the lung, as is not unfrequently seen in the pneumonia of sufferers from delirium tremens. Treatment seemed to produce no good effect; nor do I think venesection or calomel would have proved useful in the case.

"The other fatal result was in the person of a man with completely developed spanæmia, the result of a malarious fever of the Isthmus, in whom the pulmonary inflammation was complicated with universal bronchitis, and whose extremely prostrated state allowed nothing more heroic than calomel to be used. This seemed productive of no good, and he ultimately died of the secondary bronchitis.

"In the other cases, the treatment, with the exception of that which had reference to particular symptoms, was negative, so far as it was specially addressed to the cure of the disease. Diet, repose, change of posture, and relief by vomiting free accumulated bronchial mucus, constituted the whole.

"It often occurs that patients enter the New York and Bellevue Hospitals, whose histories are such as have been related in the foregoing cases. They have been taken ill with a chill, pain in the side, cough and fever, which have disabled them from work. Looking upon their illness as a 'bad cold,' from which a few days' rest, with a dose of oil or salts, will relieve them, they take to bed, and remain, in expectation of recovery, from four or five days to a week, when, finding no improvement in their condition, they have recourse to the hospital. The account given of themselves, taken with the results of physical exploration leaves no doubt that inflammation of the lungs is the disease in question.

"On referring to those who have written systematically on the pathology and therapeutics of this malady, and who, with students and young practitioners, constitute authority, I have been struck with the almost uniform tendency to consider pneumonia as a disease which threatens life in a most serious manner, and which requires, for its successful management, the energetic employment of antiphlogistic remedies. True, there are, by some, exceptions made in the cases of very old people—of those who have been attacked whilst greatly debilitated, and in epidemics of typhoid pneumonia; but there is still, no doubt, a very general recognition of the necessity for opposing what is looked upon as a formidable disease by heroic remedies. By some authors, implicit reliance is placed upon early resort to the lancet, to scarified cups, and to the production of the constitutional effect of mercury on the system, by inducing ptyalism. By others, the Rasorian administration of tartar emetic is regarded as the means most likely to insure a certain and speedy cure of the disease. It would not, perhaps, be erroneous to state that one or other of these, or a combination of the two, constitutes the basis of English and American therapeutics.

"Observation, by comparing the progress of cases in which bleeding and mercurialization had been trusted to, with those in which rest, proper regulation of ventilation, and appropriate diet, together with such *juvantia* as might be indicated in any particular case, has seemed to convince me that in pneumonia, as we see it in New York, entirely too much stress has been laid on the necessity of having recourse to the former therapeutical course. Nor can I resist the conviction that, in other localities, a careful study of the natural history of the disease would tend to lower the general estimate in which blood-letting and calomel are held as potent agents in curing pneumonia.

"Skoda, drawing not a drop of blood, employing solely *extractum graminis*, or a few grains of nitre, and in a few instances corrosive sublimate, lost three, only, of forty-four patients, whose average age was between twenty-five and twenty-six years.

"Varrentrapp, following the example of Wachrer and Baumgärtner, teaches us how the disease may be successfully brought to convalescence by giving no medicine internally except chloroform vapor, applied by inhalation to the mucous membrane of the lungs. His results show a mortality of one in twenty of those treated in this way, or 5 per cent., agreeing very closely with those of Wachrer, Baumgärtner, Helbing, and Schmidt, who, in one hundred and ninety-three cases lost nine, or 4.25 per cent., on the chloroform treatment.

"In 1848, Dr. Dietl, of Cracow, published the result of one hundred and eighty-nine cases of pneumonia, treated by diet and rest alone. Of these he lost only 7.4 per cent. In 1853, he published a second pamphlet, detailing the success he met with in seven hundred and fifty or sixty cases treated entirely by hygienic and dietetic means. I regret that this monograph has been mislaid. I can only state that the general result accords with that above mentioned, and served to confirm in his mind the superiority of the expectant over the methods by venesection, calomel, or tartar emetic.

"Skoda, Varrentrapp, and Dietl, had all previously commenced their practice by treating inflammation of the lungs according to the traditional means last mentioned. None of them, although good diagnosticians and men of great intelligence, have been otherwise than satisfied with the result of their change of practice. Skoda is not more active in reality than Dietl, in his treatment; nor can it be truly said that the inhalation of chloroform does anything in the way of curing the disease. They all trust to the well-known potency of nature, when allowed to act without restraint or interference; and if they had never made any other contribution to medical science, these physicians would deserve the gratitude of the profession for the information, so little possessed before their researches and so inadequately diffused at present, that pneumonia has a natural tendency to get well when let alone, in a great majority of cases in which it attacks healthy subjects; and that more harm than good results, as a general rule, from the employment of what are called heroic remedies in its therapeutics.

"Had Dietl, Skoda, or Varrentrapp, experimented with the infinitesimal puerilities of Hahnemann, instead of adopting the course actually pursued, we might easily imagine the result on their minds, if they had been as susceptible constituted as those of many men who, having compared the results of Sangradoism with those of Hahnemannism, have joyfully embraced the latter creed, without ever entertaining a suspicion that truth was to be found neither in one extreme nor the other, but where it usually exists, in the happy medium.

"The above is the substance of some clinical remarks delivered at the New York Hospital, when reviewing the treatment of pneumonia, during the months of January, February, and March, of the present year. I have not touched on the questions as to whether there may not be means employed which will relieve particular symptoms, such as pain, cough, and dyspnoea, from excessive secretion and from intestinal accumulations. I am very sure that we do possess such *juvantia*, and that the result of common experience has left no doubt of their value. What I have endeavoured to inculcate is the fact that, in the disease under consideration, there is no *absolute necessity* for resorting to extreme measures.

"In examining candidates for posts in the House Staff of Bellevue Hospital, since the year 1847, I have had graduates from nearly all sections of the United States among the applicants, and have been impressed with the fact that nearly all of them have been imbued with the firm conviction that inflammation of the lungs was a most dangerous affection, one which, if not promptly attacked with the lancet and calomel, or by antimonial medicines, would be very apt to prove fatal to the patient. To such, it has given me satisfaction to show the progress of the disease in cases which have entered the service with all the signs and symptoms of pneumonia, which have had no medicine previous to admission, and for the management of which, attention to diet, ventilation, posture, and a few teaspoonfuls of mucilage of gum arabic in the day, have constituted the sole treatment. Nearly all have been persons of the labouring class, whose return to health was a matter of importance. How different their condition, after the subsidence of the disease, from that of others who had been copiously bled, or who had undergone 'a course of mercury'!

"I do not speak of the power of venesection, as an ectrotic, in the first stage of the disease. My experience has reference to it as seen in hospitals, and as it is almost always met with in private practice. It would be instructive to have a number of such cases detailed, with the evidence that the disease really was the one in question."

Operation under the influence of Local Anæsthesia, induced by Cold.—Dr. L. A. DUGAS reports (*Southern Med. and Surg. Journ.*, Feb., 1855) a case in which he extirpated a fungous tumour from the arm, and in which he employed, with very satisfactory results, cold, as recommended by Mr. Arnott, to produce local anæsthesia.

"The fungous growth was about the size of two fists, somewhat globular, with a flattened surface, and projecting a little beyond the circumference of the attachment, so as to constitute this a neck of about four inches in diameter. The entire surface of the tumour presented the aspect of a ragged mucous membrane or ulcer, from which there exuded an abundant discharge of the most fetid sero-purulent matter. The fætor was such indeed as to infect the whole apartment, to the very great annoyance of the patient, and all who came about him. Around the attachment the skin was swollen, red, and evidently yielding to further encroachments. The axillary glands were enlarged and tender to the touch. The patient's general health was much impaired, from incessant pain, despondency, and, above all, the intolerable stench, which destroyed his appetite and kept him more or less nauseated. He urgently solicited the removal of the fungus, and was willing even to suffer amputation, if deemed necessary.

"In view of the patient's age, his cachectic condition, the doubtful advantage of the operation, &c., amputation was deemed inadvisable, and the simple extirpation of the offending mass was determined upon. From the extent of skin to be necessarily removed, it was evident that the edges of the wound could not be brought in contact after the operation. Yet there was no alternative, and this circumstance having been explained to the patient, as well as the probability that the disease would return soon or late, he very readily consented to the excision.

"This was a case in which the administration of anæsthetics by inhalation could not have been carried very far without risk, and it was one remarkably well adapted to the use of a freezing mixture for the production of local anæsthesia. Prof. Means was therefore requested to prepare the usual frigorific compound of ice and salt, and to superintend its application. This was done in presence of the medical class, and in less than four minutes the surface was congealed, and the operation immediately performed; a double elliptical incision being carried through the integuments from the acromion process to within three inches of the elbow, and the parts included rapidly dissected away down to the muscles. Under this extensive flaying the patient experienced so little pain that he now declares that the most unpleasant part of the operation was the dressing, or rather the application of the adhesive strips.

"The congelation was so complete as to have solidified the adipose matter to such a degree that the knife felt as if passing through a mass of tallow. Not only the surface was blanched, but we have reason to believe that the effect of the application very materially lessened the amount of hemorrhage, inasmuch as this did not exceed a gill, whereas we had expected it to be very profuse. Ligatures were applied to three small vessels, although they bled very little, because it was deemed important to prevent even a small loss of blood under the circumstances. We regard this case as fully demonstrating the value of Arnott's plan of inducing local anæsthesia. It is now seven days since the operation was performed, and the patient's spirits, as well as general health, have so much improved as to allow him to return home. The wound seems to be doing remarkably well."

Case of Double Uterus; both Impregnated.—Dr. J. T. KANNON states (*New Orleans Med. and Surg. Journ.*, May, 1855) that he was called in consultation to see a woman who had been delivered early in the morning of a child. There were still all the external appearances of another child remaining, the womb being entirely empty, the placenta having come away. Owing to great prostration consequent upon the birth of the child, and the womb being empty, Dr. K. supposed that laceration had occurred, and the fœtus had escaped into the cavity of the abdomen.

He passed his hand into the empty womb, and found its walls solid, not the

least appearance of a rupture having occurred. The probability of a septum with a double womb suggested itself to his mind, and further examination revealed the correctness of this opinion. Looking upward and forward, near the edge of the symphysis pubis he found the os uteri of another womb, with the head of the fœtus resting directly on the pubis. There was no effort making on the part of the womb to expel the fœtus, and the usual remedies failing to bring on the pains, an attempt to turn and deliver was tried; but the peculiar position occupied by this second womb rendered it very difficult. As by this effort it was discovered that the fœtus was dead, he resorted to the crotchet, and brought away a child weighing six or seven pounds.

This lady was the mother of five children, but never previously had both wombs been impregnated at the same time. The septum was horizontal, with the os tincæ of the lower and posterior womb in the natural position, the anterior considerably elevated.

Dislocation of both Thigh Bones, with Laceration of Ilio-sacral Ligaments, and Displacement of the right Os Innominatum. By JNO. I. HODGEN, M. D., Professor of Anatomy in the Medical Department of the University of Missouri.—I was called on the 24th of July, 1854, with Drs. Norris and Seely, to see Robert Torry, and found that, in attempting to stop a pair of horses that were running with a wagon, he was thrown down, and in endeavouring to get out of the way of the wagon, had raised on his hands and knees, when the wheel striking him between the legs, behind, cutting the scrotum, and passing over his sacrum, crushed him to the earth.

We found him laying on his back, the right thigh flexed at right angles to the trunk, the knees thrown out, and the foot still further out. The right trochanter was felt with some difficulty, the head of the thigh bone was pretty easily detected over the ischium just above the tuberosity, probably resting against the spine, but not in the sacro-sciatic notch. The thigh could not be moved without giving great pain. The left thigh was more flexed than the right, and more widely separated from the median line; the foot was turned inward, the trochanter was more deeply situated than usual, the head of the bone was easily felt to the left of perineum, opposite the thyroid foramen, but not forced into it. This leg, like the other, could not be moved without great pain.

I fixed a sheet between his legs and over the hip for the purpose of counter-extension. I tied a sheet around the thigh above the knee, and to it fixed a pulley; then extended a clothes-line, that was also attached to the sheet, to another pulley placed above and in front of the patient, and back again to the pulley which was attached to the sheet. I then passed a towel on the inside of the thigh, close to the pelvis, and gave it to two assistants, and ordered an assistant to keep the pelvis from being raised by the pulleys. Dr. Norris gave the patient ether, and we began the extension. When the muscles were somewhat relaxed, I directed the assistants to draw the upper part of the thigh outward, and then diminishing gradually the extending force, the head of the femur came opposite the cotyloid cavity, and we allowed it to slip in by diminishing the lateral force and turning the foot outward. In five minutes the thigh was all right, and the limb moved freely and smoothly in any direction.

The right being treated in the same way, came pretty well into its place, and moved smoothly in its socket. I passed a broad bandage around both hips, and ordered strict rest. The patient appeared well, and was free from fever during the whole course of subsequent treatment.

On the second day, I observed that the right leg appeared longer than the left, and that the toe turned out. On the third the deformity increased; on the fourth there was a difference of 1½ inch in the length of the limbs. Coming to the conclusion that the bone must have slipped out, I resorted to the pulleys, and found that by making lateral extension, and then forcing the knee upward, I could bring the thigh apparently to its proper place.

Thinking that possibly the cotyloid ligament was lacerated, and a deposit had been made in the joint, and that the femur had gradually slipped down as described, I applied a splint along the leg extending above the hip, with a foot

piece, so that by applying a strap over the opposite hip I could keep the leg in its proper position. This answered the purpose admirably. But in a few days it became so painful from the pressure of the band upon the left hip that the patient's mother loosened it. On the day following I found the leg longer, on the following day still longer, and on the third day there was about $1\frac{1}{4}$ inch difference in the two legs. Again I put the limb in place, and applied the splint more loosely. He bore it pretty well this time, but the limb was still longer than the other by half or three-quarters of an inch. After some two weeks the splint appeared to be of no further service, and I removed it.

Meanwhile a brother of the boy, passing through Jacksonville, gave Dr. Prince a history of the case. Dr. Prince wrote requesting me to place the boy on his back, draw a tape from the top of the sternum over the umbilicus and pubis to a point midway between the internal maleoli, and then compare the length of limbs. I did so, and found the maleolus of the right limb one inch lower than that of the left, with the toe turned outward; still the motion of the limb was free, smooth, and easy, though not as extensive as natural. I then placed one limb of a carpenter's square on the tape as it was stretched from sternum to maleoli, and found the anterior superior spinous process of right ilium $\frac{1}{8}$ of an inch longer than that of the left. In the same way, I drew a line from the spinous process of the seventh cervical vertebra over the first lumbar to the first sacral spinous process, and found, by applying the square as before, the posterior superior spinous process of the ilium of the right side three-quarters of an inch lower than the left. This measure could not be so accurately taken, as the points could not be as definitely fixed. From the anterior superior spinous process of ilium to the trochanter major was $4\frac{1}{2}$ inches on both sides. From symphysis pubis to the lower part of the internal maleolus of the right side was 28 inches, left side 27 inches. From the anterior superior spinous process of the ilium to the lower part of the external maleolus of the right side was $31\frac{3}{8}$ inches; of the left side $31\frac{1}{4}$ inches. From the fissure between the nates to the surface of the skin covering the trochanter major of the right side $5\frac{3}{8}$ inches; left side $5\frac{1}{4}$ inches.

On the 1st of October, two months and a half after accident, the boy is going about without crutch, or cane, and limps very little.

In this case, the thighs were both certainly dislocated, and are now in their proper sockets. Now, how are we to account for the measurements above given?—*St. Louis Med. and Surg. Journ.*, Jan. 1855.

American Medical Association.—The Association held its eighth annual meeting in the city of Philadelphia, on the 1st, 2d, 3d, and 4th of May last. The meeting was, as usual, an agreeable one, and it was particularly so to the profession of Philadelphia, who were delighted to receive as guests so many of their brethren, and to have the opportunity of reciprocating the hospitalities so generously and freely extended to themselves at the previous meetings of the Association. Everything passed off harmoniously, and the only regrets we heard expressed were, at parting so soon with old friends, and at the short period allowed for enjoying the society of new ones. Each year we look forward with increased interest to these meetings, and to the enjoyment of the delightful associations which attend them.

We have given in the *Medical News* for last month a full account of the business transactions of the meeting, and need here only refer to it for the particulars.

GRADUATES OF THE UNIVERSITY OF PENNSYLVANIA, 1855.

At a Public Commencement, held March 31, 1855, in the Musical Fund Hall, the Degree of Doctor of Medicine was conferred by HENRY VETHAKE, LL. D., Provost, upon the following gentlemen; after which an Address was delivered by ROBERT E. ROGERS, M. D., Professor of Chemistry.

NAME.	RESIDENCE.	STATE.	SUBJECT OF ESSAY.
Arndt, Peter F.	Uhlersville,	Northampton, Pa.	Bubonocoele, Scrotocoele, Merocele.
Aston, Chas. C. J.	Lebanon,	Russell, Va.	Acute Pneumonia.
Baldwin, John M.	Winchester,	Frederick, Va.	Inguinal Hernia.
Bates, Julian	St. Louis,	Mo.	Gunshot Wounds.
Bates, Cicero M.	Cleveland,	Bradley, Tenn.	Pneumonia.
Bellamy, Eugene C.	Columbus,	Muscogee, Ga.	Yellow Fever.
Berry, Reuben B.	Memphis,	Shelby, Tenn.	Circumstances Modifying the Effects of Medicines.
Billingsley, Jas. A.	Franklin,	Fulton, Ark.	Intermittent and its Sequelæ.
Binion, Algernon H.	Fairfield,	Pickens, Ala.	Yellow Fever.
Blanton, Hugh L.	Farmville,	Cumberland, Va.	Physiology of Respiration.
Bolling, Robert, Jr.	Petersburg,	Dinwiddie, Va.	Dysentery.
Bonsall, Chas. T.	Trenton,	Mercer, N. J.	Pharmacy.
Bostick, Richard S.	Charleston,	S. C.	Cause of Yellow Fever.
Boykin, Wm. D.	Cooksville,	Noxubee, Miss.	Inguinal Hernia.
Bradley, Hugh	Claiborne,	Monroe, Ala.	Reparation of Tissue.
Bramham, Horace N.	Stony Point,	Albemarle, Va.	Uterine Hemorrhage.
Bredin, Stephen	Butler,	Butler, Pa.	The term, Puerperal Fever.
Browning, Jno. B.	Gainesville,	Sumter, Ala.	Dyspepsia.
Burg, Wm. H.	Manor,	Lancaster, Pa.	Pleuritis.
Burgess, S. Nelson	Statesburg,	Sumter Dist., S. C.	Anæsthetics.
Butt, Jas. N.	Nixonton,	Pasquotank, N. C.	Duties and Imperfections of a Physician.
Bynum, Drew	De Soto City,	Miss.	Inflammation.
Bynum, Jos. N.	Marlboro',	Pitt, N. C.	The Pinus Palustris and its Products.
Callender, John H.	Nashville,	Davidson, Tenn.	Predisposing Causes of Disease.
Caldwell, Wm. J.	Fayetteville,	Lincoln, Tenn.	Gunshot Wounds.
Carpenter, Jno. T.	Pottsville,	Schuylkill, Pa.	The Ectrotic Virtues of Iodine in Variola.
Carr, Geo. W. L.	Montgomery,	Montgomery, Ala.	Pleuritis.
Carritt, Thos. W.	Amherst,	Cumberland, N. S.	Chorea.
Cattell, Samuel G.	Salem,	Salem, N. J.	Coxalgia.
Chalfant, Aaron	New London,	Chester, Pa.	Anatomy of the Skin.
Chase, Benj. D.	Natchez,	Adams, Miss.	Morbid Dentition.
Clanton, Wm.	Grove Hill,	Warren, N. C.	Anat. and Physiol. of the Pancreas.
Cloman, Saml. T.	Hamilton,	Martin, N. C.	Enteric Fever.
Coke, Geo. H.	Williamsburg,	James' City, Va.	Puerperal Peritonitis.
Cole, Jas. R.	Newbern,	Craven, N. C.	Cholera Infantum.
Colhoun, J. Bingaman	Natchez,	Adams, Miss.	Inguinal Hernia.
Conrad, Henry F.	Holidaysburg,	Blair, Pa.	Physiology of Respiration.
Cook, William	Bordentown,	Burlington, N. J.	Hæmoptysis.
Coryell, Edward C.	Nichols,	Tioga, N. Y.	Compression of the Brain.
Cowell, Wm. H.	Currituck C. H.,	N. C.	Acute Dysentery.
Cox, David	Hertford,	Perquimans, N. C.	Gonorrhœa.

Advertisements.

NAME.	RESIDENCE.	STATE.	SUBJECT OF ESSAY.
Crump, Chas. C.	Franklin,	Williamson, Tenn.	Epidemic Dysentery
Daughtrey, Wm. H.	South Quay,	Nansemond, Va.	Febris Remittens.
Deininger, Chas. A.	Reading,	Berks, Pa.	Food.
Detweiler, Benj. H.	Trappe,	Montgomery, Pa.	The Physician's Duty.
Dillard, John J.	Gainesville,	Sumter, Ala.	Past, Present, and Future Condition of Medical Science.
Dougherty, Edward C.	Philadelphia,	Pa.	Abortion.
Dowdney, Samuel M.	Salem,	N. J.	Ergota.
Drewry, S. D.	Richmond,	Henrico, Va.	Menstruation.
Duncan, T. F.	Waynmanville,	Upson, Ga.	Niasmata.
Easley, Wm. D.	Cotton Hill,	Fayette, Va.	Retroflexion and Retroversion of the Uterus.
Edwards, Wm. B.	Green Bottom,	Shelby, Tenn.	Physiology of Circulation.
Eichelberger, Geo. M.	Hope Station,	Lexington, S. C.	Philosophy of Hygiene.
Ellington, John W.	Wentworth,	Rockingham, N. C.	Digitalis.
Field, Robert S.	Memphis,	Shelby, Tenn.	Reparation of Injuries.
Flinn, V. Howard	Luray,	Page, Va.	Dysentery.
Fort, Wiley K.	Tuskegee,	Macon, Ala.	Typhlitis Perityphlitis.
Fugate, Elbert S.	Dickensonville,	Russell, Va.	Acute Dysentery.
Gaston, J. Brown	Cedar Shoals,	Chester, S. C.	Repair of Fractures.
Gatchell, Elisha	Marlboro',	Chester, Pa.	Fibrin.
Gibbs, Alfred S.	New Bedford,	Bristol, Mass.	Physiology of Liver.
Gibson, Tully S.	Vicksburg,	Warren, Miss.	Plurality of Origin of the Races.
Godfrey, Jas. M.	Jones' Bluff,	Sumter, Ala.	Menstruation.
Gordon, Smith	Alexandria,	Rapides, La.	Physiology of Digestion.
Graham, Alfred H.	Beattie's Ford,	Lincoln, N. C.	Fractures of the Cranium.
Green, Benj. Thorp	Oak Hill,	Granville, N. C.	Dyspepsia.
Griffin, Peter E.	Society Hill,	Darlington, S. C.	Influence of Specialities on the Progress of Medicine.
Griffith, R. Eglesfeld	Philadelphia,	Pa.	Glucose.
Grim, Henry A.	Allentown,	Lehigh, Pa.	Alimenti Natura et Digestio.
Gulick, J. Wiley	Cheraw,	Chesterfield Dist. S. C.	Pathology of Inflammation.
Guthrie, Hugh R.	Bakerstown,	Alleghany, Pa.	Scarlatina.
Hammond, Ed. Spann	Silver Bluff,	Barnwell, S. C.	Catalysis.
Hammond, Harry	Silver Bluff,	Barnwell, S. C.	Modern Schools of Physiology in Relation to Life.
Hand, Joseph R.	Tuskegee,	Macon, Ala.	Urinary Calculi.
Harrison, J. Prosser	Richmond,	Henrico, Va.	Physiology of Respiration.
Hayes, J. Roberts	West Chester,	Chester, Pa.	Cholera in Chester Co. Almshouse, 1854.
Helm, J. Newton	Natchez,	Adams, Miss.	Yellow Fever.
Hendrie, Jas. D.	Doylestown,	Bucks, Pa.	Pseudo Membranous Inflammation of the Fauces.
Herbert, Josephus B.	Owen's Cross R'ds,	Williamson, Tenn.	Simple Acute Dysentery.
Heston, Eber	Brandywine Sp'gs,	New Castle, Del.	Dysentery.
Hester, Jas. H. W.	Oxford,	Granville, N. C.	Intemperance.
Hicks, Robert I.	Oxford,	Granville, N. C.	Adeps.
Hill, Jesse W.	Gainesville,	Sumter, Ala.	Menstruation.
Hill, John W.	Kenansville,	Duplin, N. C.	Febris Puerperarum.
Hinton, Gerard J.	Raleigh,	Wake, N. C.	Oleum Morrhuæ.
Hodge, Chas., Jr.	Princeton,	Mercer, N. J.	The Testis and its Secretion.

Advertisements.

NAME.	RESIDENCE.	STATE.	SUBJECT OF ESSAY.
Hoge, Milton	Belmont,	Belmont,	O. Enteric Fever.
Hook, Benj. P.	Coatesville,	Chester,	Pa. Puerperal Peritonitis.
Hopkins, St. George L.	Winchester,	Frederick,	Va. Hernia.
Horner, Edward H.	Weaversville,	Northampton,	Pa. Anæsthetics.
Hotchkin, Gurdon B.	Wallace,	Chester,	Pa. Physiological Effects and Therapeutics of Alcohol.
Houston, Geo. S. J.	Davidson College,	Iredell,	N. C. Epidemic Dysentery.
Hunt, Owen T.	Wild Haws,	Izard,	Ark. Remittent Fever.
Hunter, H. Holmes	Sunbury,	Gates,	N. C. Acute Peritonitis.
Hyndshaw, Frank J.	Plainfield,	Essex,	N. J. Cursus Medicus.
Jacobs, Samuel	Leasburg,	Caswell,	N. C. Inguinal Hernia.
Jeffreys, C. W. S.	Rolesville,	Franklin,	N. C. Dyspepsia.
Johnson, Wm., Jr.	Wilmington,	New Castle,	Del. Dysentery.
Johnston, Harvey G.	Pearisburg,	Giles,	Va. Enteric Fever.
Jones, S. Preston	Phoenixville,	Chester,	Pa. Conditions of Life.
Jones, Wm. Gaston	Currituck C. H.,		N. C. Menstruation.
Kaigler, H. Middleton	Cuthbert,	Randolph,	Ga. Enteric Fever.
Kelly, George I.	Henderson,	Granville,	N. C. Oleum Terebinthinæ.
Kitchen, John S.	Philadelphia,		Pa. Periostitis.
Lane, Geo. W.	Richland,	Giles,	Tenn. Physiology of Reproduction in the Mammalia.
Late, Wm. M.	Bridgeport,	Harrison,	Va. Dysentery.
Leet, Nathan Y.	Friendsville,	Susquehanna,	Pa. Amputation.
Lewis, Clarence L.	Nashville,	Davidson,	Tenn. Epidemic Cholera.
Lovett, Samuel	Bristol,	Bucks,	Pa. Poisons.
Ludington, Horace	Uniontown,	Fayette,	Pa. Rubeola.
Lyle, Abner J.	Fredonia,	Montgomery,	Tenn. The Excito-motor Function of the Spinal Cord.
Malany, Wm. S.	West Chester,	Chester,	Pa. Dysentery.
Manning, Jos. A.	Norfolk,		Va. The Therapeutic Uses of Opium.
Marriott, John C.	Eagle Rock,	Wake,	N. C. Epidemic Influence.
Martin, Walter F.	Weaversville,	Northampton,	Pa. Fracture of Clavicle.
Matthews, Thos. L.	Kosciusko,	Attala,	Miss. Uterine Hemorrhage.
McCabe, Francis	Wilmington,	New Castle,	Del. Pleurisy.
McDavitt M.	Memphis,	Shelby,	Tenn. Epidemic Cholera.
McDow, John R.	Gainesville,	Sumter,	Ala. The Negro and the White Man.
McDowell, Jas. H. T.	Richmond,	Henrico,	Va. Epidemic Cholera.
McGuire, Jas. M. G.	Theol. Seminary,	Fairfax,	Va. Opium.
McPheeters, Wm. A.	Natchez,	Adams,	Miss. Pneumonia.
McRac, Alex. L.	Brandon,	Rankin,	Miss. Miasm and its Diseases.
Miller, Wm. T.	Vidalia,	Concordia,	La. Physiology of Pain.
Montgomery, Daniel C.	Greenville,	Washington,	Miss. Acute Gastritis.
Montgomery, Wm. F.	Edwards' Depot,	Hinds,	Miss. Yellow Fever.
Mosely, Nathaniel R.	Philadelphia,		Pa. Water.
Muth, Reuben H.	Rehrersburg,	Berks,	Pa. Poisoned Wounds.
Nassau, Wm. W.	Lawrenceville,	Mercer,	N. J. Phthisis.
Nelson, Jas. M.	Holmesville,	Pike,	Miss. Remittent Fever.
Newberry, Milton	Whitemarsh,	Montgomery,	Pa. Gunshot Wounds.
Nichols, Geo. K.	Boston,	Suffolk,	Mass. Auscultation.
Nickleason, Joshua D.	Yardleyville,	Bucks,	Pa. Syphilis.
Nixon, Wm. Goodwyn	Lowndesboro',	Lowndes,	Ala. Enteritis.
Norris, Isaac, Jr.	Philadelphia,		Pa. Epidemic Cholera.
Nunez, Bernard C.	Guanabacoa,		Cuba. Differential Diagnosis between Simple and Virulent Blennorrhagia.
O'Brien, Rich. A.	Cincinnati,	Hamilton,	O. Compound Fractures.
Olds, E. Denny	Circleville,	Pickaway,	O. Blennorrhagia.

Advertisements.

NAME.	RESIDENCE.		STATE.	SUBJECT OF ESSAY.
Page, Wm. M.	Millwood,	Clark,	Va.	Calorification.
Phillips, J. Augustus	Pittsburg,	Alleghany,	Pa.	Endosmosis.
Phillips, John B.	Chesapeake City,	Cecil,	Md.	Vegetable and Animal Life.
Potter, Robert E.	Philadelphia,		Pa.	Epilepsy.
Ramsey, Wm. R.	Norristown,	Montgomery,	Pa.	Cholera.
Rogers, Thos. C.	Middletown,	New Castle,	Del.	The Negro—a Distinct Species.
Rousseau, Louis N. D.	Bristol,	Bucks,	Pa.	Premature Death of Infants.
Russell, John T.	Russellville,	Pope,	Ark.	Malaria.
Sayers, Samuel R.	Wytheville,	Wythe,	Va.	Cinchona Bark, Effects, &c.
Scholfield, Edwin	Philadelphia,		Pa.	Enteric Fever.
Seymour, Jos. E.	Sommerville,	Fayette,	Tenn.	The Physical and Vital Properties of the Blood.
Shobert, Reuben	Sloyersville,	Luzerne,	Pa.	Cholera Morbus.
Silliman, Henry R.	Pottsville,	Schuylkill,	Pa.	Oxalate of Lime in the Urine.
Smallwood, Whitmell P.	Hotel,	Bertie,	N. C.	Colo-Rectitis.
Smith, Jas. A.	Oak Hill,	Granville,	N. C.	Enteric Fever.
Spruill, Wm. R.	Windsor,	Bertie,	N. C.	Pneumonitis.
Stalnaker, Jno. W.	Lewisburg,	Greenbrier,	Va.	White Sulphur Water.
Stamm, Aug. Theod.	Berlin,		Prussia.	The Plague, its Generation and Disappearance.
Stokes, James	Germantown,		Pa.	The Toxicological Effects of Opium.
Stone, Thos. O.	Mobile,		Ala.	Lead Colic.
Stout, Abraham	Stouts,	Northampton,	Pa.	Cataract.
Sykes, Wm. E.	Aberdeen,	Monroe,	Miss.	Acute Peritonitis.
Tabb, Robert B.	Hampton,	Eliz. City,	Va.	The Stomach and its Office.
Talbutt, Chas. B.	Paris,	Bourbon,	Ky.	Scrofula.
Test, Zacheus	Richmond,	Wayne,	Ind.	Some of the Reasons why Remedies so frequently disappoint the expectation of the practitioner.
Thornton, Burwell	Merry Mount,	Warren,	N. C.	Scarlatina.
Tilden, Thos. W.	Greensborough,	Carolina,	Md.	Tight Lacing.
Trimble, Jos. W.	Concordville,	Delaware,	Pa.	Febris Rheumatica Inflammatoria.
Turner, Campbell L.	Cooperstown,	Otsego,	N. Y.	
Turner, Matthew	Washington C. H.,	Choctaw,	Ala.	Enteric Fever.
Twaddell, L. Henry	West Philadelphia,		Pa.	The Estrus Hominis.
Van Valzah, Jno. W.	Lewistown,	Mifflin,	Pa.	Jaundice.
Webb, Amos C.	Hickory Fork,	Gloucester,	Va.	Cold.
Wentz, Geo. S.	Whitemarsh,	Montgomery,	Pa.	Fractures.
Whitehead, Chris. B.	Kenansville,	Duplin,	N. C.	Oleum Terebinthinæ.
Wickersham, Swayne	Unionville,	Chester,	Pa.	Mission of Medicine.
Winborne, Robert H.	Murfreesboro',	Hertford,	N. C.	Enteric Fever.
Wood, Wm. R.	Plymouth,	Washington,	N. C.	Diabetes Mellitus.
Woodend, Wm. D.	Portsmouth,	Norfolk,	Va.	Acute Pericarditis.

At the Commencement held July, 1854, the Degree of Doctor of Medicine was conferred on

O'Brien, John

Halifax,

N. C. Acute Dysentery.

UNIVERSITY OF PENNSYLVANIA, MEDICAL DEPARTMENT.

NINETIETH SESSION (1855-56).

The Lectures will commence on Monday, October 8, and terminate in the middle of March ensuing.

Theory and Practice of Medicine,	By GEORGE B. WOOD, M. D.
Anatomy,	" JOSEPH LEIDY, M. D.
Materia Medica and Pharmacy,	" JOSEPH CARSON, M. D.
Chemistry,	" ROBERT E. ROGERS, M. D.
Surgery,	" HENRY H. SMITH, M. D.
Obstetrics, and the Diseases of Women and Children,	" HUGH L. HODGE, M. D.
Institutes of Medicine,	" SAMUEL JACKSON, M. D.

Clinical Instruction will be given at the Pennsylvania Hospital and Philadelphia Hospital.

Clinical Instruction in Medicine and in Surgery by the Professors of the Medical Faculty.

The Course of Dissections will be conducted by the Professor of Anatomy, aided by WM. HUNT, M. D., Demonstrator.

Amount of Fees for Lectures in the University,	\$105
Matriculating Fee (paid once only),	5
Hospital Fee,	10
Practical Anatomy,	10
Graduating Fee,	30

JOSEPH CARSON, M. D., *Dean of the Medical Faculty,*
332 Walnut Street, between Twelfth and Thirteenth Streets.

F. B. DICK, *Janitor, University.*

ST. LOUIS MEDICAL COLLEGE.

(Formerly the Medical Department of the St. Louis University.)

The Regular Lectures in this Institution will commence on the 1st day of November, 1855, and continue until March. A preliminary Course at the College, as also Clinical Lectures at the Hospital and the Dispensary, will be delivered without extra charge, during the month of October.

M. L. LINTON, M. D., Professor of the Principles and Practice of Medicine.

A. LITTON, M. D., Professor of Chemistry and Pharmacy.

CHARLES A. POPE, M. D., Professor of the Principles and Practice of Surgery and Clinical Surgery.

M. M. Pallen, M. D., Professor of Obstetrics and Diseases of Women and Children.

R. S. HOLMES, M. D., Professor of Physiology and Medical Jurisprudence.

W. M. MCPHEETERS, M. D., Professor of Materia Medica and Therapeutics.

CHARLES W. STEVENS, M. D., Professor of General, Descriptive, and Surgical Anatomy.

JOHN B. JOHNSON, M. D., Professor of Clinical Medicine and Pathological Anatomy.

E. H. GREGORY, M. D., Demonstrator of Anatomy.

The most ample opportunities for Clinical Instruction, both in Medicine and Surgery, are afforded free of charge in the St. Louis Hospital, as also in the City Hospital, the Marine Wards, and the O'Fallon Dispensary.

Fees for the entire Course,	\$105
Matriculation ticket (paid but once),	5
Dissecting ticket,	10
Hospital tickets	gratuitous.
Graduating fee,	20

Students or others, desiring further information, can either address the Dean, and he will forward them a descriptive pamphlet, or on arriving in the city, call upon him at his office, S. W. cor. of Tenth and Locust Streets, or on the Janitor at the College, corner Seventh and Myrtle Streets.

CHARLES A. POPE, M. D., *Dean.*

Advertisements.

JEFFERSON MEDICAL COLLEGE.

SESSION OF 1855-6.

The regular Course of Lectures will commence on Monday, the 8th of October, and continue until the first day of March. The ANNUAL COMMENCEMENT for conferring degrees will be held early in March.

ROBLEY DUNGLISON, M. D.,	Professor of Institutes of Medicine, &c.
ROBERT M. HUSTON, M. D.,	Professor of Materia Medica and Gen. Therapeutics.
JOSEPH PANCOAST, M. D.,	Professor of General, Descriptive, and Surg. Anatomy.
JOHN K. MITCHELL, M. D.,	Professor of Practice of Medicine.
THOMAS D. MUTTER, M. D.,	Professor of Institutes and Practice of Surgery.
CHARLES D. MEIGS, M. D.,	{ Professor of Obstetrics, and Diseases of Women and Children.
FRANKLIN BACHE, M. D.,	
	Professor of Chemistry.

ELLERSLIE WALLACE, M. D., Demonstrator of Anatomy.

Every Wednesday and Saturday in the month of October, and during the Course, Medical and Surgical cases will be investigated, prescribed for, and lectured on before the class. During the year ending March the first, 1855, *sixteen hundred and twenty-four* cases were treated, and *two hundred and sixty* operations performed in the presence of the class. Amongst these were many major operations—as amputation of the thigh, leg, and arm, extirpation of the upper jaw, parotid gland, mammae, &c., lithotomy and lithotripsy.

The Lectures are so arranged as to permit the student to attend the Medical and Surgical practice and Lectures at the Pennsylvania Hospital and the Philadelphia Hospital.

On and after the 1st of October, the dissecting rooms will be open, under the direction of the Professor of Anatomy and the Demonstrator.

FEES:

Matriculation, which is paid only once,	\$ 5 .
Each Professor, \$15,	105
Graduation,	30

The number of Students during the last Session was 565; and of Graduates 257.

ROBLEY DUNGLISON, M. D.,
Dean of the Faculty.

PHILADELPHIA, *June*, 1855.

TRANSYLVANIA UNIVERSITY—MEDICAL DEPARTMENT.

THE 39th Session will commence on the 1st Monday in November next, and continue for four months under the following Faculty, viz:—

- BENJAMIN W. DUDLEY, M. D., Emeritus Prof of Surgery.
 ROBERT PETER, M. D., Professor of Chemistry and Pharmacy, and Dean of Faculty.
 JAMES M. BUSH, M. D., Professor of Anatomy.
 SAMUEL M. LETCHER, M. D., Professor of Obstetrics and Diseases of Women and Children.
 HENRY M. SKILLMAN, M. D., Professor of General and Pathological Anatomy and Physiology.
 ETHELBERT L. DUDLEY, M. D., Professor of Surgery.
 WILLIAM S. CHIPLEY, M. D., Professor of Theory and Practice of Medicine.
 BENJAMIN P. DRAKE, M. D., Professor of Materia Medica and Therapeutics, and Medical Jurisprudence.
 JAMES M. BRUCE, M. D., Demonstrator of Anatomy.

Cost for the full course,	\$105
Matriculation and library fee,	5
Graduation fee,	25
Demonstrator's ticket,	10

Boarding, \$3 50 to \$4 00 per week.

LEXINGTON, KY., *July* 16, 1855.

ROBERT PETER, M. D.,
Dean.

UNIVERSITY OF NEW YORK.

MEDICAL DEPARTMENT—SESSION 1855-6.

The Lectures will commence on Monday, October 15, and be continued until the 1st of March following.

The Session of 1854-5 was attended by a class of 307 students, on 106 of whom the Degree of Doctor of Medicine was conferred.

FACULTY OF MEDICINE.

REV. ISAAC FERRIS, D. D., LL. D., *Chancellor of the University.*

VALENTINE MOTT, M. D., LL. D., Emeritus Professor of Surgery and Surgical Anatomy, and Ex-President of the Faculty.

MARTYN PAINE, M. D., LL. D., Professor of Materia Medica and Therapeutics.

GUNNING S. BEDFORD, M. D., Professor of Obstetrics, the Diseases of Women and Children, and Clinical Midwifery.

JOHN W. DRAPER, M. D., LL. D., Professor of Chemistry and Physiology.

ALFRED C. POST, M. D., Professor of the Principles and Operations of Surgery, with Surgical and Pathological Anatomy.

WILLIAM H. VAN BUREN, M. D., Professor of General and Descriptive Anatomy.

JOHN T. METCALFE, M. D., Professor of the Institutes and Practice of Medicine.

CHARLES E. ISAACS, M. D., Demonstrator of Anatomy.

GEORGE A. PETERS, M. D., Prosector to the Professor of Surgery.

ALEXANDER B. MOTT, M. D., Prosector to the Emeritus Professor of Surgery.

JOHN W. DRAPER, M. D., LL. D., President of the Faculty.

COURSE OF INSTRUCTION.—The Courses of Lectures given will be on Anatomy—general, descriptive, surgical, and pathological; Principles and Operations of Surgery; Materia Medica and Therapeutics; Institutes and Practice of Medicine; Obstetrics, the Diseases of Women and Children, with Clinical Midwifery; Chemistry and Physiology; Clinical Surgery; Clinical Medicine; Clinical Lectures on the Diseases of the Genito-Urinary Organs; Clinical Lectures on the Diseases of Women and Children; Clinical Lectures on Physical Diagnosis.

PRACTICAL ANATOMY.—The Legislature of New York having passed the Anatomical bill, the students will be furnished with increased facilities for the study of this department.

CLINICAL INSTRUCTION will be given at the New York Hospital and Bellevue Hospital; and the various Dispensaries of the city, including the Eye and Ear Infirmary, in which there are yearly more than 80,000 patients, will be open to the students, free of charge. In addition, there are held in the College building—also free of charge to the students of the University—five Cliniques each week, as follows:—

1. *An Obstetric Clinique for the Diseases of Women and Children* on every Monday, from 2½ to 4½ o'clock P. M., by PROF. BEDFORD. Since the organization of this clinique, in October, 1850, there have been actually presented to the classes of the University between eight and nine thousand cases of the most interesting diseases incident to women and children.

2. *A Surgical Clinique* every Tuesday, from 2½ to 4½ o'clock P. M., by PROF. MOTT. This clinique presents abundant opportunity for the study of surgical disease, and witnessing every variety of surgical operation.

3. *A Medical Clinique* every Wednesday, from 2½ to 3½ o'clock P. M., by PROF. METCALFE. Patients labouring under diseases of the lungs, heart, &c., are presented at this clinique, and full opportunity is given to the students for the study of physical diagnosis, &c.

4. *Surgical Clinique, with the Diseases of the Genito-Urinary Organs*, every Wednesday, from 3½ to 4½ o'clock P. M., by PROF. VAN BUREN. Besides its general surgical advantages, this clinique presents ample opportunity for the study of syphilitic diseases, strictures, &c.

UNIVERSITY OF NEW YORK, MEDICAL DEPARTMENT—CONTINUED.

5. *Surgical Clinique* every Saturday, from 11 o'clock A. M. to 1 o'clock P. M., by PROF. POST. During the session just closed, besides various operations of importance, Prof. Post performed in this clinique before his class, lithotripsy fifteen times, and lithotomy once, with entire success.

FEES FOR THE WINTER COURSE.

Full Course of Lectures	-	-	-	-	-	-	-	-	\$105
Matriculation Fee	-	-	-	-	-	-	-	-	5
For instruction by the Demonstrator	-	-	-	-	-	-	-	-	5
Graduation Fee	-	-	-	-	-	-	-	-	30

BOARDING.—Good board can be obtained in the vicinity of the College, at about \$4 per week.

N. B.—Students will please call, on their arrival, at the College, No. 107 East Fourteenth Street, and inquire for the Janitor, Mr. Polman, who will provide them with boarding-houses.

Letters may be addressed to PROF. DRAPER, President of the Faculty, University, New York.

NEW YORK, April 5, 1855.

MEMPHIS MEDICAL COLLEGE.

THIS Institution will open on the first of October, when each Professor will commence a Preliminary Course of Lectures, on subjects connected with his department, which for want of time cannot be fully taught in the regular course. This will occupy the whole month, and will be free of charge to the students and public.

The regular Lectures of this College will commence on the 1st of November, and will continue four months.

FACULTY.

AYRES P. MERRILL, M. D., Professor of Principles and Practice of Medicine.

JOHN MILLINGTON, M. D., Professor of Chemistry and Toxicology.

OLON BORLAND, M. D., Professor of Physiology and Pathology.

HOWELL R. ROBARDS, M. D., Professor of Surgery.

ARTHUR K. TAYLOR, M. D., Professor of Anatomy.

C. B. GUTHRIE, M. D., Professor of Materia Medica and Pharmacy.

HERSCHEL S. PORTER, D. D., Professor of Natural History and Geology.

LEWIS SHANKS, M. D., Professor of Obstetrics and Diseases of Women and Children.

EMMETT WOODWARD, M. D., Demonstrator of Anatomy.

The fee for the entire course is \$105, payable invariably in advance. Matriculation fee \$5; Graduating fee \$25; Anatomy and Dissection \$10, to be taken once before graduating. Dissecting Rooms and Museum opened 1st October.

The Chemical Course can be attended independently of the others, on payment of \$20.

Clinical Instruction is given twice a week at the Memphis Hospital, and a city Dispensary Clinique is held daily at the College at 8 o'clock in the morning, at which operations are performed, and upwards of one thousand cases come before the students, and are prescribed for and lectured upon daily by the Professors during the session.

The College possesses an ample Anatomical Museum and complete and superb Chemical apparatus, with extensive samples of Materia Medica, Minerals, &c.

Students desiring further information will address Prof. L. SHANKS, M. D., or on arriving in the city call on him at his office on Main Street, below Poplar.

L. SHANKS, M. D., *Dean*.

SUMMER AND WINTER COURSE OF LECTURES
AT THE
NEW YORK MEDICAL COLLEGE.

(*East Thirteenth Street, between Third and Fourth Avenues.*)

- HORACE GREEN, M.D., LL.D., Emeritus Professor of Theory and Practice of Medicine, and Professor of Diseases of the Respiratory Organs.
E. H. DAVIS, M.D., Professor of Materia Medica and Therapeutics.
B. FORDYCE BARKER, M.D., Professor of Midwifery and Diseases of Women and Children.
R. OGDEN DOREMUS, M.D., Professor of Chemistry and Medical Jurisprudence.
J. M. CARNOCHAN, M.D., Professor of the Principles and Operations of Surgery, with Surgical Pathology.
EDMUND R. PEASLEE, M.D., Professor of Physiology and Pathology.
EDWARD H. PARKER, M.D., Professor of General, Descriptive, and Microscopic Anatomy.
HENRY G. COX, M.D., Professor of Theory and Practice of Medicine, and of Clinical Medicine.
D. S. CONANT, M.D., Demonstrator of Anatomy.

The Summer Course of Lectures will commence on June 12, 1855, and will continue until October 12, when the Winter Course will be resumed. The Summer Course will be entirely Practical and Clinical in its character. Lectures will be delivered daily at the College or at the Hospital, by the following gentlemen:—

- On Therapeutics, by Prof. DAVIS.
On Puerperal Diseases, by Prof. BARKER.
On Practical Chemistry, by Prof. DOREMUS.
On the Surgical Operations of the Eye, by Prof. CARNOCHAN.
On the Microscope as applied to the Diagnosis of Disease, by Prof. PARKER.
On Diseases of Children, by Prof. COX.
On Auscultation and Percussion, by J. HANCOCK DOUGLASS, M.D.
On Medical Jurisprudence, by DAVID UHL, M.D.

Prof. CARNOCHAN, Surgeon-in-Chief, and Prof. COX, Physician-in-Chief to the Emigrants' Hospital, which contains about 1,500 beds, will visit the Hospital with the Students, and Clinical Lectures will be delivered there by them. Students of this College can also attend at the Bellevue Hospital, Blackwell's Island Hospitals, and the New York Hospital. At the College three Cliniques are held weekly—Surgical Clinique by Prof. CARNOCHAN, Medical Clinique by Profs. DAVIS and PARKER, and Clinique for Diseases of Women by Prof. BARKER. At the latter, every variety of Uterine Disease is exhibited to the class. Midwifery cases will be furnished to the Students to attend at the houses of the patients.

The Class will be divided into sections to attend at the office of Prof. GREEN, where they will have an unequalled opportunity to witness the treatment of the Diseases of the Respiratory Organs.

It will thus be seen that unsurpassed clinical opportunities are afforded for Students of this College.

Physical Diagnosis as applied to both Medical and Surgical Diseases, will receive especial attention during the Summer Course.

Students are especially urged to avail themselves of the gratuitous instruction in Chemical Analysis, which will commence three weeks in advance of the Winter Course; opportunities for this practical knowledge, it is believed, are not afforded in any other Institution.

Dissections will be commenced as early as practicable, and an abundant supply of material will be furnished at the lowest rates.

TERMS.

For Summer and Winter Course, inclusive	-	-	-	-	\$105 00
For Winter Course alone	-	-	-	-	105 00
For Summer Course alone	-	-	-	-	25 00

If Students attending the Summer Course afterward decide to attend the Winter Course, the \$25 00 paid will be placed to their credit on taking out the tickets for the Winter Course. Matriculation Fee, \$5 00.

Letters may be addressed to any member of the Faculty, or to

- R. OGDEN DOREMUS, M.D.,

Dean of the Faculty.

NEW YORK MEDICAL COLLEGE, May 1, 1855.

FISKE MEDICAL PRIZE QUESTION.

THE TRUSTEES OF THE FISKE FUND,

AT THE

ANNUAL MEETING OF THE RHODE ISLAND MEDICAL SOCIETY,

Held at Providence, June 6, 1855, announced that the premium of Fifty Dollars offered by them in 1854, for the best dissertation on the 1st question:—

I. CROUP,

Had been awarded to the author of the dissertation bearing the motto—

“Corn from the sheaves of Science, with stubble from my own garner.”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was ALBERT NEWMAN, M. D., of Attleborough, Massachusetts.

And that the Premium of One Hundred Dollars offered by them, upon the 2d question:—

II. THE EFFECT OF CLIMATE ON TUBERCULOUS DISEASE,

Had been awarded to the author of the dissertation bearing the mottoes—

“*In longis morbis solum mutare.*”

“In the cure of almost every disease, the removal of the producing cause should be the primary object.”

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Mr. EDWIN LEE, Member of the Royal College of Surgeons, of London, &c. &c.

They propose the following subject for 1856:—

“DOES PREGNANCY ACCELERATE OR RETARD THE DEVELOPMENT OF TUBERCLE OF THE LUNGS IN PERSONS PREDISPOSED TO THIS DISEASE.”

For the best Dissertation on this subject the Trustees will pay One Hundred Dollars.

Every competitor for a premium is expected to conform to the following regulations, viz:—

To forward to the Secretary of the Fiske Fund Trustees, on or before the 1st day of May, 1856, free of all expense, a copy of his dissertation, with a motto written thereupon, and also accompanying a sealed packet, having the same motto inscribed upon the outside, and his name and place of residence within.

Previously to receiving the premium awarded, the author of the successful dissertation must transfer to the Trustees all his right, title, and interest in and to the same, for the use, benefit, and behoof of the Fiske Fund.

Letters accompanying the unsuccessful dissertations will be destroyed by the Trustees, unopened, and the dissertations may be procured by their respective authors, if application be made therefor within three months.

ARIEL BALLOU, M. D., Woonsocket,

HIRAM CLEVELAND, M. D., North Providence,

ISAAC RAY, M. D., Providence,

S. AUG. ARNOLD, M. D., Providence, Secretary.

} Trustees.

MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

The annual Course of Lectures in this Institution will commence on the first Monday in November, on the following branches:—

Anatomy by	J. E. HOLBROOK, M. D.
Surgery by	E. GEDDINGS, M. D.
Institutes and Practice of Medicine by	S. HENRY DICKSON, M. D., LL. D.
Physiology by	JAMES MOULTRIE, M. D.
Materia Medica by	HENRY R. FROST, M. D.
Obstetrics by	THOS. G. PRIDLEAU, M. D.
Chemistry by	C. U. SHEPARD, M. D.
Demonstrator of Anatomy	F. T. MILES, M. D.
Prosecutor of the Professor of Surgery	T. F. M. GEDDINGS, M. D.

CLINICAL INSTRUCTION.

D. T. Cain, M. D., Physician to the Marine Hospital and Clinical Instructor, lectures twice a week on the diseases of that Institution.

H. W. Desaussure, M. D., Physician to the Hospital of the Almshouse, at which Lectures are delivered twice a week on Diseases, the diagnosis discriminated, and the student indoctrinated in their treatment.

The anatomical rooms are opened the latter part of October, and the dissections conducted daily under the direction of the Demonstrator. Much attention is directed to this department; the material being abundant, and illustrations of various character being afforded for acquiring a competent knowledge of this all-important branch of study.

HENRY R. FROST, M. D., *Dean.*

June, 1855.

SCHMITZ'S ANCIENT HISTORY.

BLANCHARD & LEA HAVE NOW READY

A MANUAL OF ANCIENT HISTORY, FROM THE REMOTEST TIMES TO THE OVERTHROW OF THE WESTERN EMPIRE, A. D. 476.

BY DR. LEONHARD SCHMITZ, F. R. S. E.,

RECTOR OF THE HIGH SCHOOL OF EDINBURGH.

With Copious Chronological Tables.

In one handsome royal 12mo. volume of four hundred and sixty-six pages.

The object of the author has been to supply the want of a clear and compendious *résumé* of Ancient History, exhibiting within a moderate compass the annals of the Asiatic and African, as well as of the Classical Nations, as elucidated by the investigations of modern explorers and critics. The vast body of new information which has been accumulated of late years has thrown a new light over many important periods, and renders a work like the present of much importance to the scholar and private reader, as well as admirably adapted for use in colleges and academies. Availing himself of the successful labours of the more recent investigators, the author has not confined himself to the dry details of battles and sieges, but has endeavoured throughout to give a clear and accurate description of the social and political condition of the various nations, tracing the progress of their civilization, the causes of their successive preponderance, the influences which they have exerted, and the reasons of their decline. The name of the author is sufficient guarantee of the accuracy of the work, while the philosophic and democratic spirit which pervades it, and the easy and perspicuous flow of its narration, cannot fail to render it a favourite with those for whom it is intended.

The work is all that, and more than, he represents it to be. We have closely examined such portions of the history as we are best acquainted with, and have been unable to detect a single error of fact. The general accuracy of the work, therefore, seems to us unimpeachable, while the diction is concise, lucid, fluent, and vigorous. The chronological table appended is

comprehensive and well arranged, and the minute index added to this renders the volume one of the most valuable historical works of reference ever printed. We shall be surprised if it does not become a popular text-book in our high-schools and colleges, as well as a favourite volume with intelligent general readers.—*N. Y. Commercial Advertiser.*

TO READERS AND CORRESPONDENTS.

The following works have been received:—

Yellow Fever, considered in its Historical, Pathological, Etiological, and Therapeutical Relations. Including a Sketch of the Disease as it has occurred in Philadelphia from 1699 to 1854. With an Examination of the Connection between it and the Fevers known under the same name in other parts of Temperate, as well as in Tropical Regions. By R. LA ROCHE, M. D., Member of the American Medical Association, of the American Philosophical Society, etc. etc. In two volumes. Philadelphia: Blanchard & Lea, 1855.

Elements of Medicine: A Compendious View of Pathology and Therapeutics, or the History and Treatment of Diseases. By SAMUEL HENRY DICKSON, M. D., LL. D., Professor of the Institutes and Practice of Physic in the Medical College of the State of South Carolina. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Manual of Pathological Anatomy. By CARL ROKITANSKY, M. D., Curator of the Imperial Pathological Museum, and Professor at the University of Vienna, etc. Translated from the last German edition, by WM. EDWARD SWAINE, M. D., EDWARD SIEVEKING, M. D., CHAS. H. MOORE, and GEO. E. DAY, M. D., F. R. S. Four volumes in two. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

✎ We hope in our next number to be able to give some notice of this great storehouse of pathological knowledge.

A Practical Treatise on the Diseases of the Eye. By WILLIAM MACKENZIE, M. D., Surgeon Oculist in Scotland in-Ordinary to her Majesty; Lecturer on the Eye in the University of Glasgow, and one of the Surgeons to the Glasgow Eye Infirmary. To which is prefixed an Anatomical Introduction explanatory of a Horizontal Section of the Human Eyeball. By THOMAS WHARTON JONES, F. R. S., Professor of Ophthalmic Medicine and Surgery in University College, London, and Ophthalmic Surgeon to the Hospital. From the fourth revised and enlarged London edition, with Notes and Additions. By ADDINELL HEWSON, A. M., M. D., one of the Surgeons to Wills' Hospital for Diseases of the Eye, etc. etc. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Manual of Clinical Medicine and Physical Diagnosis. By T. H. TANNER, M. D., Licentiate of the Royal College of Physicians, etc. etc. To which is added the Code of Ethics of the American Medical Association. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

The Diseases of the Heart and the Aorta. By WILLIAM STOKES, Regius Professor of Physic in the University of Dublin, etc. etc. Philadelphia: Lindsay & Blakiston, 1855. (From the Publishers.)

The Cause and Prevention of Yellow Fever, contained in the Report of the Sanitary Commission of New Orleans. By E. H. BARTON, A. M., M. D., Chairman of the Sanitary Commission, etc. etc. Philadelphia: Lindsay & Blakiston, 1855. (From the Publishers.)

The Physician's Visiting List, Diary, and Book of Engagements, for 1856. Philadelphia: Lindsay & Blakiston. (From the Publishers.)

Clinical Lectures on the Diseases of Women and Children. By GUNNING S. BENFORD, M. D., Professor of Obstetrics, the Diseases of Women and Children, and Clinical Midwifery in the University of New York. New York: S. S. & W. Wood, 1855. (From the Publishers.)

Medical Lexicon of Modern Terminology; being a Complete Vocabulary of Definitions, including all the Technical Terms employed by Writers and Teachers of Medical Science at the present day. Third edition. By D. MEREDITH REESE, M. D., LL. D., Resident Physician of Bellevue Hospital, etc. etc. New York: S. S. & W. Wood, 1855. (From the Publishers.)

The Mineral and Thermal Springs of the United States and Canada. By JOHN BELL, M. D., author of "Baths and Mineral Waters;" "Baths and the

Watery Regimen;" "Lectures on the Practice of Physic," etc. etc. Philadelphia: Parry & McMillan, 1855. (From the Publishers.)

Clinical Lectures on Paralysis, Disease of the Brain, and other Affections of the Nervous System. By ROBERT BENTLEY TODD, M. D., F. R. S., Physician to King's College Hospital. Philadelphia: Lindsay & Blakiston, 1855. (From the Publishers.)

Memoirs of the Life and Services of Daniel Drake, M. D., Physician, Professor, and Author, with Notices of the Early Settlement of Cincinnati and some of its Pioneer Citizens. By EDWARD D. MANSFIELD, LL. D., author of "American Education," etc. Cincinnati: Applegate & Co., 1855. (From the Publishers.)

History of the American Medical Association, from its Organization up to January, 1855. By N. S. DAVIS, M. D., Professor of Principles and Practice of Medicine and Clinical Medicine in Rush Medical College, etc. etc. To which are appended Biographical Notices, with Portraits, of the Presidents of the Association and of the Author. Edited by S. W. BUTLER, M. D. Philadelphia: Lippincott, Grambo & Co., 1855. (From the Publishers.)

A Treatise on Epidemic Cholera. By HORATIO GATES JAMESON, Sen., M. D., Member of the Medical and Chirurgical Faculty of Maryland, etc. Philadelphia: Lindsay & Blakiston, 1855. (From the Publishers.)

Transactions of the State Medical Society of the State of New York. Transmitted to the Legislature, February 13, 1855. Albany, 1855. (From the Society.)

Transactions of the Medical Association of South Central New York, at the Ninth Annual Meeting, held at Elmira, June 5, 1855. Elmira, 1855. (From G. W. Bradford, M. D.)

Sixty-Eighth Annual Report of the Regents of the University of the State of New York. Made to the Legislature, March 1, 1855. Albany, 1855. (From Dr. T. R. Beck.)

Report of the State Librarian, relating to the Registration of Births, Marriages, and Deaths, for the year ending December 31, 1854. Printed by order of the Legislature. Hartford, 1855.

Ninth Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditures, and Condition of the Institution up to January 1, 1855, and the Proceedings of the Board up to February 24, 1855. (From the Smithsonian Institution.)

Report on the Minerals and Mineral Waters of Chile. By J. LAWRENCE SMITH, Professor of Chemistry in the Medical Department of the University of Louisville. (From the Author.)

The Great Tree on Boston Common. By J. C. WARREN, M. D., President of the Boston Society of Natural History. Boston, 1855. (From the Author.)

Observations on the Life, Disease, and Death of John Hunter, in Elucidation of the Nature and Treatment of Gout and Angina Pectoris; being the Oration delivered before the Hunterian Society, at its Thirty-sixth Anniversary. By JOSEPH RIDGE, M. D., Fellow of the Royal College of Physicians, etc. etc. London, 1855. (From the Author.)

An Address delivered on the occasion of the Inauguration of the New South Building of the New York Hospital, on the 18th of April, 1855. By JOSEPH M. SMITH, M. D., Senior Physician of the New York Hospital, etc. etc. New York, 1855. (From the Author.)

Search out the Secrets of Nature. The Annual Discourse before the Massachusetts Medical Society, at Springfield, June 27, 1855. By AUGUSTUS A. GOULD, M. D. Boston, 1855. (From the Author.)

Pustule Maligne. An Inaugural Essay submitted to the examination of Alexander H. Stevens, M. D., LL. D., President, and the Trustees and Professors of the College of Physicians and Surgeons of the University, for the Degree of Doctor in Medicine. By DANIEL WADSWORTH WAINWRIGHT, of New York City. New York, 1855.

Choroiditis Glaucoma. Clinical Lectures on some of the Diseases of the Eye, delivered at the New York Medical College, 1855. By ISIDOR GLUCK, M. D. New York, 1855. (From the Author.)

On the Hygienic Condition of the Survivors of Ovariectomy. By J. DEANE, M. D. From the "Communications of the Massachusetts Medical Society." Boston, 1855. (From the Author.)

Sixteenth Annual Announcement and Catalogue of the Baltimore College of Dental Surgery. Session of 1855-'56. Baltimore, 1855.

Circular and Catalogue of the Trustees, Faculty, and Students of Savannah Medical College, Announcement of Lectures—Session of 1855-'56, together with the Address delivered at the Commencement, March 15, 1855, by HENRY WILLIAMS, Esq. Savannah, 1855.

The following Journals have been received in exchange:—

Association Medical Journal. Edited by JOHN ROSE CORMACK, M. D. May, June, July, August, 1855.

Dublin Medical Press. June, July, August, 1855.

Medical Times and Gazette. May, June, July, August, 1855.

Dublin Hospital Gazette. May, June, July, August, 1855.

Monthly Journal of Medicine, June, July, August, 1855.

The British and Foreign Medico-Chirurgical Review. July, 1855.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. H. RANKING, M. D., and C. B. RADCLIFFE, M. D. January to June, 1855.

The Retrospect of Medicine. Edited by W. BRAITHWAITE. January to June, 1855.

Edinburgh Medical Journal. July and August, 1855.

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. July, 1855.

The Indian Annals of Medical Science. April, 1855.

The Glasgow Medical Journal. July, 1855.

The Dublin Quarterly Journal of Medical Science. August, 1855.

The Medical Chronicle. Edited by WILLIAM WRIGHT, M. D., and D. C. MACCALLUM, M. D. June, July, August, 1855.

Le Moniteur des Hôpitaux. Rédacteur en chef, M. H. De CASTELNAU. May, June, July, and August, 1855.

Archives de Physiologie, de Thérapeutique, et de Hygiène. Sous la direction de M. BOUCHARDAT. October, 1854.

Gazette Médicale de Paris. June, July, and August, 1855.

Revue de Thérapeutique Medico-Chirurgicale. Par A. MARTIN LAUZER. June, July, and August, 1855.

The Boston Medical and Surgical Journal. Edited by Drs. SMITH, MORLAND, and MINOT. July, August, and September, 1855.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. July, August, September, 1855.

The American Medical Gazette. Edited by D. MEREDITH REESE, M. D. July, August, September, 1855.

The Western Journal of Medicine and Surgery. Edited by LUNSFORD P. YANDELL, M. D. June, July, and August, 1855.

The New York Journal of Medicine. Edited by SAMUEL S. PURPLE, M. D., and STEPHEN SMITH, M. D. July and September, 1855.

The New Orleans Medical and Surgical Journal. Edited by BENNET DOWLER, M. D. July and September, 1855.

The Medical Reporter. Published by the Chester and Delaware County Medical Societies. July, 1855.

New York Medical Times. Edited by H. D. BULKLEY, M. D., and J. G. ADAMS, M. D. July, August, and September, 1855.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D. July and September, 1855.

American Journal of Pharmacy. Edited by WILLIAM PROCTER, Jr. June and September, 1855.

The American Journal of Science and Arts. Conducted by Profs. B. SILLIMAN, B. SILLIMAN, Jr., and J. A. DANA, in connection with Profs. GRAY and AGASSIZ, and Dr. GIBBS. July and September, 1855.

Buffalo Medical Journal. Edited by SANDFORD B. HUNT, M. D. July, August, and September, 1855.

The New Jersey Medical Reporter. Edited by S. W. BUTLER, M. D. July, August, and September, 1855.

The Medical Counsellor. Edited by R. HILLS, M. D. July, August, and September, 1855.

The Northwestern Medical and Surgical Journal. Edited by N. S. DAVIS, M. D., and H. A. JOHNSON, M. D. July, August, 1855.

Charleston Medical and Surgical Journal. Edited by C. HAPPOLDT, M. D., assisted by D. J. CAIN, M. D., and F. PEYRE PORCHER, M. D. July and September, 1855.

The American Medical Monthly. Edited by EDWARD H. PARKER, M. D. July and September, 1855.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M. D., and PAUL F. EVE, M. D. July, August, and September, 1855.

The Stethoscope. Edited by G. A. WILSON, M. D., and R. A. LEWIS, M. D. July, August, and September, 1855.

New Orleans Medical News. Edited by Drs. CHOPPIN, BEARD, and BRICKELL. July, August, and September, 1855.

New York Dental Recorder. Edited by C. W. BALLARD, D. D. S. July, August and September, 1855.

The New Hampshire Journal of Medicine. Edited by GEO. H. HUBBARD, M. D., and CHARLES BELL, M. D. July, August, and September, 1855.

The American Journal of Dental Science. Edited by CHAPIN A. HARRIS, M. D., D. D. S., and A. SNOWDEN PIGGOT, M. D. July, 1855.

St. Louis Medical and Surgical Journal. Edited by Drs. LINTON, MCPHEETERS, MOORE, and ALLEN. July and September, 1855.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M. D., and E. ANDREWS, M. D. July, August, and September, 1855.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. July, 1855.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D., and HENRY ROSSIGNOL, M. D. July, August and September, 1855.

The Western Lancet. Edited by T. WOOD, M. D. July, August and September, 1855.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of Iowa University. June and July, 1855.

Memphis Medical Recorder. Edited by A. P. MERRILL, M. D. July, 1855.


The Virginia Medical and Surgical Journal. Edited by Drs. J. B. McCaw, J. F. PEEBLES, and G. A. OTIS. August and September, 1855.

The Southern Journal of the Medical and Physical Sciences. Edited by Drs. JONES, WOOD, KING, and CURREY. May and July, 1855.

Atlanta Medical and Surgical Journal. Edited by Drs. LOGAN and WESTMORELAND. September, 1855.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to ISAAC HAYS, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London*; or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

ALL REMITTANCES OF MONEY, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

 The advertisement-sheet belongs to the business department of the Journal and all communications for it should be made to the publishers.

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OF THE

AMERICAN JOURNAL

OF THE

MEDICAL SCIENCES.

NO. LX. NEW SERIES.

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THE
AMERICAN JOURNAL
OF THE MEDICAL SCIENCES
FOR OCTOBER 1855.

ART. I.—*An Inquiry into the Pathological Anatomy of Acute Pneumonia.*
By J. DA COSTA, M. D., of Philadelphia. (With a plate.)

THERE is, perhaps, no subject within the wide range of medical pathology which has received more attention than Pneumonia. The frequency of its occurrence, the interest centred in its therapeutics, and the facilities which modern research has supplied to its diagnosis, have produced a larger number of exact clinical observations than most other diseases can lay claim to. Yet, notwithstanding the care with which its study has been pursued by successive medical generations, there are some points connected with it, vitally concerning its correct appreciation, which must still be regarded as unsettled.

That pneumonia, as a disease, was known to the ancients, cannot admit of doubt; but how far they understood its anatomical characters, and separated its symptoms from those of other acute thoracic affections, is not so easily decided. More especially does the distinction between it and pleurisy seem to have been to them a matter of difficulty, as may be gathered from the writings of Hippocrates, whose descriptions of the latter disease might with equal, if not with more, propriety, be made applicable to pneumonia. Aretæus, too, usually so clear a delineator of disease, is not more fortunate in this respect, and is exceedingly obscure when speaking of "peripneumonia," in the description of which he evidently includes morbid appearances and symptoms belonging to pleurisy. This confusion with regard to the two diseases lasted until the present century, since it can scarcely be said that the distinction which Sydenham introduced, of a *true* and a *false* peripneumonia, cleared up the anatomical characters of either of the affections in question. His own writings, indeed, bear ample testimony to the fact that, whilst he referred the term "peripneumonia notha," or *false* peripneumonia, to a disease at present known to us as bronchitis, or perhaps as bronchopneumonia, under the term "peripneumonia vera," he still confounded both pleurisy and pneumonia. Morgagni, Valsalva, Stoll, and even Hoffmann, followed him in this error, as

becomes sufficiently apparent from a passage in the works of the latter, in which we are informed that the difference between the two affections consists in pneumonia occupying the deeper portion of the lung, pleurisy generally the more superficial, and the pleura.¹

Thus, notwithstanding the many observations made, and the time that had been spent on the study of pneumonia, the present century found the exact pathology in a very unsettled condition, and it required some of the master minds of this age to cut through the knot, tightened by medical controversy, and point out the true path on which observers were to proceed. Amongst the first who clearly distinguished pneumonia from pleurisy, we find Bichat leading the way by his anatomical researches. Yet to Laennec—great in pathology as in diagnosis—belongs the credit of having most clearly and satisfactorily investigated the pathological lesions, and, by careful anatomical examinations, as well as by a comparison of the results thus obtained with the stethoscopic signs, of having detached pneumonia, as a separate disease, from the other affections with which it had up to then been classed. Laennec divided pneumonia into three principal stages: the stage of engorgement—lung of livid hue, heavier, but still crepitant; the stage of red hepatization—marked by loss of crepitous feel, with increased consistence and weight; and, lastly, purulent infiltration, or the formation of pus in the inflamed lung. Besides these most prevalent anatomical conditions, we find in his work descriptions of rarer forms of the disease: as of chronic pneumonia; carnification of the lung; and even allusions to lobular pneumonia, the true nature of which, and its connection with bronchitis and collapse of the air-cells, form part of those important subjects which modern pathological research is now so brilliantly elucidating.² To inquire more minutely than this into the further historical development of the exact anatomy of pneumonia, would be here out of place, and is, indeed, in so far unnecessary, as the terms and distinctions of Laennec (if we substitute gray hepatization for purulent infiltration) are the ones still most generally adopted; and as, further, the opinions of the more distinguished pathologists of the day, and the additions made by them to our knowledge of the affection, will have to be considered in discussing the subject of this paper—the changes which take place in the lung in acute lobar pneumonia.

First Stage, or Engorgement.—We have, then, following Laennec, to admit, as the first stage of pneumonia, the “engouement” or engorgement. A lung in this condition is of a livid hue, less cohesive, less elastic, and less crepitous; it has lost its spongy character, and from it transudes, when pressed, a frothy serum. If a portion be subjected to a minute examination, the following

¹ “Vera autem pleuritis sedem suam habet in ipsa pleura, quae intus thoracis cavitatem investit, et plerumque in consensum trahit annexos subinde pulmones, et peripneumonia ipsam pulmonem substantiam occupat.”—*Hoffmann, Op. Omn.*, tom. i. cap. iii. p. 294; Geneva, 1748.

² See especially the valuable contributions of Dr. Gairdner, in the *British and Foreign Medico-Chirurgical Review*, 1853 and 1854, on Collapse of the Lung, Emphysema, etc.

appearance will be found: The fibrous walls of the air-vesicles (Fig. 1, *a*) are perfectly distinct, but seem to be more easily separated, and inclose, in some parts, a slightly granular matter. The epithelial cells are detached from the sides of the air-vesicles, and from those of the minute bronchi; their nuclei are unaltered, but the cell-contents are more granular (Fig. 1, *b*) than in normal epithelium. The minute capillaries are gorged with blood; yet the separate corpuscles in them are not distinguishable. Few of the latter, also, contrary to what might be expected, can be seen lying in the air-cells.

The congested bloodvessels soon permit a fluid to transude into the air-cells and their walls. Thin, frothy, and coagulable by heat, it presents, at the onset of the inflammation, but slight differences from the serum effused in ordinary congestions and œdema. As the morbid process advances, and the spongy texture of the lung becomes less distinct, the air-vesicles contain more granules; the serous fluid becomes of greater density, and shows distinct blood-corpuscles and new elements in the shape of small nucleated cells and exudation-corpuscles. (Fig. 1, *c*.) The effusion, indeed, commences, within the lung, to present the peculiar characteristics which we are in the habit of regarding, when expectorated, as the pathognomonic, rusty-coloured sputum of pneumonia. This completes the picture of a lung in the first stage, or the so-termed stage of engorgement. Before proceeding to describe the subsequent development of the exudation, it becomes incumbent upon us to examine whether the ordinary appearances, or the minute structure of lungs in this condition, present any signs which may fairly be considered to be undeniable evidences of inflammation.

Characteristics of the First Stage.—Inflamed lung, as we have seen, exhibits to the eye a difference in colour and friability from healthy lung, as well as an augmented serosity. That the *colour*, although darker and more violet than that of the normal tissue, affords no certain distinguishing mark between the cadaveric and other engorgements, is pretty generally conceded; but this is not the case with regard to the increased *friability* of the lung, on which, indeed, some pathologists have laid, and still continue to lay, great stress. Thus, Lallemand regards the degree of friability as the only test by which an engorgement of the tissue, occurring shortly before death, can be recognized from that of an inflammation which has lasted for a day or two. In the first edition of his *Clinique Médicale*, Andral advocates the same view: “*Pour peu que l'un constate une plus grande friabilité de ce tissu, il doit être regardé comme enflammé.*” Further observations have, however, led him to modify his views; for, in the second edition of the same work, he states that more recent researches have convinced him that the increased friability of the pulmonary tissue may also be owing to the simple fact of the accumulation of blood, either during life or after death. Observations instituted with regard to this point, and made more especially on well-marked œdematous lungs, have led me to the latter conclusion; although it seemed to me, in many instances, that the lungs in which the congestion might fairly be considered to be *post mortem*, were wanting in that amount of friability which is found in lungs that have

been engorged for some time before death, or which ordinarily belongs to inflamed tissue.

Exact observations are wanting in regard to the character of the *serous* exudation, in the stage under consideration. Conditions of lung bearing a close resemblance to this first stage, are the engorgement of the tissue and the effusion of serum characterizing the disease known as "œdema," as well as the simple congestions occurring both before and after death, which may, perhaps, strictly speaking, not be designated as œdema, but which also give rise to slight exudations into the pulmonary structure. A question, therefore, of great interest arises here. Are there any signs which may serve to distinguish the effusion of the first stage of pneumonia from the ordinary serous effusion occurring in œdema? and, also, from the exudation observed in simple congestion of the lung? A solution of this inquiry is the more important, from the fact already stated, that the colour and friability furnish at the best but doubtful tests. Now, to arrive at a definite conclusion, with regard to this point, I have for some time past taken every opportunity of examining congested lungs, both with the unassisted eye and with the aid of the microscope, and of contrasting them with the appearances recorded of lungs in the first stage of inflammation. My observations have led to the following conclusions. To the unassisted eye, no difference between the effused fluids is perceptible at the onset of the inflammation; but as the pneumonia advances, the secretion becomes thicker and redder, until it ultimately approaches to the well-known rusty-coloured exudation. Neither are there, at first, microscopically, any very material distinctions. The fluids of both contain a few granules, whilst the structure of the lung is exactly similar, excepting that the walls of the air-vesicles are generally more granular in inflamed than in simply congested portions of lung.

In a more advanced stage, however, of inflammatory engorgement, the appearances are decidedly changed. We find a much larger number of exudation-corpuscles, and the epithelial cells seem to contain, on the whole, more granules, partly fat molecules, partly, perhaps, an imbibition of the inflammatory products. Again, there are elements in lungs which have been in a state of hyperæmia for some length of time previous to death, both in the tissue and in the exudations, which are not met with to any extent in the acutely inflamed organs, viz: peculiar large flakes and cells (see Fig. 2, *b*, *c*), inclosing blood-corpuscles, in numbers varying from one to twenty. These "blood-corpuscle-holding cells"—to use the term employed for similar bodies seen frequently in the spleen—degenerate readily into masses of pigment, which are seen to lie partly between, but most generally on the walls of the air-vesicles (*d*). The colouring matter of the blood which transudes from the broken-up corpuscles, imparts a uniform yellowish stain to these flakes or cells, as also to the surrounding epithelium. Many of the cells themselves are observed to be filled with a reddish or yellowish granular pigment (Fig. 2, *c*), not soluble in acetic acid, but destroyed by a strong solution of potassa; or there may be, when the

pigmental degeneration has been fairly established, no cell-walls at all visible, but merely accumulations of a reddish-brown pigment, or one of various colours of transition from that to an intense black.

In very acute congestions, and in cadaveric engorgements, these cells and the fresh pigment are mostly absent. In the much congested lung of a man who had been recently drowned, I met neither with these cells nor any new-formed pigment; whilst both I have found to be almost invariably present in the chronic engorgements of the pulmonary tissue accompanying cardiac affections. As occasional elements in these chronic congestions, I have met with granule-cells and a fatty epithelium (see Fig. 2, a); but never do I recollect to have observed them in what might with justice be presumed to be *post-mortem* engorgements. The granular cells themselves are probably the normal epithelium in a stage of fatty degeneration.

From these observations, therefore, it may be concluded that the number and variety of exudation-corpuscles, and perhaps the absence of blood-corpuscles undergoing peculiar pigmental changes, alone afford any evidences of acutely-inflamed pulmonary tissue.

The exudation of œdema, as well as that in inflammatory engorgement of the lungs, is known to be coagulable by heat and acids; the other *chemical* changes which accompany these conditions have, as far as I am aware, not been touched upon. My inquiries on this subject have been, up to the present, nearly altogether confined to one element—the amount of fat present; and have resulted in showing that in congested, as well as in inflammatory engorged lungs still permeable to air, this principle is invariably increased. The normal amount of fat in a healthy lung is stated by M. Nalatis Guillot (*Compt. Rendus*, 1847), not to exceed 6 per cent. My examinations, conducted with reference to this point, have yielded even less than that—not over 4 per cent. in proportion to the dried, or 0.65 to the wet tissue; whilst in primarily congested lungs, when dried, it ranged from 7 to 9 per cent. Subjoined is a table of some cases examined.

Age of patient.	Lung weighed; wet.	Lung weighed; dried.	Mass analyzed.	Amount of fat extracted. ¹	Per cent. of fat in wet lung.	Per cent. of fat in dried lung.	Disease patient died of.
	grs.	grs.	grs.	grs.			
40	175.5	32	32	2	1.13	6.25	Chronic pleurisy.
38	111.65	11.75	11.50	1	0.896	8.695	Tubercular meningitis.
48	56	10.3	10	.7	1.25	7	By inhaling hot air.
42	155	22	22	1.76	1.135	8	Drowned.
23	162.5	27.25	27.25	2.25	1.384	8.25	Disease of mitral valve.
31	65.5	10.5	10	.85	1.29	8.5	Phthisis.
45	658	658	4.40	0.668	Chronic pleurisy.
2	380	380	5.6	1.47	Meningitis.

¹ The fat was extracted by repeated treatment with ether. It need scarcely be stated that parts, entirely free from tubercular or other morbid changes, were selected for analysis. Several of the cases presented well-marked signs of congestion of the lung for several days before death.

Now, as the engorgement was not cadaveric in any of these cases, this table proves a marked increase of fat in hyperæmia of the lung occurring some time before death. My observations with reference to the increase of this substance, in the first stage of pneumonia, are as yet too limited to permit of my publishing here a comparative table; but the cases examined lead me to the belief, that the accumulation of fat occurs in this stage even to a much larger extent than in simply congested lungs. In one well-marked specimen, in which the portion of the lung analyzed had commenced to lose its spongy character, but still floated in water, although some parts in its vicinity were already markedly consolidated, the amount of fat present in 360 grains of wet tissue was 7.85 grains, bearing the proportion of 2.18 per cent. to the wet, and 15 per cent. to the dried structure; a much larger percentage than was obtained from any congested lung examined.

A recognizable condition of the pulmonary tissue is stated, by a most distinguished observer, to *precede* the stage of engorgement of Laennec. Dr. Stokes describes, as the true first stage, an "irritation" of the lung, in which the pulmonary tissue is drier, and of a bright vermilion colour. However convenient, in many respects, it might be to admit such a morbid condition, pathological demonstrations have not borne out this author's view. Nearly all eminent pathological anatomists of the day deny this stage as an antecedent of inflammation of the lung; nor does, indeed, Dr. Stokes himself seem to have based his assertion so much on anatomical as on clinical grounds.¹ He states, it is true, that he has met with this condition in pathological specimens, and relates (*op. cit.*) a case of an extensive burn in a child, where nearly the whole of the lung was in this state; but he fails to point out that this appearance is of necessity connected with inflammation. I have had recently an opportunity of examining the lung in a case similar to the one he describes. The organ, in this instance, was dry, and presented an unusually bright colour; yet nothing could be discerned in which it differed from ordinary congested lungs, nor did it, when minutely examined, afford any evidence of inflammatory action.

Another morbid change which has been much dwelt upon by writers, as connected with this first stage, is *splenization*; but so varied are the descriptions given of this condition, and it has been associated with so many diseases, that there is no little difficulty in referring any characteristic appearance to it, save that very general one of a resemblance to the spleen.

¹ "No one can doubt," he writes, "that the crepitating is but the diminutive of the mucous râle; it is a phenomenon produced by the passage of air through a viscid fluid secreted by the irritated cells or terminal tubes, and its peculiar characters result from the bubbles being necessarily so minute. The existence of this sign, then, points out that secretion has taken place into the cells and minute tubes; so that Laennec's first is, in reality, the secretive stage of the inflammation, and every analogy favours the opinion that a stage of irritation has existed previous to the secretion which caused the crepitus."—*Stokes on the Diseases of the Chest*, Am. ed., p. 266.

Andral represents it as a more intense engorgement of the lung, and states it to be an intermediate stadium between the first and second stage of pneumonia. Rokitsansky refers it to a "stasis" or stagnation of blood in the lung, which may lead ultimately to its hardening. Meriadec Laennec, in a note to his cousin's treatise, describes splenified lung as sinking in water, highly congested, yet devoid of granulation, and thinks it arises from a hemorrhagic congestion. Some authors state to have met with it in several pulmonary diseases, some only in pneumonia, whilst others, again (Bouillaud, Lallemand, etc.), make it a synonym of the stage of inflammatory engorgement. Considering, then, the wide scope given to the name, and the different pathological considerations splenization is made to include, it is not easy to determine what changes in the tissue produce the only universally admitted feature—the more intense congestion. Several lungs from typhoid fever patients, which were violet-coloured, yet devoid of granulations, and which, therefore, according to Rokitsansky's description, I considered as splenified, exhibited no marks by which they differed at all from ordinary congested lung, excepting, perhaps, the increased number of blood-corpuscles and of granules found in the fluid contained within the air-cells.

Splenization, when apparent in inflamed pulmonary tissue, shows, superadded to the already described microscopical appearance of this, a larger number of blood-corpuscles and exudation-cells, and vessels more intensely engorged.

Second Stage, or Red Hepatization.—As the inflammation advances towards the second stage, the lung becomes heavier, its spongy character disappears altogether, the air-vesicles are more and more distended with exudation-corpuscles, and the whole structure acquires an apparently solid condition. This second stage, or *red hepatization*, being fairly established, the lung-tissue presents the following well-known characteristics: It is heavy; dense; resembles in appearance the liver; is very friable;¹ sinks in water; if incised, a red, thick fluid transudes, and the cut surface appears distinctly granular. Between the small granulations—which are of varying size in different parts of the lung—masses of a darker colour may be observed, due generally to extravasated blood or to black lung-pigment. These and the white lines of the minuter bronchi and vessels divide the hepatized lung into numerous small sections, giving to it sometimes a distinctly marbled appearance. The granulations themselves are generally round, and difficult to isolate, except by the most careful manipulation.

Now, what are the *minute* changes which take place in the exuded mass, or

¹ The great friability of the lung, in this stage, has led Andral to propose for it the term of red softening (*ramollissement rouge*). Laennec condemns this, on the ground that, in reality, an induration occurs, whilst the humidity of the tissue becomes augmented, and adds: "Je crois que pour rendre complètement l'idée, qu'a voulu exprimer M. Andral, il faudrait dire, au lieu de ramollissement, augmentation de l'humidité." A very doubtful improvement, since increase of humidity occurs without inflammation in many affections of the lung.

in the parenchyma of the lung, which produce the appearances described? We have above traced the effused matter from a frothy serum until it attains a greater tenacity, and found it to consist mainly of granules and exudation-corpuscles, with but few blood-globules. These, indeed, only become fairly obvious at the commencement of this second stage, and are owing to rupture of the over-distended bloodvessels. In many parts of the lung, small lobules may be observed, redder and more densely congested, with their capillaries ruptured, and the air-vesicles full of blood-corpuscles, whilst in other portions no trace of the kind can be discerned. It must, thus, be admitted that the characteristic red sputum does not proceed from the whole of the inflamed lung, but only from those lobules in which the distended vessels have given way, and permitted the corpuscles to mix freely with the exudation.

In the fully-established second stage, round or slightly oval inflammatory cells, from 0.02 to 0.016 of a millimetre in size, may be detected, filling up nearly the entire cavity of the air-vesicle. (Fig. 3.) These cells are extremely varying in contents and shape. Some inclose one or two distinct nuclei, others are entirely filled up by granules. Some are round, others very irregular; although not near so much so as the imperfectly-formed bodies which have been denominated tubercle-corpuscles. Acetic acid leaves, in many, the granules untouched, but renders the nuclei more distinct. Maceration of the tissue in water for several days does not alter the cells materially. Many contain small fat-globules, which may be dissolved out by ether; the granules are not influenced by this reagent. In the majority of instances, the cells lie closely together on the basement-membrane of the vesicles, and are here in such numbers that, by a careless observer, they might readily be taken for a layer of granules. These, in reality, are the only exudation visible in some air-cells (Fig. 4); whilst others, again, contain numerous compound inflammatory cells (Fig. 3, *c*), stained more or less red by the colouring matter of the blood, and generally destroyable by caustic potassa. Besides these elements, many irregular flakes may be discerned (Fig. 3, *b*), probably formed by concrete albumen; also oil-granules and free fat.

Other new-formed substances found in the exudation in pneumonia, and to which attention was more particularly drawn by Remak,¹ are the *fibrinous casts*. These are described as ramifying in the smallest bronchial tubes and in the air-vesicles; their mode of arrangement being suggestive of areolar tissue, whilst their behaviour under acetic acid serves as the distinguishing mark. They are stated, further, to appear in the sputum, between the third and seventh day of the disease, and are considered by Remak as a sure sign of progressing exudation. He did not find them absent once in fifty cases. My own observations on this subject have not been very satisfactory, as I have frequently sought in vain, both in the sputum and the

¹ Diagnostische und Pathogen. Untersuch. Berlin, 1845.

air-cells, for these ramifying coagula. May not the frequency of their existence, as well as their import, have been overrated by their able discoverer?

All the elements enumerated are mainly found in the air-vesicles, and bear a relation, presently to be examined, to the formation of the granulations.

In the dense fibrous *walls* of the vesicles, and in their basement-membrane, but little alteration is perceptible. The former are slightly more granular, contain a few rather elongated cells, with nuclei rendered distinct by acetic acid, and occasional fat-drops; some of the elastic fibres may present a peculiar unravelled appearance. The smaller bronchi are also generally found involved, and within their walls deposits of granules and newly-formed elements are apparent. The vessels, in this as in the first stage, are gorged with blood; but they seem more compressed by the tissues, and their walls are infiltrated with albuminous granules. The epithelium, when present, is in a complete state of fatty metamorphosis.

Site of the Deposit.—These observations would go to confirm the views of those who contend for the almost exclusively vesicular site of the pneumonic deposits, and account for the granulations by the complete blocking up of the air-cells with an inflammatory lymph. Ever since the anatomy of pneumonia commenced to be fairly understood, the questions with regard to the exact situation of the exudation, and the nature of the granulations, formed subjects which gave rise to the most interminable dispute. Laennec¹ considers these granulations as characterizing the stage of hepatization, and describes them as the air-cells converted into solid grains by the thickening of their parietes and the obliteration of their cavities by a concrete fluid. Andral² shares to a certain extent this view of Laennec, at least as regards the secretion of a fluid. Williams,³ on the other hand, inclines to the opinion that the granulations, and hence also the consolidation of the lung, are caused by a mere swelling of the membranous tunics; whilst Stokes, influenced by the rapidity with which the physical signs change from vesicular to bronchial respiration, conceives excessive congestion of blood to be the cause of the granulations, and quotes Andral (correctly?) as in favour of this view.

The opinion cited, with regard to the granulations, throw, then, very little light on the nature or exact site of the pneumonic process. Laennec foreshadowed, to a certain extent, as we have seen, by his description, the view of the air-cells being primarily affected; but nearly all subsequent writers, besides those quoted, preferred admitting as the cause of the hepatization mere swelling of the tissues, and derided the idea of an exudation into the vesicles. When, however, the minute anatomy of the lung began to be more closely investigated, it became evident that mere swelling of the parietes of the cells could not account for the consolidation in pneumonic lungs, or for their undoubted increase in size; and the inquiry, an exudation being

¹ Laennec, by Forbes, p. 184.

² Clinique Méd., iii. 465.

³ Cyclop. of Pract. Med., vol. iii.

admitted, was directed with renewed care to its exact seat. Some, then, contended for the vesicular, some merely for the interstitial tissue, whilst others believed both to be equally affected. The majority of the pathologists of the present day incline to the view that the air-cells receive the pneumonic deposits, although there are not a few who still embrace the opposite sides of the question. Rokitansky takes very decided ground in favour of the exudation of lymph into the air-vesicles in ordinary acute pneumonia, and contrasts the whole process to a parenchymatous croup. He admits, however, besides this "croupy" pneumonia, as a distinct variety, a form in which the interstitial tissue alone is the seat of an effusion which gradually compresses and obliterates the air-cells.

My own observations on the minute anatomy of pneumonic lungs, as well as the clinical facts, connected with the renewed permeability of the lung-tissue after an attack of acute inflammation, have, as already stated, convinced me that the exudation is nearly entirely confined to the air-vesicles, and that only a very limited portion remains in their walls. That the interstitial tissue alone should, under any circumstances, be the sole seat of the deposit, without this reaching the air-cells, is, considering the great minuteness of the walls of the vesicles (for thus the term interstitial must be interpreted), very improbable. No author, moreover, has exactly defined what he means by the interstitial tissue, whether the tissue uniting the lobules, or the elastic tissue forming the walls of the air-cells and lobular passages. It seems, indeed, much as if this view, sanctioned to a certain extent by the high authority of the illustrious pathologist last cited, had been copied from book to book without any exact anatomical or pathological meaning having been attached to it. On the whole, then, we may be permitted to regard as established that the lymph in pneumonia is poured mainly into the air-cells, and only to a slight extent into their walls.

A form of consolidation of the pulmonary tissue, which has been variously described and explained, is that of the dense, dark, yet flabby lung, which, from the resemblance to meat, has been termed *carnification*.

This state of the organ results from the compression by a pleuritic effusion of a pneumonic, or even of a previously healthy lung; whilst, according to some (Rilliet and Barthéz), it is most frequently owing to a chronic inflammation. Minute examinations, in cases capable of being restored to nearly their natural condition by inflation, exhibit the fibrous tissue infiltrated with masses of granules, insoluble in ether; with pigment, and sometimes with oil-globules. The epithelium, also, is more granular: some cells inclose a yellowish pigment-substance.

As the second stage of pneumonia progresses, the colouring matter of the blood is absorbed, and the lung becomes of a slightly more yellowish colour. Its surface is mottled; yellow masses are distinctly visible between the discoloured red spots, and, commencing as isolated points, they soon become general, until we have the peculiar soft, grayish-yellow lung produced, which

has been termed *gray hepatization*, or *purulent infiltration*. The pulmonary parenchyma itself has then become more friable; the granular appearance is in a great measure lost; and a thick pus-like fluid may be pressed out from the apparently suppurating lung. Or if, on the other hand, the pneumonia end by *resolution*, we have the organ becoming of a paler hue, the granulations, lighter in colour, mix with a freshly exuded serous fluid, which gradually dissolves them, leaving the lung itself more infiltrated with serum. The deposit is then thrown off with the expectoration, which itself shows many exudation-corpuscles, granules, and free oil. Or, again, we may have, after an acute pneumonic attack, the lung substance becoming indurated, by portions of the exuded lymph being transformed into lung-tissue.

Before considering the *minute* changes which accompany the formation of the third stage, it will be necessary briefly to recall how exudations degenerate, and what effect inflammation has on tissues. The exudation, the result of a stoppage to the free circulation, may be (see *Paget's Lectures* or *Wedl's Pathological Histology*) of various kinds: either capable of undergoing organization, or not; either firm and fibrinized, or serous—the more special character being determined in individual tissues by the state of the blood, and the seat and degree of the inflammation. The serous effusion is thin and watery, and not a frequent result of inflammation; more generally do we meet, under these circumstances, with the fibrinous. This latter, or the true inflammatory lymph, presents itself, according to Rokitansky, either as the simple or plastic, or as the croupous lymph. The former is generally more fluid, and may readily be entirely absorbed by an effusion of fresh serum, or else it changes into fibrous tissue; the latter, the “corpuscular lymph” of Paget, is firmer, of a yellowish colour, usually excessive, extends over a larger surface, and is prone to break up with great rapidity into a pus-like fluid, exerting a corroding property on the tissues with which it is in contact. These two principal forms of lymph are frequently found mixed in recent exudations.

If any of the varieties of lymph go on towards a further development, they demand, as a necessary condition, the cessation of the inflammatory process. If, on the contrary, they degenerate, the fibrinous part withers up or liquefies, undergoing, ordinarily, a fatty, or in some cases, a pigmental change. The corpuscular elements, to use Paget's term, may also dry up, or undergo a pigmental or calcareous degeneration, but most usually, they change into pus, or are subject to a true fatty metamorphosis, similar to the one observed in the cells of the liver.

On the tissues themselves, inflammation shows its results in several ways. It leads mostly to their softening—from a true disintegration, or from their penetration by the inflammatory products—or to their fatty degeneration.

This last named result, which may affect all normal, as well as abnormal structures, is one of the most interesting, as well as one of the most frequent of all pathological processes. Its most essential feature is the substitution of

minute granules, or oil drops, for the elements of the organ it attacks. In cells it leads to the formation of granule-cells and granular masses.¹

Fatty degeneration may be the effect of a slow chronic affection, in which the nutrition of the part has been gradually impaired, but it may also occur as the result of acute inflammation, and of a sudden disturbance of the nutritive function. Fibrinous exudations, whether simple or croupous, are especially prone to undergo this change, as are also pus and formed tissues. In the latter, when acutely inflamed, fatty degeneration goes, as proved beyond doubt by the researches of Virchow and Reinhardt, hand in hand with softening. Thus Virchow² observes that, in acute inflammation of muscle (excepting when exceedingly violent, in which case softening of the fibres occurred without a change of the exuded lymph), a fatty molecular mass takes the place of the exuded protein substance and of the fibres, leading, within a short space of time, to their complete disintegration. The same changes in the constituents of an organ were produced by the experiments of one of his pupils, who found artificially excited inflammation of the cornea to be followed by alteration in its texture, and especially by the appearance within its elements of fatty molecules.

Third Stage, or Gray Hepatization.—To return from this digression, the bearing of which the reader will presently see, to our legitimate subject. The changes visible in the stage of “purulent” infiltration, or *gray hepatization*, are generally believed to be owing to suppuration having occurred in the parenchyma of the lung and in the exudation. But are pus-corpuscles the elements most prevalent in cases of gray hepatization? By no means. The number of these bodies present is, on the contrary, generally remarkably small, whilst other elements are observed to abound, which cannot be referred to a suppurative degeneration, viz: distinctly marked corpuscles, much larger than those of pus, and filled with granules and small oil drops (Fig. 5). But few of these cells contain nuclei, which, when present, are generally oval and apparent without the use of acetic acid, while the cells inclosing them are distinct, and not very granular. These nucleated cells are perhaps due to a more recent fresh exudation (which undoubtedly sometimes occurs in this stage) still in progress of development. In “yellow” condensation, a lesion presently to be described, they exist in larger proportion. In the ordinary “gray” hepatization, the non-nucleated granular cells are, however, far more predominant. They are uninfluenced by maceration in water, and not broken up by acetic acid. Ether

¹ These granule-cells (*corps granuleux* of Donné, compound inflammatory globules of Gluge) are not always necessarily caused by fatty degeneration of previously existing cells, whether inflammatory or normal. They may form from exudations devoid of corpuscles; although this is the exception, and not the rule.

² See for this, and other points bearing on the subject, his admirable Essay on “*Parenchymatöse Entzündung*,” in *Archiv. für Path. Anat.* Bd. iv. p. 266.

clears them of their granules, and renders them almost transparent vesicles (Fig. 5, *b*); in some it shrivels up the cell-walls. Other elements met with, are flakes of albumen or fibrin, some containing small oil-globules; in exceptional cases, masses of pus-cells (Fig. 5, *e*) predominate. The fibrous tissue in the walls of the air-vesicles is very indistinct, and more or less densely infiltrated with granules (Fig. 5, *c*), which are mostly soluble in ether. Epithelium is entirely absent; nor are compound granule-cells in advanced gray hepatization noticed in the same quantity, as in the stage of red hepatization, whilst free fat molecules and globules abound. The vessels are generally dark brown, and their walls (Fig. 5, *d*), coated with large masses of granules, destroyable by the caustic alkalis.

These minute structural appearances, then, point conclusively to the fact, that gray hepatization is, certainly, in most instances, not a suppuration; but rather a gradual breaking up of the exudation (and, secondarily, of the lung-tissue), allied perhaps in its nature to fatty degeneration, such as we have stated to occur in other parts of the body, as a result of acute inflammation, and commencing with the earlier stages of the pneumonic process. In support of the latter assertion, besides the appearances presented by the microscope, may be cited the fact, that hepatized lungs yield a much larger quantity of fat than the normal organs, or even than such as are in a state of engorgement. Thus Natalis Guillot (*loc. cit.*) writes, that, in hepatized lungs, he finds from 18 to 40 per cent. of fat, whilst in engorged lungs, whether inflammatory or not, the fat, as we have above seen, rarely exceeds 10, or at the highest, 15 per cent. As he does not inform us in what stage of hepatization the lungs examined were, we cannot make any further use of his observations for ascertaining the relative amount of fat in the two stages. Yet there is a strong probability that the organs were mostly in the third, or in a far advanced second stage, as pneumonic lungs in the earlier conditions are more rarely attainable for pathological researches. My own comparative analyses are as yet too few to justify a conclusion on purely chemical grounds; nor do I conceive that this—considering that we have proved the fatty metamorphosis to be a gradual one, and shown that a large amount of oil and granules are visible in the epithelial cells, and in the exudation in the earlier stages of red hepatization—should be as decisive as the microscopic appearances. These last exhibit the exudation-cells much more oily, and further—what most concerns the proposition advanced—the fibrous tissue more indistinct, softer, broken up, and infiltrated with fat-granules, the nearer we approach to the yellowish-gray appearance of the third stage; which we thus can regard, in many respects, as simply a continuation of the second.

The opinion expressed with regard to the non-suppurative nature of most cases of gray hepatization derives further support from a fact which every pathologist has admitted—the extreme rarity of pulmonary abscesses following pneumonia. In over 100 *post-mortem* examinations, extended over a space of 20 years, Laennec only met five or six times with a collection of pus in

an inflamed lung. Andral, when he published the first edition of his *Clinique Médicale*, had seen but one. Chomel,¹ in 25 years, has met with but three. Stokes, too, although he conceives the rarity of the formation of pulmonary abscesses to have been overrated, bears testimony to the infrequency of their occurrence.

The difference, finally, between the matter, in cases of purulent infiltration and ordinary pus, has not escaped the attention of some of the most distinguished pathologists. We find, indeed, in Laennec, that the pus, when first exhaled, is "concrete or plastic, like the false membranes, and passes rapidly through different degrees of softening before it acquires its proper mucilaginous character. When it begins to soften, if the part containing it is pressed or scraped, it escapes under the form of a greasy substance, which a superficial observer might mistake for fat." Rokitansky admits the fact that the third stage does not always bear the aspect of suppuration, and states that the lung has a peculiar yellow appearance before it becomes entirely gray. Lallemand² even affirms that when the pus replaces the blood the lung has the appearance of a fatty liver; whilst Addison,³ evidently struck by the frequent opaqueness and solidity of the matter, expresses his belief that the tissues of the lung are, in the third stage, reconverted into albumen, or undergo, to use his own term, albuminization.

Gradual Changes in the Exudation and Tissue in Pneumonia.—Now, let us attempt, according to the views advanced, a *rationale*, to account for the successive steps of the degeneration of the exudation and of the tissue. In pneumonia, we have an arrest of circulation in the bloodvessels going to the inflamed part, and as a consequence defective nutrition, which is admitted to be one of the most powerful causes of atrophy and fatty degeneration. The morbid condition progressing, interferes at the same time with the supply of oxygen, and changes, as the microscopical examination proves, the character of the normal cells first, and then of the exudation, by converting (to follow the generally received chemical hypothesis of fatty changes) their nitrogenized basis into fat. As the hepatization advances, the same causes operate on the walls of the air-cells and true tissue of the lung, and permit them to become gradually more granular and to become atrophied; which process is accompanied by softening, and, as may be readily seen, frequently by a fatty substitution. If the second stage end in recovery, the effused and degenerating matter is expectorated, and the tissues, which have scarcely commenced to be involved, restored to their natural condition. If, however, the morbid process runs on, it produces a complete state of softening and disorganization of the textures, while sometimes, even in this last stage, a fresh, dense infiltration may occur into the

¹ Dictionnaire de Médecine, vol. xxv. p. 151.

² Recherches Anatomico-Pathol. sur l'Encephale, etc.

³ On Pneumonia, and on its Diagnosis, in vols. i. and ii. of *Guy's Hospital Reports*. 1843 and 1844.

air-cells of a portion of the lung. In cases of recovery from a far advanced second stage, or from the third stage—the possibility of which is proved by Stokes¹ and Andral²—loss of tissue and depressions would probably be observed in the lung. What the minute appearances accompanying these changes are, I am unable to state, as I have never had an opportunity of examining lungs in this condition, and as I am equally unaware of any examinations of the kind having been recorded.

Before bringing this paper to a conclusion, a few questions of great interest, and which have a direct bearing on the subject, remain to be considered. Does the deposit in pneumonia always undergo this degeneration, or does true suppuration ever occur? Are the different terminations of the inflammatory process owing to the kind of lymph deposited? And again, may any variety of exuded lymph give to the eye the peculiar yellowish appearance of the third stage, without having been preceded by red hepatization?

Now, with regard to the *first* question, it is undoubted that the exudation into the lung may be transformed into pus, without any other degeneration of the inflammatory product occurring. For, although the majority of lungs, in the so-called third stage, do not, as stated, present any evidences indicative of suppurative action, we also meet with others in which there is no appearance of any other process. The air-cells are then observed to be filled with pus-corpuscles and molecular masses, whilst the tissues themselves, partly perhaps from admixture with the purulent fluid, are in a state of complete degeneration. Yet these cases are not the most frequent, and are probably such in which the disease occurs most acutely and in depraved constitutions, thus causing the lymph to be rapidly transformed into pus-corpuscles.

The *second* inquiry is not easily answered, as opportunities for examining the effusion, when it first occurs, are extremely rare. The lymph, in most cases of pneumonia, is, as far as can be ascertained, of a mixed character, both fibrinous and corpuscular. Reasoning from analogy renders it not improbable that the more the latter elements abound, the greater would be the tendency of the exudation to a complete and rapid fatty degeneration, or even, in individual cases, to a suppurative process; the more the former, the increased liability to permanent indurations: but any opinion on this subject must, in the present state of our knowledge, be extremely hypothetical.

The *third* question, finally, demands an affirmative answer, as I have met with specimens in which the lung, in “gray” hepatization, was firm, yellowish, and granular, and exhibited, under the microscope, no elements indicative of true suppuration, and but slight traces of any fatty degeneration, but, on the contrary, distinct nucleated cells of varying shapes, a solid blastema, and protein granules. (Fig. 6.) Lungs in this condition are heavy; evidently enlarged; the air-cells distended; and the yellow appearance remark-

¹ Op. cit., p. 269.

² Cours de Path. Interne, par Latour, tom. ii. p. 7.

ably uniform.¹ In no portion of the affected lobe is a transition stage from red hepatization visible, although a red crepitating tissue (the first stage?) may surround the yellow mass. Chemically examined, these lungs yield comparatively only a small proportion of fat; in one specimen I found not more than 7 per cent. The exudation causing this peculiar "albuminous" lung, or *yellow condensation*, if I may so term it, is probably a peculiar kind of inflammatory lymph, deposited rapidly, and allied to the croupous or corpuscular variety. It may break up into pus or into fatty molecules, and evidently gives rise to a distinct pathological species of consolidation, which has incorrectly been classed with gray hepatization, from which it may be distinguished by the greater density of the tissue, a more uniform extension and yellowish colour; and a different structural appearance, when microscopically examined. This yellow condensation may also sometimes occur near portions of lung-tissue already in an advanced state of inflammatory change. Whether it be owing to a peculiarly defective nutrition; to nervous depression; whether its metamorphosis bears any relation to the formation of tubercle, are questions which a more advanced pathology, and a more intimate acquaintance with inflammation and its products will have to solve; as, on the other hand, close clinical scrutiny may learn to connect these different varieties and terminations of acute pneumonia with different symptoms, different diagnostic, and, perhaps, with different therapeutical indications.

¹ For drawings of lungs representing the *yellow condensation*, as above set forth, see *Bock's Atlas*, Part 3d, Pl. I. Fig. 3, and Cruveilhier's *Anat. Pathol.*, Vol. ii. Part I. Livr. xxix. Pl. 5. The latter plate more especially depicts the lesions, as generally observed; it is, at least, the best delineation of the condition referred to, which I have met with in the current works upon Pathological Anatomy. Cruveilhier describes the pulmonary tissue, in the case from which the drawing was taken, as very heavy and dense, and calls the affection "concrete suppuration" terminating a subacute pneumonia. Dr. Hodgkin, in his *Lectures on the Morbid Anatomy of the Serous and Mucous Membranes*, London, 1840, classifies this "yellow" condensation as a *non-plastic* variety of pneumonia; a name adopted on account of the general properties of the lymph deposited. He is, as far as I am aware, the only writer who does not confound this form of hepatization with purulent infiltration. Yet even he does not indicate sufficiently closely the minute appearances of the lung-tissue in these different conditions, and this may explain the fact, that his divisions of pneumonia have been so generally overlooked.

FIG. 1.

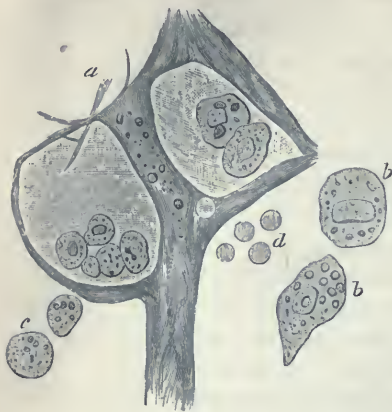


FIG. 3.



FIG. 2.



FIG. 4.



FIG. 5.

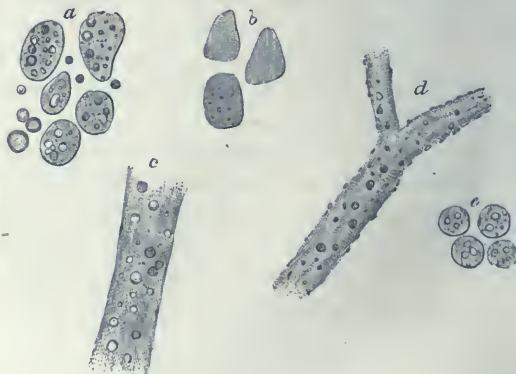
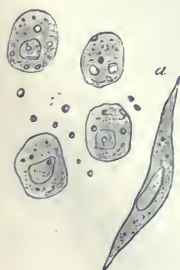


FIG. 6.



DESCRIPTION OF THE PLATE.

FIG. I.—Drawing taken from a lung in rather an advanced stage of inflammatory engorgement, magnified 430 diameters.

- a. The fibrous tissue forming the walls of the air-cells, distinct and inclosing a few granules.
- b. Very granular epithelial cells of bronchi and air-vesicles.
- c. Small nucleated cells and exudation-corpuscles, lying partly within, partly outside of the air-vesicles.
- d. Blood-corpuscles.

FIG. II.—Elements observed in lungs that had been in a state of chronic hyperæmia.

- a. Epithelial cells being transformed into granular cells.
- b. Flakes inclosing blood-corpuscles which are irregular and of a yellowish colour. The colour of the whole mass, indeed, is slightly yellowish.
- c. Blood-corpuscular cell, breaking up into a dark, reddish-brown pigment.
- d. Situation of freshly formed pigment, in the tissue of the lung.

FIG. III.—Appearance of the lung-tissue in *red* hepatization.

- a. Air-vesicles filled with inflammatory cells, some of which are nucleated.
- b. Concrete albuminoid masses.
- c. Granule-cells.

FIG. IV.—Pneumonic lung of a child, magnified 430 diameters.

- a. Air-vesicles inclosing mostly granules; oil drops are observable in the fibrous walls.
- b. Irregular, small, non-nucleated corpuscles occasionally met with in red hepatization.
- c. Granule-cells and oil drops.

FIG. V.—Elements found in the lung in *gray* hepatization.

- a. Granular exudation-corpuscles and free oil.
- b. Corpuscles when treated with ether.
- c. Fibrous tissue infiltrated with oil-globules; fibres very indistinct.
- d. Small vessel, the walls of which are coated with granules.
- e. Pus-corpuscles, rather dark and granular.

FIG. VI.—Elements in peculiar "yellow" condensation, found in a lung which was completely and uniformly infiltrated; magnified 480 diameters.

- a. Nucleated corpuscles and fibroid cells.
- b. Basis-substance in which many of the cells are imbedded.

ART. II.—*Vital Statistics of the City of Baltimore.* By CHARLES FRICK, M. D. (Read before the Baltimore Pathological Society.)

IN the present article on the "Vital Statistics of the City of Baltimore," I have endeavoured to arrive at some conclusions in regard to the present ratio of mortality as compared with previous years, as well as the relation that some of the individual diseases bear to the whole causes of death, and to each other. I have been induced to make this attempt, laborious as it is, in the hope that some facts might be gleaned of importance to the medical profession; and from the assurance that if the community could be made aware that correct statistics could exhibit many important facts influencing their health, they would no longer hesitate and delay about the adoption of some uniform system of registration, which would include all the data necessary for obtaining proper results. Unfortunately, our State and city are without any registration of births or deaths, except such returns as are made to the health office of the city of Baltimore. These are obtained from the sextons of the various graveyards, who make a weekly report of the number of burials; and, although the number, age, and colour of these are in all probability correct, yet the causes of death are by no means accurate, these being ascertained by the sexton from the friends of the deceased, and then returned to the office. As an additional source of error, it is to be remembered that the deaths occurring as the results of syphilis or intemperance are scarcely ever stated as such, the friends being naturally averse to making such disclosures. But this would also be the case under any system of registration; and we are not sure that the causes of death, as now recorded, are not almost as accurate as those obtained under a systematic registration, when we remember what different ideas prevail among members of the medical profession, not only in relation to the treatment, but also in regard to the nomenclature of disease. But one thing registration could do. If we look at one of the facts here recorded, it will be seen that, from 1840 to 1850, the mortality increased from 1 in 50 to 1 in 40 nearly, or 20 per cent. We can see by the records to what causes this is due; but, unless we know what particular sections of the city present the largest number of cases, we are far from ascertaining the true cause, and still farther from providing the remedy this registration could effect. It is of exceeding importance, also, to ascertain the ratio of mortality as it exists among the whites, free blacks, and slaves, separately. But this cannot be done by our present system; for, as will be seen, the return makes the slave mortality 1 in 12, and the free blacks 1 in 41—a difference which is almost impossible, and is only to be explained by assuming that many of the free blacks were returned in the record as slaves.

With the exception of an able analysis of the vital statistics of Baltimore, by Dr. Joynes, published in this Journal for October, 1850, I am not

aware that any attempt has been made, statistically, to exhibit the mortality and its causes in our city. He, however, carried his observations as far only as 1850. I propose to continue them from that time, and to compare the results so obtained with those recorded by him.

And thus it is, in a large city of 200,000 inhabitants, with an annual mortality of over 5,000, there is no record kept but such as the health office for the time being may direct. Without legislative authority, it is impossible for them to ascertain correctly the various facts necessary for drawing correct conclusions, and we, therefore, are content to go on from year to year, lamenting the unhealthiness of the season, alarmed and taken aback when some epidemic disease threatens to extend its devastating influence to our own homes, yet blindly shutting our eyes to the fact that, by a complete and careful system of registration, we might ascertain with tolerable exactness, not only the causes of death, but the localities in which it occurs, and the class of people who suffer the most, and thus be able, not to avoid entirely, yet so modify these causes, as to reduce the ratio of mortality within certain well defined and reasonable limits.

The population of Baltimore for the year 1850, according to the United States Census, was 169,054, showing an increase in ten years of 66 per cent. Of these, 140,666 were whites, and 28,388 blacks. We have not separated the free blacks from the slaves, because we find it impossible to compare them with the mortality as recorded in the returns to the health office. They are divided by age as follows:—

	WHITES.				BLACKS.			
	Males.	Females.	Excess of males.	Excess of females.	Males.	Females.	Excess of males.	Excess of females.
Under 1 year	2,636	2,593	43	...	438	460	...	22
Between 1 and 5	7,708	7,833	...	125	1,313	1,573	...	260
“ 5 “ 10	8,328	8,324	4	...	1,435	1,981	...	546
“ 10 “ 15	8,310	8,452	...	142	1,369	1,396	...	27
“ 15 “ 20	6,526	7,417	...	891	1,113	2,009	...	896
“ 20 “ 30	14,787	13,746	1041	...	2,273	3,548	...	1275
“ 30 “ 40	11,067	9,385	1682	...	1,690	2,419	...	729
“ 40 “ 50	6,213	5,680	533	...	1,045	1,571	...	526
“ 50 “ 60	3,111	3,432	...	321	650	909	...	259
“ 60 “ 70	1,470	1,875	...	405	296	477	...	181
“ 70 “ 80	585	785	...	200	101	172	...	71
“ 80 “ 90	120	238	...	118	38	67	...	29
“ 90 “ 100	13	32	...	19	16	41	...	25
Over 100	...	1	...	1	2	16	...	14
	70,873	69,793	11,779	16,609

We see here how great a difference exists between the two races, so far as regards the proportion of the different ages and sexes, and how necessary it is to include this element in estimating the mortality, either general or special.

General Ratio of Mortality.—Dr. Joynes gives as his ratio for the nine years preceding, including and following 1840—

For the general population, excluding stillborn	.	.	.	1 in 45.42
“ white “ “	.	.	.	1 in 46.40
“ free black “ “	.	.	.	1 in 37.17
“ slave “ “	.	.	.	1 in 26.59

My ratio differs somewhat from this, although calculated, I believe, from the same data. It does not amount to much, and I give it only that the difference between his ratio and that for the nine years preceding, including and following 1850, may not appear greater than it really is.

	Nine years, including 1840.	Nine years, including 1850.
For the general population, excluding stillborn	1 in 43	1 in 40.7
“ white “ “	1 in 45.2	1 in 41
“ free black “ “	1 in 39	1 in 41.5
“ slave “ “	1 in 32.4	1 in 12.6
“ all coloured “ “	1 in 37.8	1 in 33.9

I am satisfied that although, in the nine years including 1850, the ratio for the whole population and for the whites is correct, yet I am sure there is no such mortality among the slave population as 8 per cent.; and almost equally so, that the deaths among the free blacks are not less than among the whites. This may be explained by the fact, that although the *whole deaths* for 1850 are increased over 1840 only 80 per cent., the increase among the *slaves* for the same period is 250 per cent., and, moreover, this great increase is principally registered in the year 1854. It is probable that, in the returns to the health office, many of the deaths of the free blacks were recorded as slaves. This would explain the inconsistency, but not afford the remedy. The United States Census gives me no assistance in correcting this error, for, assuming the number of slave deaths there recorded to be correct, the mortality would be only 1 in 67, which is just too far in the other extreme. Again, the slave population, as recorded in the census for 1850, is one-tenth less than in 1840, while the free blacks have increased one-third. It is impossible to reconcile these discrepancies, and we must, therefore, include the two classes together, and confine ourselves to a comparison between the whites and blacks alone.

It will be seen by comparing the present mortality with that of 1840, that the health of the community is decidedly lessened, about equally both in blacks and whites. Of this fact there can be no doubt, and as the compilation of statistics, whether vital or otherwise, is but a waste of time, unless leading to some practical conclusions, I shall endeavour, in an examination of the causes of death, and the mortality between different ages, to arrive at some conclusions on this point. In ascertaining the ratio of mortality for any number of years, I have adopted the usual plan of selecting the mortality of the year in which the United States Census is taken, and an equal number of

years preceding and following this; averaging the whole, and comparing it with the population as recorded in the census, supposing this to give the average population of the whole number of years examined. I should state that, in all cases, the stillborn are excluded, except when mentioned.

As we wish to make a comparison of the health of the city since 1850 with the preceding ten years, we will state here the comparative mortality of the entire population for the separate years, it being almost impossible, except in the years in which the census is taken, to separate the races with any degree of accuracy.

1840 1 in 50	1850 1 in 40.1
1845 1 in 46	1851 1 in 42.9
1846 1 in 46.4	1852 1 in 38.5
1847 1 in 42.6	1853 1 in 41.9
1848 1 in 39.8	1854 1 in 39.8
1849 1 in 38	
Average of '48, '49, '50 1 in 39.3	Average of '52, '53, '54 1 in 40.1
For New York City, average of 1849, '50, and '51 1 in 26.17	
“ Suffolk County, Mass., in 1849 1 in 27.4	
“ State of Massachusetts, 1849 1 in 35.8	
“ Philadelphia, 1854 1 in 38.4	
“ London, average of 1850, '51, '52, '53, and '54 1 in 27.9	

This comparison shows an almost steady increase of mortality in the city of Baltimore from 1840 to 1850, when it is again diminished for two years, and then varies slightly up to the present. It is, however, seen that the average for '48, '49, and '50 is somewhat higher than that of '52, '53, and '54. The probable explanation of the above lies in the fact, that in 1849 cholera made its appearance, in a very virulent form, in the Baltimore City Almshouse, and the citizens, becoming alarmed, aided the corporation in effecting a thorough cleansing of the alleys and purlieus of the city. The diminished mortality of the two following years shows the benefit the city experienced from these precautions; and, although the difference between 1 death in 38 and 1 in 42.9 does not seem to be very great at first glance, yet a simple calculation shows the difference in actual deaths to be over 500 in the course of the year.

As a comparison with the northern cities, I have calculated the ratio of mortality in New York for three years. It is 1 in 26.7. In Suffolk County, Massachusetts (nearly the whole of which is made up of the city of Boston), for 1849, 1 in 27.4. The whole State of Massachusetts, 1 in 35.8. In Philadelphia, for the year 1854, 1 in 38.4; and for the city of London, an average of five years, 1 in 27.9. We thus see that we enjoy a less mortality in Baltimore than in New York, Boston, Philadelphia, or London, or even than the entire State of Massachusetts. But the data of the first two cities are not exactly the same, and the comparison, therefore, is somewhat unfair. The large mortality in New York is owing to the number of deaths occurring

among the emigrant population, and cannot be fairly considered the mortality of the city, many of them having disease at landing, or soon after; while the comparison for Boston is for the year when epidemic cholera was prevalent, and occasioned nearly 700 deaths. But still the fact remains, that the average mortality of Baltimore is less than any of the above-mentioned places. But in the comparison it is found that the ratio for different ages is not always the same; I have, therefore, compiled the following tables, with a view of illustrating this. In the second, a separate comparison is made, as I am enabled only to divide the deaths in the city of London into three separate heads.

Percentage of Deaths at different Ages, as compared to whole Mortality.

	BALTIMORE.							PHILADEL- PHIA.	NEW YORK.	MASSACHU- SETTS.
	Average from 1845 to 1850.	Average from 1850 to 1854.	1850.	1851.	1852.	1853.	1854.	1849.	1851.	1849.
Under 1 year	25.59	24.38	24.33	24.91	24.21	24.44	23.15	21.73	26.17	18.38
Between 1 and 2	10.88	12.10	12.85	11.90	13.44	10.40	12.30	10.24	12.31	10.42
“ 2 “ 5	11.42	12.27	11.57	11.19	10.98	12.37	12.15	8.62	12.50	10.60
“ 5 “ 10	5.09	6.00	6.25	5.55	6.39	6.00	5.34	4.58	8.25	5.81
“ 10 “ 20	5.48	5.3	5.83	4.90	7.29	5.65	7.29	4.85	4.17	5.89
“ 20 “ 30	9.53	9.7	9.52	10.31	9.69	9.52	10.00	10.85	10.38	11.24
“ 30 “ 40	10.00	9.34	9.00	9.43	7.89	10.20	9.48	11.3	10.18	9.00
“ 40 “ 50	7.59	7.13	6.66	7.57	6.93	7.14	7.24	8.58	6.05	6.76
“ 50 “ 60	4.75	4.58	5.21	4.54	4.35	4.54	4.24	6.80	4.16	5.21
“ 60 “ 70	4.19	4.28	3.7	4.16	4.40	4.60	4.38	5.42	3.06	5.96
“ 70 “ 80	3.34	3.12	3.23	4.00	2.65	3.03	2.76	4.18	1.79	5.90
“ 80 “ 90	1.55	1.35	1.43	1.21	1.25	1.66	1.22	2.28	0.87	4.23
“ 90 “ 100	0.45	0.34	0.31	0.24	0.40	0.43	0.31	0.51	0.14	0.86
Over 100	0.15	0.12	0.11	0.08	0.12	0.05	0.22	0.06	0.01	0.06

	BALTIMORE.		PHILADEL- PHIA.	MASSACHU- SETTS.	NEW YORK.	LONDON.
	Average from 1845 to 1850, exclusive.	Average from 1850 to 1854, inclusive.	1849.	1849.	1851.	1850 to 1854, inclusive.
Under 15 years of age	55.98	57.04	48.02	48.01	61.40	46.84
Between 15 and 60	34.36	32.75	39.53	35.08	32.73	34.13
Over 60	9.68	10.21	12.45	17.01	5.87	19.03

Calculating, from this and the preceding table, the average age in which death takes place, I find it to be, in Baltimore, from 1845 to 1850, 19.8 years, at the present time, 20.6 years—evidencing a slight increase in the duration of life; for New York, 17.9 years; Philadelphia, 26.7 years; Massachusetts, 26 years; and London, 26.8 years. It is interesting to see that the average age in which death occurs is almost exactly the same for Philadelphia and London. But I did not expect to find the result so unfavourable to New York and Baltimore. It is, however, easily explained by a glance at

the last table, where we see the very great mortality in these two cities of persons under 15 years of age. I examined carefully the returns, to see if the percentage of deaths at the different ages, as compared to the whole, would show any increase of mortality among the blacks over the whites; but the ratio is nearly the same in both colours, although showing in both a greater longevity among the females than the males. Thus—

For white males the average age is	19.4 years	} For both, 20.3 years.
“ “ females “	21.3 “	
“ black males “	18.4 “	} For both, 20.9 years.
“ “ females “	23.0 “	

This result is a proof that the ratio of mortality is not always in relation to the longevity. It will be seen, by examining the first table, that a remarkable uniformity exists in the deaths of the different years, at the various ages specified, in Baltimore and also in New York; while of those occurring under one year of age, there is a difference of nearly 4 per cent. in favour of Philadelphia, and no less than 7 per cent. for Massachusetts. Subsequently, we shall endeavour to ascertain what causes tend to make up this difference.

In 1829, '30, '31, and '32, it is stated by Dr. Dunglison, in his *Elements of Hygiene*, that the deaths under five years of age, in Baltimore, were 43.8 per cent.; from 1840 to 1850, they were 47.9; and from 1850 to '54, 48.8—showing an increase of 5 per cent. in 25 years.

The yearly average of deaths from 1850 to 1854, both inclusive, gives as a result 2,689 males and 2,394 females, or 1.16 males to 1 female, and, inasmuch as the female population outnumbers the males by nearly 4,000, the ratio in favor of the females is still greater. This varies with the different months, being greatest in February, May, and June. In other words, these are the months in which the smallest number of females die as compared to the males. The following table illustrates this, and the calculation is made proportionably to the population of each sex:—

	Male deaths.	Female deaths.	Proportion of females to males.
January	191	189	1 to 1.06
February	208	176	1 to 1.24
March	226	198	1 to 1.19
April	193	179	1 to 1.06
May	209	177	1 to 1.23
June	210	178	1 to 1.22
July	317	288	1 to 1.15
August	304	282	1 to 1.12
September	229	206	1 to 1.13
October	188	189	1 to 1.04
November	188	157	1 to 1.12
December	183	179	1 to 1.02

The following table, an average of the same data as the preceding, illustrates, in the same way, the proportion of deaths among the blacks and whites,

as compared to each other, in the different months. It exhibits the fact, which has been observed before, that, in those months which are most fatal to whites—namely, July, August, and September—we do not find a proportionate increase among the black race, for the difference between the two is at this time the most clearly marked. The comparison is made without reference to the population.

	Whites.	Blacks.	Proportion of blacks to whites.
January	299	78	1 to 3.83
February	310	73	1 to 4.24
March	339	82	1 to 4.13
April	316	75	1 to 4.21
May	306	80	1 to 3.82
June	323	65	1 to 4.98
July	527	81	1 to 6.5
August	498	88	1 to 5.7
September	365	70	1 to 5.21
October	315	64	1 to 4.96
November	282	64	1 to 4.4
December	293	69	1 to 4.24
Total	4177	885	1 to 4.72

I have also calculated a table, which is here appended, showing the ratio of mortality at the different ages among the males and females of each race. It is the ratio to the population, separating from each other the males and females, blacks and whites. The numbers are to be read, one death in 4.1, and so on, and is an average from 1850 to 1854, including both years.

	White male.	White female.	Excess.	Black male.	Black female.	Excess.
Under 1 year	4.1	4.6	Male	4.5	4.8	Male
Between 1 and 5 . .	21.2	22.9	Male	14.7	20.5	Male
“ 5 “ 10	77.3	83.5	Male	77.8	119.3	Male
“ 10 “ 20	155.7	140.4	Female	90.3	110.2	Male
“ 20 “ 30	97.0	76.8	Female	35.4	64.6	Male
“ 30 “ 40	82.1	62.6	Female	51.3	21.1	Female
“ 40 “ 50	50.1	70.3	Male	34.1	52.3	Male
“ 50 “ 60	37.4	43.4	Male	26.0	60.6	Male
“ 60 “ 70	20.1	36.6	Male	26.5	29.3	Male
“ 70 “ 80	16.7	15.1	Female	15.7	21.2	Male
“ 80 “ 90	12.0	6.5	Female	17.5	10.4	Female
“ 90 “ 100	5.5	10.6	Male	...	8.0	...
100 and over	1.0

This table is both important and interesting. I shall, therefore, examine it in detail. In the first place, it is shown that one out of every four and a half inhabitants of that age dies under one year, and that the proportion is very similar for both races and sexes, except a slight excess in both races of the male deaths. We do not, however, observe the remarkable preponderance of coloured deaths over whites at this age, as remarked by Dr. Emerson in

Philadelphia, and Dr. Joynes in Baltimore, some years since. Between 1 and 5 years, the general mortality is only one-fifth the preceding period, and the excess of male deaths is more evident; among the whites to only a slight degree, but, among the blacks, the male excess amounts to a third. We also see that, while the ratio between the white males, white females, and black females is nearly the same, the excess of deaths among the male blacks is very great. Between 5 and 10 years, the general mortality is only one-twentieth of that of the first period—there is a greater preponderance of male deaths as compared to the previous age, and the deaths of the black females are far less than the others.

Between ten and twenty years, the deaths are diminished to their lowest ratio, amounting scarcely to one-fortieth of those under one year, and the mortality of the blacks is greater than the whites. The white females show an excess of mortality, and the black males the same. Between twenty and thirty years, the mortality again increases, going on to increase progressively at each subsequent period of life. Among the whites, the females are still in excess, but, with the blacks, more males die. Between thirty and forty years, the same ratio obtains among the whites, and a similar one is observed among the blacks. The mortality of the female blacks here is enormous, being more than double that of the male blacks, and three times as great as the white females.

It has been suggested that the troubles incident to child-bearing would explain the preponderance of female deaths at these two last periods. But such, I think, cannot be the case, from the fact that the deaths recorded from this cause are too few to account for such a difference, and would be balanced by those resulting from accident and exposure among the males. And, moreover, the same excess of female mortality is observed among the whites between ten and twenty years. Is not a more probable explanation to be found in the habits of life of the two sexes, an explanation so reasonable, that it behooves us to look more closely into these habits, inasmuch as the same relation of mortality, and the same habits are found to be coexistent among all civilized people. Until children arrive at ten years of age, both sexes are treated nearly alike, are clothed, fed, and housed alike, and have nearly the same amount of exercise, and similar amusements. After that age, a marked difference in their habits becomes perceptible. The boy, as a general rule, lives in the open air, in which he finds both amusement and occupation. The girl, on the contrary, particularly as she approaches the age of twenty, is confined more to the house, leads a sedentary life, and rarely exercises her limbs and chest. And, while her brother is pursuing the occupation of a blacksmith, carpenter, or bricklayer, she is passing her life at the needle, or some similar occupation. This difference of occupation holds good from twenty to thirty, and from thirty to forty. Afterwards, the occupations more nearly assimilate, and the ratio becomes more equalized. Among the blacks, from ten to twenty years, instead of the

excess of mortality being with the females, we find it among the males. And this fact, we think, adds further support to what we have said. Among the young blacks, all, or nearly all, do hard work. Both sexes are exposed to out-door exercise; the only difference being that the males are principally engaged at those employments in which the risk of life is greatest, and hence their larger mortality. Between forty and fifty years, the males are again in excess, and the difference between the two races is about the same, but a much greater mortality exists among the blacks than the whites. Between fifty and sixty years, the males are still in excess—among the whites, to about one-sixth, but with the blacks, as much as two and a half times. It is remarkable, also, that at this period of life, the mortality among the female blacks is less than between thirty and fifty years.

Between sixty and seventy years, the mortality among the males is still the greatest. The difference is more marked for the whites, and the average of both sexes gives nearly the same result for both races.

Between seventy and eighty years, the duration of life is somewhat in favour of the blacks—with the white females, and the black males.

Between eighty and ninety years, the mortality is with the males, but the average of both sexes is greatly in favour of the blacks. It must be remembered, however, that there is a tendency with the blacks, among whom a record is rarely kept, to magnify their ages, and this would account for a portion of the excess. Between 90 and 100 years, among the whites, the excess of males is nearly doubled, but the reverse is the case among the blacks; for, of sixteen males living at that age, no death is recorded.

Over 100 years, the longevity is in favour of the blacks. There is only one white female recorded in the census, and one death. While, among the blacks, two males and sixteen females are recorded, with no death. We thus see, as stated before, that the ratio of mortality and longevity are not always in accordance. For, of the blacks, where the first is less than the whites, the latter is decidedly the greatest.

Special Causes of Mortality.—All that has been said heretofore relates entirely to the colour, age, sex, and number of those dying in the city of Baltimore, and I have no doubt of the correctness of the data upon which the results have been founded. But the particular causes of these deaths are not so much to be relied upon, for reasons stated in the beginning. Still, they are worth something, and as all were obtained in the same way, are valuable for making a comparison of one series of years with another. And it is not a little remarkable to see how nearly these are in relation with each other, proving, at any rate, a certain uniformity in the diagnosis, even if it is not always correct. Some of these diseases, however, are pretty well marked, and their diagnosis easy; for instance, smallpox, scarlatina, cholera infantum, croup, &c., and we shall therefore make them the subject of a more special examination. The following table consists of the causes of death occurring in Balti-

more for the last five years. I have arranged them in the form proposed by the Committee on Registration of the American Medical Association, departing from it only in one or two trifling particulars. For instance, bilious fever and remittent fever are classed together, instead of being separated; and congestive fever is placed by itself, instead of being classed with typhus. Dr. Joynes, whose paper is before me, and to whom I refer with much pleasure, has, I find, made the same alterations.

Tables showing the Mortality from different Diseases in Baltimore during five years, viz: from 1850 to 1854 inclusive.

	1850.	1851.	1852.	1853.	1854.
I. ZYMOTIC DISEASES—EPIDEMIC, ENDEMIC, AND CONTAGIOUS.					
Cholera	2
Cholera morbus	27	6	10	11	129
Cholera infantum	347	290	339	256	395
Croup	142	134	164	143	210
Diarrhœa	40	15	49	34	39
Dysentery	237	161	222	242	252
Erysipelas	32	19	12	22	9
Fever, congestive	2	4	3	5	4
“ gastric	30	19	14	18	15
“ intermittent	9	2	6	18	3
“ remittent	24	86	77	100	72
“ typhus	107	24	18	17	19
“ typhoid	3	71	78	101	95
Hooping-cough	22	86	77	100	72
Measles	7	21	314	16	99
Mumps	5	11	1	...	1
Scarlatina	225	234	232	362	234
Smallpox	145	100	64	9	26
Varioloid	8	4	2
Varicella	3	3	1
Syphilis	1
Thrush	8	13	6	5	9
	1433	1313	1698	1460	1688
II. SPORADIC, OF UNCERTAIN OR GENERAL SEAT.					
Abscess	1	3	3	3	6
Cancer	11	20	13	16	26
Dropsy	118	140	109	146	142
Gout	1	...	1
Hemorrhage	13	12	15	17	14
Inflammation	2
Marasmus	9
Mortification	2	5	1	1	3
Scrofula	8	11	13	17	16
Sudden death	28	21	28	4	27
Tumour	4	3	6	5	4
	188	215	187	209	247

Tables of Mortality in Baltimore from 1850 to 1854—Continued.

	1850.	1851.	1852.	1853.	1854.
III. OF THE NERVOUS SYSTEM.					
Apoplexy	22	23	30	64	37
Cephalitis, &c.	112	104	134	164	115
Chorea	1
Convulsions	92	99	102	136	116
Delirium tremens	6	7	8	4	8
Hydrocephalus	122	120	118	45	165
Insanity	12	8	2	3	6
Paralysis	42	47	34	36	38
Tetanus	4	1	6	2	3
Brain, disease of	15	15	18	9	13
	427	424	453	463	501
IV. OF THE ORGANS OF RESPIRATION.					
Asthma	7	6	6	9	8
Bronchitis	12	18	30	20	10
Catarrh fever	121	101	144	79	91
Consumption	581	679	728	893	928
Pleurisy	27	28	39	41	57
Pneumonia	87	73	144	132	160
Quinsy	2	3	1
Disease of respiratory organs	9	10	4	8	4
	844	915	1097	1185	1259
V. ORGANS OF DIGESTION.					
Colic	13	11	13	31	115
Dentition	99	93	120	94	114
Dyspepsia	9	4	7	8	13
Enteritis	54	66	78	80	55
Gastritis	4	4	3	4	10
Hernia	2	3	4	1	7
Worms	10	16	6	1	3
Disease of stomach and bowels	2	10	3
Hepatitis	14	18	15	18	21
Jaundice	4	1	1	2	3
Piles	1
Fistula in ano	1
	211	226	250	239	343
VI. ORGANS OF CIRCULATION.					
Organic disease of heart	53	83	76	10	69
VII. OF THE URINARY ORGANS.					
Diabetes	2	1	...	8
Cystitis	3	2	1	5	1
Gravel and stone	2	4	3	...	28
Nephritis	3	3	10	7	2
VIII. OF THE GENERATIVE ORGANS.					
Childbirth	75	70	83	65	71
Disease of womb	6	1	2	1	2
IX. OF THE ORGANS OF LOCOMOTION.					
Rheumatism	7	8	10	7	7
Joints, disease of	2
Hip, disease of	2
Spine, disease of	5	14	11	10	10

Tables of Mortality in Baltimore from 1850 to 1854—Continued.

	1850.	1851.	1852.	1853.	1854.
X. OF THE INTEGUMENTARY SYSTEM.					
Ulcer	1	...	2
Carbuncle	2	...	3
XI. EXTERNAL CAUSES.					
Accident	33	46	56	81	106
Burn	16	17	18	22	24
Drowned	29	46	42	52	47
Debility	1	...	30	175
Exposure	6	2	4	1	12
Sunstroke	1	3	1	12	15
Drinking cold water	4	2	2	5	1
Infanticide	1	...	1	...
Intemperance	24	28	34	30	23
Murder	1
Poison	1	1	1	7	3
Strangulation	1
Suicide	5	6	11	1	10
Violence	1	1	13	2	8
Wounds	7	...	3	1	...
XII. CAUSES UNKNOWN.					
Infantile	585	556	647	554	439
Adult	72	75	101	101	107
XIII. OLD AGE					
	140	156	174	180	184
XIV. STILLBORN					
	415	372	348	347	340
	4625	4539	5313	5150	5676

RECAPITULATION.

I. Zymotic diseases	7592
II. Sporadic diseases	1048
III. Of the nervous system	2268
IV. Of the organs of respiration	5290
V. Organs of digestion	1269
VI. Organs of circulation	291
VII. Of the urinary organs	85
VIII. Of the generative organs	376
IX. Organs of locomotion	93
X. Of the integumentary system	8
XI. External causes	1138
XII. Causes unknown	3247
XIII. Old age	834
XIV. Stillborn	1822
Total	25,300

The deaths from zymotic diseases are one in 3.33 of the whole, rather less than usually prevails in other large cities. The principal diseases among this class are, first, cholera infantum, next scarlatina, then dysentery, and then croup. These four diseases constitute three-fourths of the mortality of this class, and one-fifth of the deaths from all causes. From smallpox, the mortality has gradually declined within the past four years, although an increase

is noticed as compared to previous years, as will be shown subsequently. Six per cent. of the deaths from zymotic diseases are attributable to measles. But this, large as it is, is undoubtedly not all. Death more often occurs from the sequelæ of this disease than from the disease itself, and a large proportion of the cases of dropsy should be placed to this account. But two deaths are recorded from epidemic cholera, and these in the year 1854. This is undoubtedly a mistake. I myself saw three fatal cases, and heard of more among my friends. The deaths from cholera morbus for this year are put down at 129, while, for the previous four years, they averaged only 13. Many of these, in 1854, were undoubtedly epidemic cholera; but even admitting this, we have the assurance that Baltimore, within the past few years, has enjoyed an extraordinary immunity from this pestilence, so devastating to the human family.

In the following table, I have selected the principal diseases belonging to each class, and given the yearly average of deaths produced by each, for the last five years. They are arranged side by side with similar averages of the preceding ten years. The difference in population for the years corresponding to these averages, is fifty per cent. I have therefore increased each number in the latter column that amount, in order that the increase or diminution of deaths from the separate diseases may be evident at a glance.

	Average from 1850 to 1855.	Average from 1840 to 1850, 50 per cent. added.
Cholera infantum	325	275
Croup	159	128
Dysentery	223	56
Fever	201	200
Measles	91	59
Scarlatina	257	261
Smallpox	70	39
Dropsy	131	93
Convulsions	109	120
Hydrocephalus	114	96
Catarrh fever	107	156
Consumption	762	786
Pleurisy and pneumonia	157	117
Dentition	104	68
Childbirth	73	44
Accident	64	50
Intemperance	28	33
Stillborn	364	390
Old age	167	180
Unknown	447	571

In the beginning of this article, we started with the fact that the average mortality of our city, for the last five years, had increased over the previous ten, from one in 46.3 to one in 40.6. We see now to what diseases this increase is due. Dysentery exhibits the largest, it being as much as four hundred per

cent. A large increase is also observed in cholera infantum, croup, measles, smallpox, dropsy, hydrocephalus, pneumonia, dentition, childbirth, and accident. The mortality from fever, as a whole, remains the same. While it is a source of congratulation that from scarlatina, convulsions, catarrh fever, consumption, and intemperance, the deaths have considerably diminished. The diminished number of deaths, recorded from old age and unknown causes, is due to greater accuracy in the returns of the causes of death to the Health Office.

I have also prepared a table, which is here subjoined, showing the proportion of deaths to each 10,000 inhabitants, in Baltimore, for the periods above mentioned, and also for some other places, of which I have been enabled to obtain reports. I have separated Suffolk County from the rest of Massachusetts, as the first is composed almost entirely of the city of Boston—in which city I was unable to procure a separate return—in order to show the difference between the mortality as it occurs in the city and country.

I have also included together, catarrh fever, pneumonia, bilious pleurisy, inflammation of the lungs, and congestion of the lungs, under the head of pneumonia, in order to make a comparison with the city of London. I did not do this with the other places, for fear of mistake, as their nomenclature was more indefinite than the London reports.

Proportion of Deaths to each 10,000 Inhabitants.

	BALTIMORE.		NEW YORK.	SUFFOLK COUNTY.	MASSACHUSETTS.	LONDON.
	From 1840 to 1850.	From 1850 to 1855.	1851.	1849.	1849.	From 1850 to 1855.
Smallpox	3.41	3.73	10.22	1.41	1.51	2.86
Scarlatina	15.00	13.20	11.41	23.21	15.53	14.49
Cholera infantum . .	14.51	17.72	13.10	4.35	4.82	...
Dysentery	2.89	11.56	21.67	21.94	33.31	0.75
Croup	6.73	8.00	8.41	5.15	4.63	2.12
Consumption	40.84	39.25	43.28	46.5	49.5	30.08
Pneumonia	11.46	11.72	17.86
Convulsions	6.09	5.33	3.25	4.96	5.63	9.08
Childbed	3.36	3.74
Old age	9.80	8.63	9.68
Stillborn	22.72	18.76	23.42	19.13	5.94	...
Teething	4.45	5.30	2.75	12.61	3.93	3.09
Infantile unknown . .	39.06	21.38
Disease of brain . .	14.06	16.02
Disease of bowels . .	6.28	9.26
Bronchitis and pleurisy	2.91	2.88
Fever	10.24	10.29

I have arranged, also, in the following table, the number of deaths produced by the individual diseases at different ages, in Baltimore. It will be seen that it includes, under definite heads, ninety per cent. of the whole number of deaths. Under the head of fever, are included all enumerated in the table as such; under diseases of the brain, all enumerated as diseases of the nervous system;

and, under diseases of the bowels, cholera morbus, diarrhœa, colic, dyspepsia, enteritis, gastritis, and diseases of stomach and bowels.

		Consumption.	Cholera infantum.	Smallpox.	Childbed and old age.	Croup.	Dysentery.	Convulsions.	Pneumonia.	Teething. -	Scarlet fever.	Diseases of brain.	Diseases of bowels.	Fever.	Infantile and adult unk'wn.	Diseases not enumerated.	Total deaths.
Under 1 year	5	213	28	...	54	31	49	47	29	24	75	14	7	536	11	1123	
Between 1 and 2	10	134	25	...	45	42	14	37	71	55	76	15	18	80	22	594	
“ 2 “ 5	7	...	23	...	35	23	11	46	...	79	50	14	18	19	118	448	
“ 5 “ 10	12	...	12	...	8	38	6	13	...	45	20	8	11	...	90	263	
“ 10 “ 20	53	...	11	12	...	25	4	8	...	21	19	21	24	...	49	247	
“ 20 “ 30	180	...	26	42	...	19	3	13	...	1	8	23	34	18	32	399	
“ 30 “ 40	145	...	11	18	...	20	2	14	14	14	20	18	105	381	
“ 40 “ 50	74	...	14	3	...	9	1	19	20	16	17	12	93	278	
“ 50 “ 60	58	...	3	10	1	11	11	6	12	16	92	220	
Over 60	37	...	3	140	...	15	1	12	30	12	6	8	93	367	

Smallpox.—In examining in detail the table preceding the last, we see that while the mortality from this disease has slightly increased within the last ten years, it is less in London, Boston, and Massachusetts, than in Baltimore, but much greater in New York. This disease being evidently under the control of vaccination, we may presume, from these facts, as we also know from other circumstances, that the laws to prevent its extension are more strictly carried out in the cities first mentioned than with us. Examining still more in detail the deaths from this disease for the year 1850, as we are better enabled to compare the mortality of that year with the population, we find that, for the whites, the deaths are 8.1, and for the blacks, 14.5 to 10,000 inhabitants, nearly double among the latter race. The disease is more fatal to males than females, in the proportion of 60.6 per cent. of the first, to 39.4 per cent. of the latter. The proportion of deaths, at different ages, is:—

	Per cent.
Under 1 year	18.0
Between 1 and 5 years	30.8
“ 5 “ 10 “	7.5
“ 10 “ 20 “	7.7
“ 20 “ 30 “	17.0
“ 30 “ 60 “	17.3
Over 60 years	1.7

In January the deaths are	12	In August the deaths are	7
“ February “	15	“ September “	9
“ March “	20	“ October “	10
“ April “	12	“ November “	13
“ May “	9	“ December “	25
“ June “	13		—
“ July “	11		154

Showing that nearly two-thirds took place in the six cold months, when persons are more exposed to contagion.

Scarlatina.—From this disease, the deaths have diminished within ten years. They are greater than in New York, but less than in Boston, Massachusetts, or London. It is also shown that, in Boston, the mortality is much greater from this cause, than in the State at large, and nearly double that of Baltimore. For the year 1850, the following is the result: The deaths are in the ratio of 13.8 per cent. for the whites, in 10,000 inhabitants, to 10.8 per cent. for the blacks. For the males, 49 per cent. of the whole, and the females, 51 per cent. The less mortality among the blacks is probably due to the disease being recognized among them with more difficulty, and consequently overlooked. The proportion to the different ages is:—

	Per cent.
Under 1 year	10.9
Between 1 and 2 years	25.1
“ 2 “ 3 “	20.0
“ 3 “ 4 “	10.9
“ 4 “ 5 “	9.1
“ 5 “ 10 “	20.0
“ 10 “ 20 “	9.7
Over 20 years	0.3

January	21	August	6
February	31	September	13
March	17	October	15
April	31	November	18
May	20	December	13
June	19		
July	19		223

Nearly one-half of the deaths occurred during the first four months.

Cholera Infantum.—From this cause, the mortality has increased considerably within ten years. It is greater than in New York, Boston, or Massachusetts. In the latter places, the deaths from this cause are comparatively small, but this is due to the difference of climate. In London, no deaths are recorded from this disease, or from any similar to it. For 1850, the proportion to the 10,000 inhabitants is 22.5 for the whites, and 10.6 for the blacks, showing that the whites are more than twice as liable to it as the other race. The sexes are about equally divided, 48 per cent. being males, and 52 per cent. females. The proportion to the different ages is:—

	Per cent.
Under 3 months	7.1
Between 3 and 6 months	13.3
“ 6 “ 9 “	13.8
“ 9 “ 12 “	27.7
“ 12 “ 18 “	33.1
“ 18 “ 24 “	6.0

June	10	September	75
July	131	October	9
August	122		
			347

60.8 per cent. are recorded between nine and eighteen months.

Dysentery.—It is in this disease that is shown the largest increase of mortality within the past ten years. Of the causes for this, it is idle to speculate unless we knew in what particular localities the deaths occurred. Large as the proportion is, it is, however, much less than recorded elsewhere, with the exception of London. In the State of Massachusetts, it is one-third greater than in Boston, and three times larger than in Baltimore. This excess for the rural districts over the city, as shown for Massachusetts, I am inclined to think, also prevails in Maryland. By equalizing the deaths from dysentery in proportion to the population, I have shown the increase to be four hundred per cent. If we examine the records from 1836 to 1849, the yearly average of deaths from this disease is only 24. While, from 1849 to 1854, it is 210, or nine times greater. Now, 1849 was the year in which epidemic cholera was prevalent, and we thus see that although Baltimore escaped this disease, yet its influence was felt then, and has continued to the present time, in the more frequent attacks and greater fatality of dysentery. So great has been the increase, that the number of deaths from this disease alone, would alter the yearly mortality from 1 in 42 to 1 in 40. In the analysis of the year 1850, this disease is found to be four times more fatal among the whites than the blacks, the numbers being for the first 16 to the 10,000 inhabitants, and only 4.6 for the latter. It is also more fatal among males, the proportion being 59 per cent. for the males, and 41 per cent. for the females. They are divided among the different ages as follows:—

	Per cent.
Under 5 years	42.8
Between 5 and 10 years	16.8
" 10 " 20 "	10.5
" 20 " 30 "	8.2
" 30 " 40 "	8.4
" 40 " 60 "	8.0
Over 60 years	5.8
<hr/>	
February 2	August 86
March 3	September 77
May 2	October 15
June 5	November 3
July 44	
	<hr/>
	237

90 per cent. of the whole occur in July, August, and September.

Croup.—From this disease, the mortality has somewhat increased within ten years. It is shown to be slightly less than in New York, but greater than in the other places recorded. For the year 1850, the analysis exhibits about the same mortality in both races, it being for the whites, 8.5 to the 10,000 inhabitants, and for the blacks, 8.8. The deaths among the males are 49 per cent. and the females 51 per cent. The mortality at different ages and seasons is:—

January	55	August	35
February	49	September	58
March	53	October	55
April	60	November	42
May	42	December	63
June	25		—
July	44		581

The mortality from consumption, as compared to the whole deaths, excluding the stillborn, is—

Baltimore, average from 1850 to 1854 is	1 in 6.2
Philadelphia, 1854	1 in 8.0
New York, 1852 to 1854	1 in 7.5
Boston, 1848 to 1852	1 in 6.6
London, 1853 and 1854	1 in 9.2

Dr. Joynes makes the deaths from this disease, in Baltimore, previous to 1850, 1 in 5.7. The present ratio, therefore, exhibits a less mortality from this cause. This table would seem to be at variance with what I have stated previously. Here, Baltimore shows the largest mortality, while in the previous table, it is less than Boston or New York. But it must be recollected that the one is the ratio to the number of deaths, and the other, to the population. If the mortality from all causes were the same for Boston and Baltimore, this difference could not occur. But as it is much larger in Boston, the discrepancy is easily explained.

Pneumonia.—Under this head, as stated previously, I have included several affections as returned in the Health Register, such as inflammation and congestion of the lungs, bilious pleurisy, and catarrhal fever. These are probably all pneumonia except the last, which consists, as I understand it, of infantile pneumonia and bronchitis, and as these cannot be separated, I have included them together. The ratio, therefore, of pneumonia from this cause, will be somewhat too high. We remark a very slight increase in the deaths from this cause within the past ten years, but still much less than in the city of London, the only place with which we can make a comparison, for reasons above stated. For the year 1850, we find that 60 per cent. of the deaths from this disease are males, and 40 per cent. females. Under five years of age, the difference is more marked, 67 per cent. being males, and 33 per cent. females. The fatality is greater among the blacks than whites, the proportion being 12.4 to the 10,000 inhabitants for the whites, and 14.5 for the blacks. The ratio to the different ages and seasons is as follows:—

	Per cent.
Under 1 year	20.4
Between 1 and 2 years	16.8
“ 2 “ 5 “	21.2
“ 5 “ 10 “	5.9
“ 10 “ 20 “	3.8
“ 20 “ 30 “	5.9
“ 30 “ 40 “	6.2
“ 40 “ 60 “	13.9
Over 60 years	5.9

January	26	August	5
February	24	September	1
March	34	October	10
April	41	November	18
May	24	December	13
June	14		—
July	7		217

58.4 per cent. are under 5 years of age, and more than one-half the deaths occur during the first four months.

Convulsions.—The deaths from this cause have diminished within ten years, but are still greater than in New York or Boston. They are much higher in London, and about the same in Massachusetts as in Baltimore. For 1850, the analysis shows the mortality to be 37 per cent. for the males, and 63 per cent. for the females. Also, 5.4 deaths to the 10,000 inhabitants for the whites, and 7.9 for the blacks. The ratio to the different ages is—

	Per cent.
Under 6 months	34.5
Between 6 and 12 months	18.5
“ 1 “ 2 years	15.4
“ 2 “ 5 “	11.8
Over 5 years	19.8

In January	10	In August	7
“ February	6	“ September	4
“ March	10	“ October	6
“ April	16	“ November	7
“ May	4	“ December	10
“ June	6		—
“ July	8		92

It would seem from these results, that convulsions do not bear a very close relation with dentition. The latter may be divided into two periods, viz: from the seventh to the twelfth month, and from the eighteenth to the twenty-fourth; and we see here that the largest mortality occurs before the eruption of the teeth, that is, the sixth month. A separate head is made, in the reports, of dentition. The mode of death, in many of these cases, must in all probability have been from convulsions, but we have no means of distinguishing.

Dentition.—The table shows the deaths, from this cause, to be increased within the past ten years, and greater than in the other cities recorded, excepting Boston. For 1850, the result is, 43 per cent. for the males, and 57 per cent. for the females. For the whites, 5.9 to the 10,000 inhabitants, and for the blacks, 7.6 The ratio to different ages is—

	Per cent.
Between 6 and 8 months	8.7
“ 8 “ 10 “	10.0
“ 10 “ 12 “	10.0
“ 12 “ 14 “	20.4
“ 14 “ 16 “	17.8
“ 16 “ 18 “	24.4
“ 18 “ 24 “	8.7

January	4	August	14
February	9	September	13
March	3	October	8
April	11	November	7
May	5	December	4
June	9		—
July	13		102

62.6 per cent. occurred between twelve and eighteen months.

Old Age.—The deaths recorded under this head have diminished somewhat in ten years, and are slightly less than in London, the only place with which we have made a comparison. The diminution is in all probability due to more accuracy in diagnosis. For 1850, we have the proportion of whites dying from this cause, in 10,000 inhabitants, 7.8, while, of the blacks, 11.6 die. The percentage for males and females is about the same, between 60 and 70 years of age; after this, it is 71 per cent. for the females, and 29 per cent. for the males. The proportion at various ages is as follows:—

	Per cent.
Between 60 and 70 years	17.7
“ 70 “ 80 “	40.0
“ 80 “ 90 “	34.4
“ 90 “ 100 “	7.1
Over 100 years	0.8

In January	15	In August	13
“ February	10	“ September	9
“ March	13	“ October	10
“ April	14	“ November	3
“ May	12	“ December	11
“ June	6		—
“ July	23		139

Childbed.—The mortality has slightly increased from this cause. Inasmuch as there is no register of births kept, we can make no approximation of its fatality in proportion to the number of cases. In Massachusetts, the births are to the population as 1 to 38. The same proportion here would give, after subtracting the stillborn, 600 less than the number of deaths, which we are sure cannot be the case. In 1850, they are divided as follows:—

	Per cent.
Under 20 years	16.1
Between 20 and 25 years	29.3
“ 25 “ 30 “	25.2
“ 30 “ 40 “	25.2
“ 40 “ 45 “	4.0

January	5	August	2
February	4	September	4
March	8	October	6
April	11	November	6
May	8	December	8
June	4		—
July	9		75

Stillborn.—The mortality under this head, has diminished within ten years. The cases recorded are less than in New York and Boston, but three times greater than in the State of Massachusetts. It is an exceedingly interesting fact, that these cases are very much less frequent in the rural districts, owing no doubt to the different class of population, and it is a source of congratulation to remark the diminution in Baltimore. We can make no comparison of importance in reference to the frequency of their occurrence among the different races, and other particulars, for the reasons stated in speaking of childbirth. For 1850, the whites were 314, and the blacks 101, a great preponderance of blacks, as we might expect. The males were 57 per cent. and the females 43 per cent. The excess of male births is found to be, in most places, about one-twentieth; if the same relation should hold good here, as it probably does, we would find the same excess of stillborn males, as is observed in the deaths for the first years of life.

There is nothing to be said about the other diseases recorded in the table, as they speak for themselves.

Infantile Unknown.—From more care in the returns to the Health Office, we find the number of deaths, from this cause, to have diminished nearly 50 per cent. It is interesting, also, to examine the returns for 1850, and see how much of this might be avoided. The deaths recorded are 585; 536, or 92 per cent., are under one year of age, and 81 per cent. under six months. Thus:—

	Per cent.
Under 1 month	48.0
Between 1 and 3 months	19.0
“ 3 “ 6 “	14.5
“ 6 “ 12 “	10.6
“ 2 “ 5 years	7.9

It is difficult to make a diagnosis under six months, and still more under one month, and such would be the case under any system of registration. We have therefore but 19 per cent. of them which may be said to be strictly unaccounted for.

In summing up these analyses, we should say that our object in compiling the table of separate diseases, was to collect together as many of the most important and easily recognized, as might be done without occupying too much space, or fatiguing the reader. And that the analyses consist of 72 per cent. of the white deaths, and 40 per cent. of those occurring among the blacks. The others recorded in the health returns include but few deaths, and are thus of small importance compared to those we have had under examination. I have here, as in all cases throughout this article, omitted the repetition of figures as much as possible, inserting those only that seemed necessary, in the hope that the tediousness inseparable from such reports would be somewhat lessened, as also the labour and expense of printing. We might go on arranging table after table, and swell this article to twice its present length, but enough, we think, has been said to lay before the profession, as fully as the record will

admit of, all the prominent points connected with the vital statistics of Baltimore, up to the present period. We have seen that the mortality has increased, and to what causes this is due. But, let us remark that the tendency in large cities is for the ratio of mortality to increase with the augmentation of the population, and that our present ratio is better than five years since, or than any of the large cities to the north. The advantage we possess over the latter is to be explained by accident of situation, building, &c. But the improvement since 1849, can only be explained by the greater care bestowed on the cleanliness of the streets, and public hygiene generally. To these causes we owe our almost complete exemption from cholera. But still more can be done, and could the community at large only have impressed upon them, through the agency of the medical profession, the importance of proper registration, causes now obscure, but operating most extensively upon the welfare of those around us, might be discovered and remedied. These often escape observation, because we are not aware of the extent of evil influence they produce; nor will they ever be correctly ascertained without statistical investigations. The objection is made, that such investigations must necessarily be imperfect; but I think I have shown that, with the data now attainable, some important conclusions may be drawn, and certainly, with legislative authority, they might be made much better, and, as a consequence, the deductions would be still more reliable than they now are.

I am glad that the Medico-Chirurgical Faculty of Maryland, at their last meeting, appointed a committee to memorialize the Legislature in regard to this matter; and should they fail, I see no reason why it might not be done for our own city. We personally are interested only in the latter, but, for the sake of science and humanity, let us hope that it may be made to include the whole State of Maryland.

BALTIMORE, June 28, 1855.

ART. III.—*Report made to the Medical Society of Alleghany County, Pa., on the Epidemic Cholera which prevailed in Pittsburg during the months of September and October, 1854.* By THOS. J. GALLAHER, M. D., A. M. POLLOCK, M. D., and W. DRAINE, M. D. (Published by order of the Society.)

THE committee appointed to draw up in a condensed form the interesting points connected with the epidemic cholera as it occurred recently in Pittsburg, would respectfully present the following report:—

The summer of 1854 was one of the hottest and driest ever experienced in this part of the country. The hot weather commenced in June, and continued till the last of September, during which period the thermometer frequently

ranged from 98° to 100° in the shade, and from 120° to 140° in the sun.¹ No heavy showers occurred during the whole season until the 12th of September, on the morning of which day a refreshing shower set in, lasting for two hours. In consequence of the long-continued heat and drought, the soil of the country had become dry and parched, and vegetation, in a great measure, destroyed. Provisions in the autumn, of course, rose to such an enormous price that the poor were, in a great measure, unable to procure them, and were compelled to live mostly on salted and dried meats, with bread, and a scanty supply of vegetables. Even the middling classes, from the complete stagnation in business which then existed, were unable to supply themselves with their usual abundance of healthy nutritious food.

These causes operated powerfully in reducing the flesh and in enfeebling many of our citizens. In fact, all suffered more or less from them. When, therefore, the fall came, and with it the cholera, the reduced systems, and debilitated constitutions of our fellow citizens, were little able to resist the operation of the cholera poison.

The summer, while it was hot and dry, was also rather more sickly than usual; but the diseases which prevailed were not different from what we ordinarily see at this season. Diarrhœa, dysentery, and pneumonia formed the principal of these. Towards the end of August, and early in September, an unusual number of persons were afflicted with felons on their fingers, and boils and pustular eruptions on various parts of the body. Cases of pustular eruptions, in particular, were very numerous.

During the first twelve days in September, there was a marked increase in the amount of sickness in the city, and in the number of deaths. Among these last a few cases of cholera were reported; but bilious and serous diarrhœa and dysentery were known to have produced the greatest mortality.

The first appearance of cholera in this locality was early in July. In that month, a lady, who had returned from a western and southern tour, where the disease was then prevailing, took the disease in the borough of Sligo, and, after a few hours, died. In a few days, several members of the family contracted the complaint, and some of them died also. From this time to the sudden and severe outbreak of the disease in the city, an occasional case occurred in Sligo and Birmingham, and a few in the city, in persons who had contracted the disease down the river.

As has already been stated, on the morning of the 12th of September, a copious shower of rain fell; and, on the 13th, the cholera first appeared as an epidemic. It was, therefore, ushered in by a heavy rain, as it had been noticed to do elsewhere. The lower part, comprising the old wards, was the first to suffer. Gradually it spread through the Fifth and Ninth Wards, and made its way up the hill to the Sixth and Seventh. The hilly part was certainly the last attacked by a few days. It appeared, in-

¹ See appendix to this report.

deed, to rage there with the greatest malignancy, while it was declining in the district below. The very summit of the hill, comprising the district in the neighbourhood of the new basin, and the whole face of the hill next to the Fifth Ward, comparatively escaped; the few cases that did occur having been no doubt contracted in the infected districts. Part of the First and Fourth Wards, forming what is generally called the Point, and that part of the Eighth which lies on the northern slope of Boyd's Hill, were comparatively uninfected. Every other part of the city may be said to have been infected, though in different degrees.

In the districts where it prevailed to the greatest extent—the narrow streets, lanes, and confined courts, where the houses are small, confined, and badly ventilated—the disease appeared to be the most malignant. Thus, in the Fifth Ward, Mulberry Alley suffered most; in the Third Ward, Webster Street, Cherry Alley, upper part of Virgin Alley, Miltenberger's Alley, with the courts proceeding therefrom, contained the greater number of cases; whilst in the Second Ward, Hardscrabble produced the greater number of deaths.

Perhaps no part of the city, of an equal size and population, had as many deaths within its confines as this last named spot. This place contains, mostly, small frame houses, and is situated at the base of that part of Boyd's Hill which faces the west. Along the western border of this cluster of houses the canal, which is partially filled with all kinds of filth, mineral, vegetable, and animal, and which contains but a small stream of water, runs, forming the outlet of the Pennsylvania Canal. The population is almost entirely foreign, composed mostly of Irish and Welsh, who are either common labourers or are employed in the iron and glass manufactories. The two streets of the district are not paved, and therefore not kept clean. The dirt, which is allowed to collect upon the streets and small by-ways is washed, in a great measure, into the canal by the rain. This summer, however, as there had been but little rain, a greater accumulation of domestic filth took place than usual. No doubt this accumulation, both about the houses of the district and within the canal, which forms the great reservoir for their impurities, was one of the causes which led to the almost universal prevalence of cholera here. It is a singular fact that, whilst the inhabitants of this district, which, as has already been stated, is situated at the base of the *western* declivity of Boyd's Hill, perished in such numbers, those living on the northern and southern declivities of the same hill, whose occupations and nationalities are the same with those in Hardscrabble, suffered but little from the cholera.

When the cholera visited this city in 1849 and '50, it confined its ravages mostly to the German and coloured population; in this visitation, however, all classes and nations of people were attacked; but the Irish, especially those who had recently arrived in the country, were the principal sufferers. Unquestionably, many persons of American birth fell victims to the disease,

but of these the aged, the intemperate, and the weakly, formed the principal part. The coloured population was comparatively exempt.

It may be worthy of remark that, while from forty to ninety died daily in our city for a period of at least ten days, not more than one or two of these occurred in our principal hotels.

As to the total number of deaths during the recent epidemic, we can give but a near estimate. The number reported to the Board of Health, and published in our newspapers, exceeded eight hundred. There were many deaths, however, unknown both to our Board of Health and to newspaper reporters. One of the members of the committee knew himself of five or six cases which were hurried to the graveyard and buried without any note being taken of them, and whose names never appeared in our newspaper reports. Knowing that there must have been a great many others similarly buried, from the haste it required to inter the great number who fell, we are led to the conviction that, at the lowest estimate, one thousand persons perished in this city from epidemic cholera in the fall of 1854.¹

When the cholera first made its appearance among us, its attacks were exceedingly malignant; but, in a few days, many of them assumed milder types. Accompanying cholera, and especially when it was on the decline, and had assumed a milder form, we had every variety of bowel affection, such as dysentery, cholera morbus, bilious and serous diarrhœa, &c., all of which were rather difficult to manage.

We have now to detail the most prominent symptoms of cholera as it appeared in this city. For the sake of brevity, we will divide the disease into three periods, viz: 1st. The period of accession; 2d. That of collapse; 3d. That of reaction.

1. In the period of *accession*, in many cases the patient complained of debility, uneasiness in the bowels, and slight headache, previous to the occurrence of diarrhœa. In the majority of cases, however, no premonitory symptoms ushered in the attack, diarrhœa being the first noticeable symptom. The alvine evacuations, which recurred every fifteen or thirty minutes, or more or less frequently, according to the violence of the attack, were copious, painless, and thin; at first, mostly of a bilious or feculent character, but, as the disease progressed, they became albuminous and thinner than at first, and of a whitish colour, resembling rice-water or fine soapsuds. Occasionally, they were transparent, or of a slight brownish hue, as though a few drops of tincture of kino had been mingled with them. These evacuations were occasionally squirted from the anus with considerable force, but they mostly came

¹ According to the record of the Board of Health, the number of deaths which occurred in this city from April 1, 1853, to April 1, 1854, was 865, whilst the number which occurred from April 1, 1854, to April 1, 1855, including the period in which epidemic cholera prevailed, was 1,936, making the total number of deaths the present year, ending April 1st, exceed that of the preceding by 1,171.

away without any effort on the part of the patient, the desire to evacuate the rectum being sudden and irresistible. These discharges were very exhausting to the patient, and reduced his flesh so rapidly that his body became thin, and his features contracted and pointed in a few hours. After the diarrhœa had continued for a longer or shorter period, vomiting ensued, the evacuations being, in many cases, like those passed by the bowels. Most frequently, however, the discharges from the stomach consisted of glairy mucus, or of the medicines and drinks which the patient had been taking. Occasionally, the irritability of the stomach was so great that everything received into it was immediately ejected with considerable force.

Early in the progress of the complaint, spasmodic contraction of the muscles ensued, which generally commenced in the lower extremities, and extended thence to the abdomen and upper parts of the body. These spasms were clonic in their character, frequent, and exceedingly painful. There were generally present, also, great thirst, uneasiness in the epigastric region, feebleness and slight increase of frequency in the pulse, coldness and moisture of the skin, partial or complete suppression of the urine, and stupidity, though great clearness of the intellectual faculties.

The above sketch is a brief and rough outline of the first stage of cholera, as it occurred, perhaps, in the greater number of cases, but variations from this description were very numerous. In very many cases witnessed by us, there was neither vomiting nor cramps, the patient apparently sinking into collapse from the exhausting diarrhœa. It was observed, too, that these cases, which were free from spasms, were the most severe, and likely to have the most speedily fatal termination. In every case of cholera, without a single exception, so far as our experience goes, diarrhœa was present, and formed one of the earliest and most prominent symptoms. We have seen a few cases in which from one to three evacuations only were passed, but we saw none in which diarrhœa was entirely absent.

Under the combined effects of diarrhœa and the other symptoms described, the patient rapidly lost strength and flesh, and, in the course of from two to twenty-four hours, was precipitated into the second, or collapsed stage of cholera.

2. In the *collapse* of cholera, there were great emaciation and loss of strength; a blueness and contraction of the whole body, and especially of the face; sunken eyes; a peculiar dryness and huskiness of the voice; feebleness, smallness, and finally entire cessation of the pulse at the wrist; a coldness of the skin, tongue, and breath; and suppression of the urine. The thirst became very intense, and the skin covered with a copious, cold, clammy perspiration. The patient was stupid and dull, and averse to all exertion, mental and physical, but, when aroused, would answer questions rationally, give proper directions about things which interested him, and appeared perfectly rational. The diarrhœa and cramps would greatly harass him, but, in the interval of these, he would be perfectly easy. Towards the termination of

the period of collapse the stupidity increased, whilst the diarrhoea and cramps diminished, and finally ceased, allowing the patient to pass the few last hours of his existence in a kind of pleasant and tranquil stupidity. The case generally terminated fatally in the cold stage without any evidences whatever of reaction. A short time before death the patient sometimes became delirious; though most generally he remained perfectly rational to the last. When the case did not terminate fatally, but exhibited evidences of improvement, then the patient gradually passed into the period of reaction, which we will now consider.

3. In passing into the *reactive* period, the pulse gradually returned, and became fuller and stronger; the skin lost its pallor and blueness, and gradually became warm and red; the secretions of bile and urine returned; the vomiting and diarrhoea ceased; the huskiness of the voice diminished; the spasms disappeared; and, in a short time, a return of appetite and inclinations indicated an approach to the restoration of health. In such a case, where no inflammatory symptoms supervened, the patient would likely recover his usual health in the course of a few days. Occasionally, however, the most favourable case would relapse into collapse, and die in a short time. When this unfavourable result did occur, it was found mostly owing to some indiscretion in diet, or to too early rising from the bed. It not unfrequently happened that a patient, after reaction had taken place, instead of rapidly recovering, passed into a state of low fever, resembling the typhoid, marked by hot skin, frequent and feeble pulse, coated tongue, great prostration of strength, &c., which ran on for several weeks, and sometimes terminated fatally. In this period, other complications frequently arose, which greatly retarded the restoration of the patient's health, or entirely destroyed his hopes of recovery. Among these gastric or gastro-enteric inflammation or irritation, and inflammation and congestion of the brain and its membranes, were the most common. Various eruptive affections of the skin, and abscesses of an enormous size, were observed to form during this period, but patients afflicted with these were generally known to get well.

Cases of cholera which terminated fatally did so mostly in from six to thirty-six hours. Some few of exceeding malignancy were said to have terminated in three hours. Quite a number lingered on for several days. When a case was protracted beyond two or three days, a recovery generally ensued. The cold stage was the period in which they generally died. When death took place after reaction had set in, it was mostly in consequence of one or other of the inflammatory complications already spoken of. When death was approaching, a cessation of the pulse at the wrist for twelve or twenty-four hours before life was extinguished was not an uncommon event.

Cases were numerous in which there was no collapse, the disease having been arrested by appropriate remedies previous to the accession of that stage.

Causes.—The predisposing causes were operating during the whole summer preceding the epidemic outbreak, so that by fall, when the atmosphere of

this city suddenly became filled with the choleraic poison, suitable subjects were plentifully found on which it might exert its baneful influence. The long continued and excessive heat and drought of the whole season from May till September, the deficient supply of good and wholesome vegetables and meats, and the presence in the water with which the city is supplied of an unusual number of living and putrid animalculæ, were the principal predisposing causes. At the time of the outbreak of cholera, the streets, lanes, and sewers of the city were in a deplorably filthy state, the rivers lower than they had been for many years, navigation entirely suspended, and the labouring population mostly out of employment. Every kind of business appeared to be in a state of suspension or decline, and the citizens were in a state of great anxiety and depression of spirits. Just then, when everything appeared most unfavourable, the cholera commenced its ravages. From the causes above enumerated, the systems of our people became enfeebled, and were thus placed in a proper condition for the reception and development of the choleraic poison.

As to the exciting causes, we have little to say. The attack of cholera was generally so sudden that we were not able, except in very few instances, to trace any exciting cause. Of those in which we could trace an exciting cause, we found that those substances which, in ordinary times, would produce diarrhœa, developed then both cholérine and cholera.

The specific cause of cholera now commands our attention, and, in justice we must confess that we know but little about it. Some, as Dr. Snow and others, contend that, whatever might be its essential nature, this substance is found alone in water which forms the medium of its communication. Our experience in the present epidemic runs counter to Dr. Snow's. As has already been observed, the disease fastened upon the lower parts of the city first, and gradually extended to the hill; left parts comparatively exempt from its ravages, whilst others it attacked with the greatest malignancy; and even in those districts where it prevailed to the greatest extent, it appeared to select certain parts in which its victims were more numerous than in the rest. When we recollect that our citizens are all supplied with water from the same source, we cannot reconcile the above facts with the idea that water was the medium for the diffusion of the choleraic poison. Equally inconsistent are they with the idea that winds scatter this deleterious agent, and thus spread cholera. We cannot say what this agent is; all we know of it is by its effects. We are convinced, however, that it has its existence in the atmosphere—not in any altered proportion of its constituents, but in the addition to it of a material poisonous substance.

An interesting question now arises, Can cholera be propagated by contagion? We were once of opinion that cholera was not contagious, but our recent experience has shaken our former convictions. We are now of opinion that the complaint can be and has been propagated by contagion. It is not, however, a very communicable disease; it becomes so only under certain

circumstances. A patient confined with the disease in a small, close room, in which the air is confined, we think can generate an animal poison which may become so concentrated as to produce the disease in some who may be exposed to it. In corroboration of this opinion, we will adduce a few facts which, although negative in their character, are nevertheless almost conclusive on this point.

The origin of cholera in this locality we have briefly stated in the commencement of this report, but the full particulars were not then given. These are as follows :—

Mrs. B——n arrived at the residence of her brother-in-law, Mr. B., in South Pittsburg, which is situated on the bank of the Monongahela, opposite the city, on the 12th of July. On her passage up from New Orleans she had symptoms of cholera, and was laid up for two or three days at New Albany in consequence of these symptoms. On the 14th, just two days after her arrival, she sickened, and died of cholera in six hours. A daughter of Mrs. B., who had been waiting on Mrs. B——n, took the disease on the following day, the 17th, and died also. Mr. B.'s friends, becoming alarmed at his situation, prevailed on him to change his residence to the city the same day of his daughter's death. The next day after his removal, the 18th, his son, aged four years, and an infant, were attacked, the latter of whom recovered. On the 19th, Mrs. B. and another son took the complaint, the former of whom died. It is known, also, that a neighbouring woman, who had visited the house in South Pittsburg during the illness of the family, in two or three days after exposure took cholera and died. After that time, several in the immediate neighbourhood were afflicted with the same disease. In fact, this part appeared to be a nidus from which it spread all along the river as far up as Birmingham, and down as far as Temperanceville, where an occasional case occurred up to the time that it broke out with such malignancy in the city. It must be recollected that, up to the time of the arrival of Mrs. B——n from the South, not one case of cholera had occurred in the city or its environs. She was the first subject attacked, and she evidently brought it with her. From her it spread among the inmates of the house in which she lodged, and from thence to the neighbourhood. A plainer instance of the disease having spread by contagion could not well be adduced.

Another instance of not less striking character may be related. Whilst the cholera was raging in the city, Mr. W. fled from it to a small village on Saw-mill Run, named Shalerville, a distance of about three miles. He, with his family, put up at the house of a friend who lived in a small cabin, in which were lodged several boarders, mostly coal-diggers. The day after his arrival, Mr. W. took sick, and died with the cholera. Within a couple of days, two of the boarders—coal-diggers—likewise were taken with the disease, and died. Neither of these had been in the city for two weeks previous to this event, nor had they been exposed in any way to a choleraic atmosphere previous to the occurrence of the disease in the person of Mr. W.

They had constantly been employed in the coal-pits during this time, among the workmen of which not one case had occurred, nor had there been a case in the village in which they reside. No other attacks were known in the village afterward, as rumour of the complaint prevented the villagers from visiting the house. It seems pretty certain that the disease was contagious in this instance, as the coal-diggers evidently caught it from Mr. W.

During the recent epidemic, it was observed that, when the disease attacked one of the inmates of a large, clean, well-ventilated house, it did not generally spread to the other members of the family; whereas, when it appeared in an inmate of a small, filthy house, situated in some confined court or alley, in which the air must necessarily be confined and impure, more of the family were sure to suffer. It was not an unfrequent event for a whole family thus situated to be taken down with the cholera, the individuals being attacked in succession. The most rational method of accounting for this peculiarity is in the supposition that filth and contagion were concerned in the propagation of cholera. In the case of the former, the poison which we suppose to be generated by the body of the afflicted person was driven away, or so much diluted by the fresh air which was constantly admitted to the room, that it was rendered in a measure innocuous. In the case of the latter, in consequence of the bad ventilation, the choleraic poison was allowed to accumulate in the air of the room in which the patient was confined to such an extent as to produce the cholera in very susceptible persons who might breathe it. It seems, therefore, that the cholera became contagious only where the patient was confined in an imperfectly ventilated room, where the poison could acquire a certain degree of concentration. This poison appears active in proportion to its concentration. In this respect, it resembles the contagious principle of all other contagious diseases. In this explanation, we can very readily perceive the reason why so many of the poor were carried off by this awful plague, while so few of the more favoured classes were taken.

Going a little beyond the present epidemic, we may be permitted to refer to the occurrence of the disease in our western steamboats. Among the cabin passengers, who have the advantage of cleanliness and good ventilation, the cholera seldom occurs; but among the deck passengers, who are crowded together on the lower part of the boat, where the air is much more confined, and where cleanliness is unknown, the disease is quite common. It has been observed, too, that when one case occurs on the lower deck, others are sure to follow with great rapidity. It thus appears the same on steamboats as in the city. Good ventilation and cleanliness check its progress, bad or imperfect ventilation and filth favour its rapid extension.

These considerations lead us to the conclusion that cholera is a contagious disease, though its contagious character is not so evident as some others of that class. Its progress through a city we have no doubt may be considerably retarded by paying great attention to cleanliness and ventilation.

Cholera may likewise be portable, as Dr. Baly believes ; but we have nothing in our possession to conclusively demonstrate this fact.

Some physicians have thought that there was a connection between cholera and scurvy—that, in fact, the former was nothing more than the full development of the latter. The committee were unable to trace any connection between them. We did not see a single case of cholera in which scurvy was present, nor one of scurvy in which symptoms of cholera existed. One of the members of this committee, who is Physician to the Western Penitentiary, in his report to the Board of Inspectors, mentions the fact that, immediately on the cessation of cholera in this city, the scurvy broke out in that institution ; but not one of those attacked had a symptom of cholera. The report referred to remarks : “If cholera is only a full development of the scorbutic diathesis, as some theorists would have it, here was a fine field for its revels.”

Prophylactics.—Although we know of nothing which acts as a perfect prophylactic of cholera, yet we have no doubt that certain precautions, diseases, &c., will, to a certain extent, protect from that awful pestilence. Personal cleanliness, good ventilation, temperate habits, wholesome food, and moderate exercise of the mental and physical systems, will contribute much in preventing an attack of cholera.

It has long been the opinion of many of our physicians that our smoky atmosphere tended to keep off epidemic diseases. When the cholera first visited this country, in 1832, many of our principal cities were severely scourged by it, while Pittsburg was but slightly touched. Again, in 1849 and '50, not more than one hundred and twenty deaths occurred from this disease in the city, while perhaps twice that number happened in its immediate vicinity. These facts seemed to confirm the surmise of the disinfecting nature of our smoke, and made the citizens feel secure from the future attacks of that malady. The scourge of 1854, however, destroyed in part this feeling of security, and apparently exploded our much cherished theory. A little examination of this subject, however, will confirm our preconceived opinions. Cholera has never proved so fatal here as it did in the other great cities of the Union. Even in the present epidemic, which was by far the most severe one we ever had, the mortality, so far as we can learn, was not near so great as in many other of our cities the same year. Thus Pittsburg, out of a population of 60,000, lost 1,000 from cholera, being 1 to every 60 of its citizens.

In consequence of the great quantity of bituminous coal consumed in our city, the atmosphere is almost continually filled with the impalpable powder of carbon and other results of combustion. This carbon we can frequently see, especially before a rain, falling to the earth in the form of flakes. The disinfecting power of our atmosphere we consider to be owing to this carbon, and perhaps to sulphurous acid and other products of combustion with which it abounds. It should, therefore, follow that the disinfecting quality of the atmosphere would be in direct ratio with the amount of these products in it ;

and this we find to be exactly the case. During the summer and fall of 1854, much less coal was consumed in this city, both in private dwellings and in manufacturing establishments, than ordinary; and, therefore, the usual amount of carbon and other products of combustion were not thrown into the air. An invasion of cholera at this time, would, of course, be less controlled by these disinfecting agents. This, we believe, is one of the reasons why the disease attacked so many of our citizens. Had the season been a usual one, had our markets been filled with their usual variety of good vegetable productions, had our citizens generally been employed in their usual avocations, and our atmosphere filled with its usual supply of smoke, we hardly think it probable that the cholera could have committed such ravages among us.

As has already been stated, the cholera commenced on the 13th of September. From this day it gradually increased until Monday, the 18th, when it appeared to be near its acme. This day fifty-one persons died of cholera. On the afternoon of this day, bonfires of coal, pitch, pine boards, shavings, &c., were built on nearly all the streets and alleys where cholera was known to prevail, by which means the atmosphere of the city was soon filled with smoke. These fires, probably numbering several hundred, were kept in a state of activity from the 18th to the 20th, inclusive, when, on account of the partial subsidence of the disease, many of them were allowed to become low or entirely die out. The good effects of these fires were apparent on the very following day, as forty-three deaths from cholera only were reported on the 19th, and many of these were from attacks of the previous day. On the 20th, there was a still further reduction in the number of deaths, as we find but twenty-four reported to the Board of Health. The 21st was set apart by the public authorities as a day of fasting, humiliation, and prayer. On this day our large manufacturing establishments ceased operations, and the bonfires were greatly neglected, and many of them allowed to burn out. The same day the cholera renewed its energies, and attacked more than it had done on any previous day, the number of deaths reported being sixty-five. On the approach of evening, the fires were renewed and many new ones built, coal being the material from which they were made, and these were kept constantly burning for more than a week. During this period, the whole city appeared dotted over with large coal fires, giving the appearance at night of an immense military camp, and the amount of smoke floating in our atmosphere was unprecedented. The day after the renewal of the fires the number of deaths was reduced to forty-eight, and many of these occurred in persons who had been attacked the day previous. From this time the disease rapidly declined, as we find in the record only thirty-one deaths for the 23d, twenty-six for the 24th, twenty-five for the 25th, &c.; so that, in three weeks from the first onset of the disease, it was no longer regarded as epidemic among us.

As has already been observed, the record of the Board of Health (from which we have obtained our statistics) does not show the exact number of

deaths in this city, and it is unfortunate that it does not; but it is sufficiently accurate to give us the relative proportion of deaths *per diem* during the cholera season. In this respect, it accords entirely with our own individual experience.

The brief relation of the above historical facts shows an exact relation, in an inverse ratio, between the amount of smoke in our atmosphere and the number of deaths from cholera; that, just in proportion as the smoke increased, the cholera diminished, and *vice versa*. The inference fairly deducible from this is, that the products of the combustion of our bituminous coal protect us in part from pestilential cholera.

Our experience during the present epidemic also leads us to regard the occurrence of pustular affections on the skin, and abscesses, to a certain extent, as preventives of cholera. For some time previous to the occurrence of the epidemic, and during its prevalence, we noticed a greater number of these eruptions than we ever recollect of having seen before in the same time, and we cannot remember a single instance in which any of these were attacked with the cholera. Even in cases of lingering cholera, whenever we discovered a cutaneous eruption or abscess of any kind commencing, we always regarded it as a favourable indication, and generally predicted correctly the recovery of the patient. We saw a few cases bearing a very unfavourable aspect, in which very large and painful abscesses commenced on the neck at the base of the ear, and spread over the whole side of the neck as far down as the clavicle, and over the side of the face, and the patients gradually recovered. These facts lead us to the belief that various inflammatory affections of the skin and subcutaneous cellular tissue afford protection from cholera.

Treatment.—In this part of our subject we will be brief. When the cholera first came among us, such was the malignancy of the attacks that nearly all of the first few cases died. In a short time, however, the disease assumed a milder form, when it became, in a great measure, amenable to treatment. At the outset of this branch of our subject, we may remark that we saw or heard of no infallible remedy for the cure of the cholera. All old plans of treatment were tried, besides new ones introduced by our physicians, but in none could implicit confidence be placed. Patients died, no matter what treatment they might have been subjected to.

In the first stage of cholera, especially in the commencement of it, in moderately severe cases, we found sugar of lead and morphia, with or without calomel, a very excellent preparation. In very many cases, it would speedily check the diarrhoea, and prevent the patient from falling into the collapsed condition, from which he was by no means likely to recover. We generally gave it to adults in the proportion of two and a half or three grains of sugar of lead, to one-sixteenth or one-eighth of a grain of sulphate of morphia, dissolved in a teaspoonful of cinnamon-water. This dose we gave every fifteen minutes or half an hour, or less frequently, according to the greater or less

urgency of the case. Should he vomit up a dose, it was repeated immediately, when it would generally remain. In connection with this remedy, the patient was strictly enjoined to remain perfectly quiet in a horizontal posture, and use the bed-pan for the reception of his evacuations, when it could be procured. This precaution it was necessary to insist upon most strongly, as it was in most cases absolutely necessary for the recovery of the patient. Indeed, we saw many cases which had a fatal termination, or at least a severe relapse, in persons who did not altogether comply with this injunction after they had commenced to improve. The patient was generally covered up warmly in bed, hot bricks applied to his feet, a large mustard cataplasm placed over his abdomen, and sometimes to his back, and drinks strictly prohibited. In lieu of drinks, he was allowed to suck ice, which generally proved very refreshing. An occasional injection of twenty grains of sugar of lead, or ten grains of tannic acid, in one or two tablespoonfuls of thin starch-water, was administered, and directions to retain it, if possible, and suppress an évacuation from his bowels if possible. Light cases, subjected to this treatment in the earlier stages of the disease, generally had a favourable termination. The diarrhœa was soon checked, and neither cramps nor collapse supervened. Recovery, considering the violence of the malady, was mostly rapid and complete.

There were many cases, however, and these were mostly those of a severer type, in which this treatment—at least the internal employment of sugar of lead, and opium or morphia—had not the slightest effect. These cases generally proved fatal, unless arrested by some other treatment. After trying many remedies and modes of treatment in these unyielding and severe cases, the plan which succeeded best in our hands we shall now briefly relate. The patient was placed in bed, and *perfect* rest in a horizontal posture strictly required. Pillow-cases or small bags filled with hot oats were placed around the body, a large mustard plaster applied over the abdomen, and an injection of ten or fifteen grains of tannic acid, in a little thin starch-water was given, if possible, after every copious discharge from the bowels, which the patient was requested to retain as long as possible. Drinks of every kind, in cases attended with much gastric irritability, were forbidden, though free indulgence in the luxury of sucking ice was allowed. As an internal remedy, we employed strychnia and spirit of turpentine. Our usual prescription, varied by the age of the patient and other circumstances, was as follows: R. Strychnia sulph. gr. $\frac{1}{4}$ vel $\frac{1}{2}$; spir. terebinth. \mathfrak{zj} ; mucilag. acaciæ \mathfrak{ziv} . M. One teaspoonful of this was ordered every fifteen minutes, until a marked improvement in the symptoms was observed, when the interval was lengthened to twenty minutes or half an hour. As the case continued to improve, the dose was given not more frequently than once an hour, or once in two or three hours, according to its effects on the symptoms, and continued as long as any active symptom of cholera beyond mere prostration was observed. Where the patient appeared to be sinking rapidly, we sometimes removed the warm applications

which were around his body, and had him rubbed very severely all over with coarse flannel cloths dipped in dry mustard; afterwards renewing the hot applications. When there was no vomiting or irritability of the stomach, we ordered, if there was danger of rapidly sinking into collapse, one tablespoonful or more of the best French brandy to be given every half hour, and a tablespoonful of beef essence every hour, or less frequently, if the symptoms were less threatening.

Under this treatment, patients frequently recovered from the most malignant attacks of cholera, many of whom unquestionably would have succumbed under any other mode of treatment we saw tried. We therefore unhesitatingly give it the preference to all others. We must here acknowledge that we did not cure every case by the plan proposed, as there were some which went on to a fatal termination, notwithstanding our treatment. Some physicians of this city, and a few, indeed, at our suggestion, tried the strychnia treatment, but were not so happy in its employment as we profess to have been. This, we think—in fact, we know—was owing to their not following it up with the same promptness that we did, or with the same determination. Either larger doses were given at more distant intervals, or the prescription changed for something else before it had time to produce its specific effects. The fatal effects which mostly followed in either of these events were not fairly chargeable to the inefficiency of the strychnia. That this remedy may prove beneficial, it is absolutely necessary that it be given in small doses at short intervals, and followed up strictly by the other directions already pointed out. Without perfect rest in a lying posture, and the use of the other adjuncts alluded to, as ice, mustard plasters, fresh air, &c., we could not reasonably look for the recovery of the patient, no matter what internal preparations might be given him.

We do not claim the formula employed as original with us; it was first published, we think, two or three years ago, in the *Transactions of the Medical Society of Ohio*, from which we obtained it; but the other means we employed were derived from other sources, and were such as our common sense and individual experience led us to adopt. We claim, however, to have been eminently successful, and can conscientiously recommend the treatment we employed to our medical brethren for a trial.

In the mild variety of the disease, where nearly, or perhaps the only symptom present was the peculiar diarrhoea, the tincture of kino, with some preparation of opium, given every hour or so, frequently cut short the complaint and established convalescence. Tincture of kino was sometimes given in combination with brandy with advantage.

Some of our physicians had more confidence in Cartright's preparation of capsicum, camphor, pulverized carbon, mercury with chalk and gum Arabic, than in any other. One of our principal physicians informed us that he carried some of this powder about him during the whole cholera season, and used it in many cases of cholera with great advantage.

The ultra opium treatment, we believe, was generally discarded, it having been tried here ineffectually in 1849.

The calomel treatment was partially tried in one of our hospitals, and, it is said, with considerable success. We cannot, however, speak of it from experience.

The sulphuric acid, in large doses, was also employed, and, in the hands of some, with success. We gave it in two cases, but, both the patients dying, we lost confidence in it.

The administration of heroic doses of sulphate of quinia was also resorted to, but not with the success anticipated. Two cases in which we used it died.

It sometimes happened that, in consequence of the vomiting, the medicine given internally would not be retained. When this occurred, the application of Granville's liniment to the epigastrium for a few minutes would frequently allay the irritability of the stomach, so that medicines could be retained. Sometimes it was necessary to remove the skin from the vesicated part, and sprinkle upon the abraded surface one grain of sulphate of morphia. By this application the vomiting would often be checked.

From the collapsed stage very few recovered. The most of those who died were in the collapsed stage before any medical assistance was called to them, and many of them were really moribund. The treatment employed in this was various. Cartright's preparation, calomel, charcoal, sulphuric acid, sulphate of quinia, brandy, carbonate of ammonia, sulphuric ether, strychnia, and a host of other astringents, antiseptics, stimulants, and tonics, were employed with varying success. For our part, we derived more advantage from the strychnia, in connection with brandy or carbonate of ammonia, frictions and hot applications to the external surface of the body, than from any other remedies.

When reaction had become fully established, if no complication supervened, the patient generally recovered in a few days by keeping quiet and taking care not to eat indigestible or improper articles of diet. Some gentle tonic hastened the return to health.

The variety of fever which sometimes followed this reactive stage was treated on general principles. The remedies employed depended upon its violence and peculiarities.

In case the brain or its membranes suffered from inflammatory symptoms, the remedies employed were such as are generally had recourse to when these symptoms are noticed in the progress of any other complaint. Purging, however, in a great measure, was avoided. Leeching the temples or cupping behind the ears, and repeating these as often as the case required; applying ice or other refrigerating applications to the head; bathing the lower extremities in water strongly impregnated with salt, mustard, or other stimulants; keeping the head elevated; and administering an occasional stimulating injection, were the chief means of cure. In some cases, blistering behind the ears, or on the back of the neck, contributed to the

recovery of the patient. By the use of these means, a favourable termination was generally obtained.

Another complication which frequently harassed the patient was an inflammatory condition of the stomach, and sometimes of the intestines, in which case the continued uneasiness in the epigastrium and vomiting prevented the medicines and food from lying on the stomach, and occasionally placed his life in jeopardy. When this condition was present, the application of a few leeches, repeated several times if necessary, to the epigastrium, or, what we preferred, the use of Granville's lotion, and morphia applied in the manner pointed out when speaking of the treatment of the first stage of cholera, was generally found to afford prompt and complete relief. Common poultices to this part were also used with great advantage. Great care was taken in the diet. Small quantities of gum-water alone were given at first for food and drink; but, as the case improved, a more substantial diet was allowed.

We need not speak of any other of the complications occurring in the reactive stage of cholera, as they were but few in comparison with those already given. The brain and its appendages, and the stomach and its prolongations, were unquestionably the organs which suffered most in convalescence from cholera.

With regard to the relative mortality of cholera in private practice, or under different systems of treatment, we could obtain no reliable information beyond our own experience. We are persuaded, however, that by far the greater number of those attacked recovered. In hospital practice, the mortality was unquestionably greater than in private; but this no doubt arose from the difference in the kind of patients, and the period of the disease at which treatment was commenced, many of those received into hospitals being moribund on admission.

We deem it proper to state here that this report thus far was written by the chairman of the committee, and he alone is responsible for the statements and opinions contained therein.

Since the above was written, we have received from one of the members of the committee, who is one of the physicians to Mercy Hospital, the following note relative to the admissions, treatment, and comparative mortality of cholera cases in that hospital:—

“Admitted to Mercy Hospital from September 14th to 30th, 72 cases of cholera; 38 died and 34 recovered. Of those that died, 30 were admitted in a dying condition; many of them I never saw, as they were admitted and died between my morning and evening visit. They were treated with quinia, calomel, capsicum, and opium, in combination, which was usually retained by the stomach when given in moistened sugar. If rejected, the same was given in pill, only substituting *ol. piperis nig.* for the *pulv. capsici*. Sulphuric acid and strychnia were used in a few cases, but with no beneficial result. However, these medicines were not resorted to in cases where there was a probability of success with other remedies.”

In bringing our article to a close, we will briefly sum up some of the conclusions arrived at:—

1st. The cholera, on its first outbreak, was more virulent in its attacks, and more likely to prove fatal, than it was after it had existed among us a short time.

2d. Its specific cause is believed to have been the existence in the atmosphere of some material substance, the nature of which we do not fully understand.

3d. We believe cholera, under certain circumstances, to be a contagious disease. Ordinarily, it did not prove itself such; but, when it existed in places where ventilation could not be had, its contagious character was then more fully shown.

4th. The smoke formed by the combustion of our bituminous coal afforded us partial protection from the ravages of cholera.

5th. The occurrence of various cutaneous pustular eruptions and abscesses appeared to give protection from cholera.

6th. Persons of foreign birth, the aged, feeble, and intemperate, suffered more severely from cholera than any other class of our citizens.

PITTSBURG, July 3, 1855.

We append to this report an account of the weather during the cholera period, as well as a tabular statement of meteorological observations taken during a period of two years, commencing April 1, 1853, by our fellow-townsmen, W. W. Wilson, Esq., who has politely furnished us with them. Table II. is of our own construction from the material supplied by him. The great heat and drought which prevailed during the months of June, July, and August of 1854, can readily be seen at a glance.

TABLE I.

Observations on the Weather taken during the Cholera periods, September, 1854.

1854.—Sept. 14.	Morning cloudy; evening, 6 o'clock, heavy clouds, lightning in N. W., wind from S. W. From 6½ to 8 o'clock, heavy rain, with vivid lightning. Amount of rain, .720 in.
“ “ 15.	A. M., sun very warm in the morning.
“ “ 16.	Clear.
“ “ 17.	“
“ “ 18.	“
“ “ 19.	A. M., cloudy, wind from N. W.; P. M., cloudy. From 5½ o'clock it rained three hours. Amount of rain, .520. Wind from S. W.
“ “ 20.	Clear.
“ “ 21.	“
“ “ 22.	“
“ “ 23.	“
“ “ 24.	“
“ “ 25.	“
“ “ 26.	“
“ “ 27.	“

TABLE II.

	Quantity of rain.	Mean height of barometer.	Mean tem- perature.		Quantity of rain.	Mean height of barometer.	Mean tem- perature.
1853.	Inches.	Inches.	Degrees.	1854.	Inches.	Inches.	Degrees.
April	3.640	28.89	48	April	3.590	28.93	47
May	4.297	29.02	61	May	2.320	28.96	60
June	1.853	29.22	73	June	.700	29.01	73
July	3.428	29.08	70	July	.723	29.12	79
Aug.	7.747	29.10	71	Aug.	.630	29.08	77
Sept.	2.411	29.06	63	Sept.	1.915	29.11	72
Oct.	2.048	29.14	48	Oct.	2.267	29.10	56
Nov.	3.320	29.12	44	Nov.	1.595	28.86	36
Dec.	1.575	29.00	28	Dec.	1.680	29.00	28
1854.				1855.			
Jan.	2.626	28.99	14	Jan.	.820	29.01	26
Feb.	2.875	29.00	34	Feb.	1.640	29.86	20
March	2.343	28.91	39	March	3.300	28.97	33
Means	3.180	29.04	49.4	Means	1.765	29.08	51.4

In the first twelve months of the above table, there were 83 days on which rain fell, whilst in the latter twelve there were but 57.

ART. IV.—*Statistics of two hundred and fifty-eight Cases of Intestinal Obstruction, with Remarks.* By S. FOSTER HAVEN, Jr., M. D.

THERE are some disorders of whose history we know very little, although their results are readily anticipated, and their medical management has been regarded as simple. It is to this class that cases of intestinal obstruction belong. Yet, in these cases, a correct diagnosis is the first and most indispensable requisite. When the state of facts has been ascertained, the treatment is usually matter of little doubt. Diagnosis, however, as a system, cannot be said to possess a reliable value, unless it rests upon a sufficient number of well-authenticated facts; and to the numerical system, chiefly, must we look for such an accumulation of phenomena as will constitute a solid basis for intelligent treatment. We will, therefore, offer no further apology for presenting the following pages—mostly a collection of statistical tables—believing that whatever tends to reduce a mass of isolated facts within the range of immediate observation cannot be altogether useless.

Where an individual has an opportunity to make his own records from a large number of cases of the same disease—as Louis has done—it is possible to produce a unique and complete history of that disease. But where, as in the disorder of which we propose to speak, a practitioner meets with comparatively few cases in a lifetime, such a result can be only approximately

obtained. The compiler is obliged to depend upon meagre reports, collected here and there, from a great variety of authors, and too often finds a long and laborious search rewarded with but negative conclusions.

We have succeeded in collecting—mostly from the French, English, and American periodicals of this century—a list of two hundred and fifty-eight cases of intestinal obstruction, taking only those which were well marked, and not including the varieties of inguinal, crural, and umbilical hernia. Well-marked cases of obstruction, however, generally turn out unfavourably, and hence our list will be found to contain only those that either proved fatal or were relieved by an operation, or such as may have undergone a spontaneous cure by the discharge of an intussuscepted portion.

In classifying the different species of obstruction, there is no arrangement, perhaps, that would not be open to some objections. Ours, adopted for convenience, must by no means be regarded as arbitrary. We propose to follow, in the main, that given by Mr. Pollock in the *Medico-Chirurgical Review* for October, 1853, making a few alterations, however, to suit our own purposes.

It will be convenient, in the first place, to make three general divisions of the various causes of intestinal obstruction, viz:—

1. Those originating in and implicating the mucous and muscular coats of the intestinal walls, which we will take the liberty to call *Intermural*.

2. *Extramural*, or those causes acting from without or affecting the serous covering.

3. *Intramural*, or obstructions produced by the lodgement of foreign substances.

Under the first head will be classed the following varieties:—

1. *a.* Cancerous stricture.

2. *b.* Non-cancerous stricture, comprising—

1. Contractions of cicatrices following ulceration.

2. Contraction of walls of intestine from inflammation, non-cancerous deposit, or injury.

3. *c.* Intussusception.

4. *d.* Intussusception associated with polypi.

Under the second—

5. *a.* Bands and adhesions from effusion of lymph.

6. *b.* Twists, or displacements.

7. *c.* Diverticula.

8. *d.* External tumours, or abscesses.

9. *e.* Mesocolic and mesenteric hernia.

10. *f.* Diaphragmatic hernia.

11. *g.* Omental hernia.

12. *h.* Obturator hernia.

Under the third—

13. *a.* Foreign bodies, calculi, &c.

We will take up the different varieties in succession, and consider their peculiarities under a tabular form.

TABLE I.—*Cancerous Obstructions.*

Whole number of cancerous obstructions	24
Males	15
Females	9
	<hr/>
	24

Average age of whole number	51 years.
“ males	49 “
“ females	53 “
Age of youngest male	28 “
“ oldest male	75 “
“ youngest female	30 “
“ oldest female	68 “

Part of Intestine affected.

Rectum was affected in	14 cases.
Transverse colon in	5 “
Descending colon in	3 “
Sigmoid flexure in	1 case.
Duodenum in	1 “
	<hr/>
	24

Whole number operated upon	8 cases.
“ not operated upon	16 “
	<hr/>
	24

Artificial anus was formed in	7 cases.
From which there recovered	6
And from which there died	1
	<hr/>
	7

Part of intestine was excised in	1 case.
Recovered in	1 “

Symptoms.

Complete constipation: average number of days in 10 cases	22
Vomiting was mentioned in	7 cases.
Fecal vomiting in	1 case.
No vomiting in	2 cases.
Constipation in	17 “
Diarrhœa in	1 case.
Retention of urine in	2 cases.
No retention of urine in	1 case.
Peritonitis, acute, in	4 cases. ¹
No peritonitis, acute, in	4 “
Abdominal distension in	8 “
Abdominal pain in	10 “
Tumour felt in	2 “
Gradual prostration in	8 “

¹ Two of these were from rupture or perforation.

Remarks.—The proportion of males to females, according to the above table, appears to be as 5 to 3.

The average age of the whole number is 51; a period somewhat beyond the middle time of life, and corresponding with what has been observed concerning cancerous growths in general.

The part of intestines the most often affected seems to be the rectum, a fact also well known, while the higher up we go the less frequent becomes the disease.

Vomiting is mentioned but seven times, and fecal vomiting but once; a circumstance readily explained by the fact that the obstructions are generally quite low down.

In two cases, retention of urine was spoken of. In one, the disease was seated at the angle of the transverse with the descending colon, while to this the jejunum had become adherent, and formed part of the foul cavity. In the other, the walls of the bladder itself were involved in the cancerous growth.

Prostration was mentioned as gradual in eight cases, and early in none. The slow progress and general wasting character of carcinomatous disease would account for this, as well as for the rare occurrence of acute peritonitis.

A tumour was felt in two instances, each time the transverse colon being the part affected; the locality, perhaps, where a tumour could be the most readily detected.

TABLE II.—*Non-cancerous Obstructions.*

Whole number of non-cancerous strictures	46
Males	18
Females	28
	<hr/>
	46
Average age of 40	43 years.
" 26 females	42 "
" 14 males	45 "
Age of youngest male	23 "
" oldest male	74 "
" youngest female	3 "
" oldest female	84 "
<i>Part of Intestine affected.</i>	
Rectum was affected in	23 cases.
Sigmoid flexure in	9 "
Cæcum in	4 "
Transverse colon in	3 "
Descending colon in	3 "
Ileum in	2 "
Ascending colon in	1 case.
Colon in	1 "
	<hr/>
	46

Large intestine	44 cases.
Small intestine	2 "
	<hr/>
	46
Whole number operated upon	11 cases.
" not operated upon	35 "
	<hr/>
	46
Artificial anus was formed in	10 cases.
Recovered	6
Died	4
	<hr/>
	10
Gastrotomy was performed in	1 case.
Died	1 "

Symptoms.

Complete constipation: average number of days in 14 cases	35
Constipation was mentioned in	36 cases.
Diarrhoea in	3 "
Constipation and diarrhoea in	2 "
Vomiting in	27 "
Fecal vomiting in	5 "
No fecal vomiting in	3 "
Nausea in	3 "
Abdominal pain in	25 "
Abdominal distension in	31 "
Retention of urine in	5 "
No retention of urine in	3 "
Dysuria in	1 case.
Acute peritonitis in	6 cases.
Gradual prostration in	6 "
Early prostration in	3 "
Tumour felt in	2 "

Remarks.—In this class of obstructions, the females seem to have predominated over the males in the proportion of 14 to 9; while the average age is less by eight years than in the previous class. Here, too, the rectum is decidedly the most often affected, and the small intestine very rarely. In only 4 cases out of the 46 was the stricture produced by ulceration.

According to Table II., the average number of days of complete constipation is 35. In two cases, however, the duration was very remarkable, being three and six months respectively.¹ In the former, profuse diarrhoea occurred a few days before death, and an autopsy showed the colon to be thickened by inflammation, producing contraction, and preventing action. In the latter case, the intestines were found enormously swollen, containing 30 or 40 lbs.

¹ Such is the positive statement made with regard to them. Our readers can form their own opinions. (Vide *Lancet*, 1837-8, p. 722; and *London Med. Gaz.*, vol. ii. p. 511, 1828.)

of matter resembling dry mud, and, on opening the abdomen, immediately burst, projecting their contents with considerable force. Taking out these two cases, the average number of days is reduced to 18.

Diarrhœa without constipation occurred in 3 instances, in two of which there was no vomiting, and in the third none is mentioned.

Prostration seems to have been less gradual, and abdominal distension much more frequent, than in the cancerous obstructions.

A tumour, felt in two instances at the seat of stricture, proved to be from a thickening of the cæcum.

TABLE III.—*Intussusception.*

Whole number of cases	59
Males	34
Females	10
Unknown	15
	<hr/> 59
Average age of 49 cases	18 years.
" 28 males	22 "
" 7 females	26 "
" 14 unknown	9 months.
	<hr/> 49
Age of youngest male	4 months.
" oldest male	47 years.
" youngest female	3 months.
" oldest female	65 years.
<i>Part of Intestine affected.</i>	
Ileum was affected in	12 cases.
Small-intestine in	9 "
Ascending and transverse colon in	9 "
Ascending, transverse, and descending colon in	7 "
Ascending colon in	4 "
Ileum and ascending colon in	4 "
Ileum and cæcum in	3 "
Descending colon in	3 "
Jejunum in	2 "
Colon in	2 "
Ascending, transverse, and descending colon in	1 case.
Transverse colon in	1 "
Unknown in	2 cases.
	<hr/> 59
Small intestine	23 cases.
Large intestine	11 "
Both intestines	23 "
Unknown	2 "
	<hr/> 59

Operated on by gastrotomy in	3 cases.
Recovered in	3 “
Invaginated portion passed per anum in	12 cases.
Recovered	10 “
Died	2 “
Retrograde intussusception in	3 cases.

Symptoms.

Duration of attack: average number of days in 19 cases	4
Complete constipation: “ “ 4 “	9
Constipation was mentioned in	41 cases.
Diarrhoea in	6 “
Constipation and diarrhoea in	2 “
No constipation in	1 case. ¹
Bloody stools in	22 cases.
Tenesmus in	9 “
Vomiting in	40 “
Fecal vomiting in	13 “
No vomiting in	4 “
Very little vomiting in	1 case.
Nausea in	3 cases.
Abdominal pain in	43 “
Abdominal distension in	13 “
Retention of urine in	4 “
No retention of urine in	3 “
Suppression of urine in	1 case.
Early prostration in	17 cases.
Gradual prostration in	6 “
Acute peritonitis in	4 “
Gangrene in	6 “
No acute peritonitis in	2 “
Tumour felt in	15 “
No tumour felt in	2 “
Convulsions in	6 “
Infant healthy in	13 “
Infant not healthy in	6 “

Remarks.—Here we find that the males take precedence of the females in the proportion of $3\frac{2}{3}$ to 1.

The average age, 18, is much younger than that of any other class, and confirms the common observation with regard to the frequency of intussusception in children. In 22 cases, it occurred under the age of one year.²

Intussusception of the small intestine was found twice as often as that of

¹ Retrograde intussusception of ileum, 12 inches. Patient died with intermittent fever.

² It may be well to state that these are all supposed to be real cases of intussusception, and not mere *post-mortem* changes such as are often found in infants.

the large, while invagination, involving both, occurred as frequently as that of the small intestine alone.

Discharge of the intestine per anum appears to be not uncommon, it being spoken of twelve times, with recovery in all but two.

Retrograde intussusception proves to be quite rare, but three cases being mentioned.

The duration of sickness is shorter in this form of obstruction than in any other, the average of 19 cases amounting to but 4 days. In this class, too, we have, as common symptoms, tenesmus and bloody stools, which are seldom mentioned elsewhere. Gangrene, also, is a pathological appearance more often found here than in other classes.

In the 12 cases where the invaginated portion was passed per anum, the average number of inches was $23\frac{1}{2}$. Only 2 died; the portions passed being, respectively, 39 and 44 inches.

The subject of intussusception has been more fully treated of by authors than any other branch of intestinal obstruction, excepting, of course, hernia. *Guy's Hospital Reports*, for 1838, contain an excellent article, by Mr. Gorham, on intussusception as it occurs in infants alone.

TABLE IV.—*Intussusception with Polypi.*

Whole number of cases	4
Males	2
Females	2
	<hr/> 4
Average age of 4	46 years.
" 2 males	$37\frac{1}{2}$ "
" 2 females	$54\frac{1}{2}$ "
Age of youngest male	29 "
" oldest male	46 "
" youngest female	34 "
" oldest female	75 "

The small intestine was affected in all.

None were operated upon. All died.

Symptoms.

Complete constipation: average number days in 4 cases	8
Duration of attack " " 1 case	11
Constipation was mentioned in	4 cases.
Vomiting in	2 "
Fecal vomiting in	1 case.
Abdominal pain in	1 "
Little abdominal pain in	1 "
Abdominal distension in	1 "
No retention of urine in	1 "
Acute peritonitis in	1 "
No acute peritonitis in	1 "
Tumour felt in	1 "

Remarks.—Our short table of intussusception with polypi shows an equal division of the sexes, with an average age older, and a duration of sickness longer, than in simple intussusception. The polypi were in each instance found in the small intestine, being described in one case as of the size and shape of a pear; in another $2\frac{3}{4}$ inches long; and in another, the size of a thumb, with a short pedicle.

The very rare occurrence and peculiar nature of this affection will, perhaps, render it desirable to give a fuller illustration from the details of an actual case. The following one, from the *Transactions* of the London Pathological Society, is quite interesting in several points of view:—

A man, æt. 46, was admitted into St. George's Hospital with symptoms of peritonitis, accompanied with diarrhœa, profuse perspiration, sleeplessness, and emaciation. The diarrhœa, which had existed for some months, was preceded by obstinate constipation of eight days' standing. The urgent symptoms were slightly relieved, but soon returned, and the patient died on the seventeenth day after admission.

Autopsy.—Extensive effusion of puriform fluid in peritoneal cavity, fetid, and apparently mixed with feculent matter. Peritoneum of intestine much thickened in many places, and of an ash-gray color. Small intestine much distended, especially at upper part. Omentum thickened and contracted, and, with a convolution of small intestine, adherent to parietal peritoneum of left lumbar region. After detaching the adhesions, an invagination was found in this part of the intestine, two feet above the cæcum. Enormous dilatation above the invagination, and slight contraction below. Within the intestine, connected with the intussuscepted portion, and depending from it, was a large polypus, pyriform, $2\frac{3}{4}$ inches long, $1\frac{1}{2}$ wide at the broadest part, and with a pedicle $1\frac{1}{2}$ inches long, of the size of the middle finger. The surface was quite smooth, texture livid, quite firm, presenting appearances of fibrous tissue, and plentifully supplied with bloodvessels. Microscopical examination proved it to be decidedly of a fibrous nature. The peritoneal surfaces of the invagination were firmly united, and this portion of intestine was dark and livid. The intestine immediately above presented, towards its mucous surface, a broad ulceration, occupying two-thirds of its diameter; here the gut easily gave way, when pulled upon, and allowed the contents to escape before it was laid open.

Mr. P. Hewett remarked to the Society, that this case presented a point of very great interest; he alluded to the existence of the broad ulceration just above the invagination—an ulceration by which the invaginating portion, with the polypus, might have been separated and passed by stool.¹

¹ *Vide Lancet*, Nov. 11, 1846, p. 534.

TABLE V.—*Bands and Adhesions.*

Whole number of cases	39
Males	21
Females	17
Unknown	1
	<hr/> 39

Average age of 32 cases	32 years.
" 18 males	31 "
" 14 females	33 "
Age of youngest male	11 "
" oldest male	65 "
" youngest female	6 "
" oldest female	57 "

Part of Intestine affected.

Ileum was mentioned in	29 cases.
Small intestine in	4 "
Jejunum in	1 case.
Cæcum in	1 "
Ascending colon in	1 "
Descending colon in	1 "
Rectum in	1 "
Whole intestine in	1 "
	<hr/> 39

Whole number operated on	6 cases.
" not operated on	33 "
	<hr/> 39

Gastrotomy was performed in	5 cases.
Recovered	0 "
Died	5 "
Artificial anus was formed in	1 case.
Recovered	1 "

Symptoms.

Complete constipation: average number of days in 17 cases	8
Duration of attack: " " 9 "	6½
Constipation was mentioned in	39 cases.
Constipation and diarrhoea in	2 "
Vomiting in	36 "
Fæcal vomiting in	20 "
Abdominal pain in	31 "
Abdominal distension in	16 "
No abdominal distension in	2 "
Retention of urine in	2 "
No retention of urine in	3 "
Suppression	2 "

Acute peritonitis	12 ¹ cases.
No acute peritonitis	4 “
Early prostration	9 “

Remarks.—Not much disproportion between the sexes is apparent in this class, nor is the average age remarkable.

In but 4 cases out of 38 was the large intestine strangulated; affording a strong contrast to the cancerous and non-cancerous strictures, and to the obstructions by displacement (Table VI.). In the first of these, the rectum was surrounded and strangulated by a dense cartilaginous ring. In the second, the ascending colon was bound down to the bladder. In the third, one of the appendices epiploicæ was coiled round the descending colon, and the latter adherent to the fundus and side of the uterus. In the fourth, the omentum adhered to ileum, colon, and uterus, and the latter, contracting after delivery, probably strangulated the cæcum.

Obstruction occurred in eight instances, from an adherence of the vermiform appendage causing a constriction of the ileum.

The numerous convolutions and opposed surfaces of the small intestines will account for the frequency of strangulation by bands in this part.

The average duration of sickness appears to be quite brief, gradual prostration having been mentioned in no instance.

Perhaps this class of obstructions presents more frequently than any other those symptoms which stand as the especial characteristics of internal strangulation. Constipation occurred in every case; vomiting is noted in all but three; and fecal vomiting in one-half.

TABLE VI.—*Twists or Displacements.*

Whole number of cases	18
Males	16
Females	2
	<hr/> 18
Average age of 15	35 years.
“ 13 males	38 “
“ 2 females	33 “
Age of youngest male	20 months.
“ oldest male	65 years.
“ youngest female	27 “
“ oldest female	40 “

Part of Intestine affected.

Sigmoid flexure was mentioned in	14 cases.
Ileum in	3 “
Transverse colon in	1 case.
	<hr/> 18

¹ One of these was from perforation.

Died from gastrotomy.	1 case.
Not operated on	17 cases.
	<hr/> 18

Symptoms.

Complete constipation: average number of days in 10 cases	6
Duration of attack: " " 8 "	5½
Constipation was mentioned in	18 cases.
Diarrhœa at first in	2 "
Vomiting in	9 "
Fœcal vomiting in	1 case.
No vomiting in	3 cases.
Nausea in	2 "
Retention of urine in	2 "
No retention of urine in	1 case.
Frequent micturition in	1 "
Abdominal pain in	14 cases.
Abdominal distension in	10 "
No abdominal distension in	1 case.
Acute peritonitis in	7 cases.
Early prostration in	10 "
Gradual prostration in	1 case.

Remarks.—The proportion of males to females is here quite remarkable, being as 8 to 1. We know of no cause for this, other than the fact that the greater bodily activity of men, and their more frequent exposure to sudden strains, render them more liable to any displacement of the abdominal viscera. The ten cases where occupation is mentioned, all come under the head of laborious professions, three of them being soldiers.

In only two instances, is any apparently immediate cause for the obstruction spoken of. In one, the symptoms manifested themselves after a frequent jumping up and down from a hay-cart; and in the other, that of a soldier, they came on after much exertion from marching, followed by a rather glutinous meal.

The average age, 35, does not appear to be very young nor very old, but about as might be expected.

The sigmoid flexure claims to be the part by far the most often affected, being over all others in the proportion of 7 to 2, owing, probably, to its lying more loosely in the abdominal cavity than any other part of the intestine, and to the readiness with which it may be pushed out of place and pressed upon by the weight of the descending intestines above. It was most generally found twisted over to the right side.

The ileum was affected but three times, in one of which the patient remarked, a few days before death, that he felt as if his intestines were "tied in a knot." A *post-mortem* revealed the small intestine tied in a double bow-knot, involving twelve or fifteen feet.

The average number of days of complete constipation, and duration of attack seems to be quite small.

Prostration is spoken of as sudden in five cases; as early in five; and as gradual in but one.

Constipation occurred in every instance.

TABLE VII.—*Diverticula.*

Whole number of cases	10
Males	9
Females	1
	<hr/> 10

Average age of 8 cases	30 years.
" 7 males	33 "
" 1 female	27 "
Age of youngest male	14 "
" oldest male	72 "

Part of Intestine affected.

Ileum was mentioned in	9 cases.
Small intestine in	1 case.
	<hr/> 10

Symptoms.

Complete constipation: average number of days in 4 cases	6
Duration of attack: " " 3 "	4
Constipation was mentioned in	10 cases.
Constipation and diarrhoea in	1 case.
Vomiting in	9 cases.
Fecal vomiting in	3 "
Abdominal pain in	6 "
Very little abdominal pain in	2 "
Abdominal distension in	2 "
No abdominal distension in	1 case.
Retention of urine in	1 "
Acute peritonitis in	4 cases.
Early prostration in	6 "
Gradual prostration in	2 "
None were operated on.	

Remarks.—Why diverticula should possess such a preference for males, we cannot say; but out of the whole number, 10, there appears but one case of a female.

The average age, 30, is rather young.

In every instance, the small intestine was the part affected, and, probably, in every one the ileum.

Four of the cases had the diverticula adherent to the mesentery. In one, it was attached to the peritoneum, at the back part of the abdomen; in one;

to the brim of the pelvis; in one, to the obliterated omphalo-mesenteric duct; and in one the diverticulum contained 'orange-pips, cherry-stones, &c., being $3\frac{1}{2}$ inches long, as broad at base as the intestine, and increased in size towards the extremity. The average number of days of complete constipation, and duration of attack, shows a short period.

TABLE VIII.—*External Tumours or Abscesses.*

Whole number of cases	5
Males	2
Females	2
Unknown	1
	<hr/> 5
Average age of 4 cases	31 years.
" 2 males	47 "
" 2 females	15 "
Age of youngest male	46 "
" oldest male	49 "
" youngest female	5 "
" oldest female	25 "

Part of Intestine affected.

Jejunum was mentioned in	1 case.
Small intestine in	1 "
Cæcum in	1 "
Ascending colon in	1 "
Duodenum in	1 "
	<hr/> 5
Whole number not operated on	4 cases.
Gastrotomy was performed in	1 case.
	<hr/> 5
Died	1 case.

Symptoms.

Complete constipation: number of days in 1 case	17
Duration of attack: average 2 cases	323
Constipation was mentioned in	5 cases.
Constipation and diarrhoea in	1 case.
Little constipation in	1 "
Vomiting in	5 cases.
Fecal vomiting in	1 case.
Abdominal pain in	3 cases.
No retention of urine in	1 case.
Gradual prostration in	3 cases.
Tumour felt in	3 "

Remarks.—Obstructions from external tumours prove to be quite limited in number. But 5 cases were found out of the 258.

No. 1 was an encysted tumour of the mesentery, containing 32 ounces of fluid, and pressing upon the jejunum.

No. 2. A large tumour in the left iliac region enveloping a considerable portion of the small intestine and mesentery.

No. 3. A scirrhus mass attached to the pelvis, and obstructing the cæcum.

No. 4. Encephaloid of pancreas pressing upon the duodenum. Encephaloid of the pancreas, according to writers, is of very rare occurrence,¹ and, taken in connection with the fact of its causing obstruction, renders this case an exceedingly interesting one.

No. 5. Enlarged gland within mesocolon obstructing ascending colon.

The average duration of sickness would of course be long, from the general slow growth of tumours.

TABLE IX.—*Mesocolic and Mesenteric Hernia.*

Whole number of cases	6
Males	5
Female	1
	<hr/> 6

Average age of 6 cases	29 years.
“ 5 males	33½ “
“ 1 female	24 “
Age of youngest male	20 “
“ oldest male	65 “

Part of Intestine affected.

Ileum was mentioned in	3 cases.
Small intestine in	2 “
Colon in	1 case.
	<hr/> 6

None were operated upon. All died.

Symptoms.

Complete constipation: average number of days in 2 cases	7½
Duration of attack: “ “ 3 “	6
Constipation was mentioned in	5 cases.
Vomiting in	5 “
Fecal vomiting in	2 “
Abdominal pain in	5 “
Abdominal distension in	2 “
Early prostration in	3 “

Remarks.—The same causes that render twists and displacements more

¹ Dr. Tanchou collected from the Paris registers 9,118 cases of death from cancerous affections (using this term in its widest sense). Out of this number, cancer of the pancreas was observed but twice. (Warren's edition of Walshe on Cancer.) Dr. J. B. S. Jackson, however, informs us, personally, that he has met with six or seven cases.

frequent in males, probably operate here in giving a proportion of 5 to 1; or, rather, this class might be considered as but a subdivision of the former. We accordingly find a similar brevity of duration, and nearly the same frequency of symptoms, in both.

Mesocolic hernia occurred four times, and mesenteric hernia twice.

In No. 1, a knuckle of colon, 12 inches long, was strangulated through an aperture in the mesocolon the size of a quarter of a dollar.

In No. 2, no small intestine could be seen on opening the body. It was found concealed in the mesocolon. This patient, however, died of typhus, and not from obstruction.¹

In No. 3, the descending colon was pushed to the right side, lying close to the cæcum. The small intestine was found contained in a sac formed in the folds of the mesocolon. Out of this sac, the ileum appeared for two inches above the cæcum, where it was contracted, thickened, and gangrenous.

In No. 4, a knuckle of the ileum was strangulated in the mesocolon.

In Nos. 5 and 6, strangulation of the ileum was produced through an aperture in the mesentery.

TABLE X.—*Diaphragmatic Hernia.*

Whole number of cases	8
Males	6
Females	2
	<hr/>
	8
Average age of 5 cases	41 years.
" 4 males	45 "
" 1 female	23 "
Age of 2 males unknown	adults.
" 1 female unknown	aged.
" youngest male	23 years.
" oldest male	74 "

Part of Intestine affected.

Transverse colon was mentioned in	4 cases.
Descending colon in	1 case.
Transverse and descending colon in	2 cases.
Small intestine in	1 case.
	<hr/>
	8
Whole number large intestine	7 cases.
" small intestine	1 case.
	<hr/>
	8

None operated on. All died.

¹ We have admitted into our list this case—not properly one of obstruction—for the sake of showing a fine instance of mesocolic hernia.

Symptoms.

Average duration of attack in 5 cases	3 $\frac{3}{4}$ days.
Number of days of complete constipation in 1 case	9 "
Constipation was mentioned in	2 cases.
Diarrhœa in	1 case.
Vomiting in	4 cases.
Fecal vomiting in	2 "
Nausea in	2 "
Abdominal pain in	4 "
Abdominal distension in	1 case.
No abdominal distension in	1 "
Pain in left side in	1 "
Dyspnœa in	5 cases.
Fever in	2 "
Early prostration in	3 "

Remarks.—As this also comes under the class of displacements, the proportion of 1 female to 4 males is readily accounted for.

The average age of 5, 41, seems to be tolerably high; but we have not cases enough to establish any definite conclusions.

From its proximity, the transverse colon might be expected to be most frequently implicated, and such is found to be the case.

The average duration of attack, only 3 $\frac{3}{4}$ days, seems to be quite short.

Under this class, we have two new symptoms frequently manifesting themselves—dyspnœa and fever—readily explained by the encroachments made upon the thoracic organs.

It may be well to give an abstract of the *post-mortem* appearances found in each case.

No. 1. A loop of 14 inches of transverse and descending colon passed through an opening in the cordiform tendon into the left pleural cavity.

No. 2. Stomach, transverse, and part of descending colon found in left thoracic cavity, through a fissure in muscular part of diaphragm.

No. 3. Omentum and portion of transverse colon strangulated through an aperture in diaphragm.

No. 4. Descending colon, omentum, spleen, and pancreas in pleural cavity, through a small hole in posterior part of diaphragm. Lung compressed to size of fist.

No. 5. Transverse colon, omentum, and pyloric half of stomach in left thoracic cavity, through a foramen a little anterior to the cardia. Lung very much compressed.

No. 6. Large portion of intestines found in left pleural cavity, crowding heart to the right.

No. 7. Stomach, omentum, and transverse colon found in thorax.

No. 8. Stomach, transverse colon, and omentum in left chest.

Causes.—In No. 1, the patient fell down and fractured tenth and eleventh ribs. In No. 3, the patient was wounded several years before with a knife,

and diaphragm probably lacerated. In No. 5, the patient was at full period of pregnancy. In No. 6, it was supposed to be congenital. In No. 8, the patient injured chest about one year before by a fall. The immediate cause of obstruction was probably distension of stomach from a considerable quantity of cold acidulated drink.

We find the left pleural cavity affected four times, and the right none.

Dr. Bowditch has written a monograph on the subject of diaphragmatic hernia—it being a numerical analysis of all the cases on record—which, of course, stands as authority in regard to this malady. Phrenic hernia, however, is of very rare occurrence,¹ and, though Dr. B.'s industry has collected 88 cases, yet most of them, we should think, must be necessarily very imperfect and unsatisfactory.²

TABLE XI.—*Omental Hernia.*

Whole number of cases	3
Males	3
Average age of 2 cases	55½ years.
Age of youngest	28 “
“ oldest	83 “

Part of Intestine affected.

Ileum was mentioned in	2 cases.
Small intestine in	1 case.
	<hr/> 3

None operated on. All died.

¹ It is said to be of more frequent occurrence in animals, especially in horses, probably from the horizontal direction of their bodies, and the relative position of the internal organs. Dr. Warren has in his collection the skeleton of a lion, whose death was occasioned by diaphragmatic hernia, and Dr. Bowditch has shown us the MS. notes of 12 cases of the same occurring in 5 horses, 1 mule, 3 dogs, 2 sheep, and 1 baboon.

² Diaphragmatic hernia, of course, does not necessarily imply intestinal obstruction, with which alone we have to do. The following table of causes, however, from Dr. Bowditch's work, may not be uninteresting.

It was congenital in	26 cases.
It was from drunkenness and debaucheries in	3 “
“ bullet wound in	3 “
“ stabs with sword or lance in	9 “
“ “wounds” in	3 “
“ absence of diaphragm in	1 case.
“ falls in	13 cases.
“ labour pains in	2 “
“ sudden strain in	1 case.
“ blows in	4 cases.
“ being run over in	2 “
“ fracture of the rib in	1 case.
	<hr/> 68 cases.

Symptoms.

Complete constipation, 5 days, occurred in	1 case.
Constipation was mentioned in	3 cases.
Vomiting in	1 "
Abdominal pain in	1 "
Abdominal distension in	1 "
No tumour felt in	1 "
Retention of urine in	1 "
Acute peritonitis in	1 ¹ "
Early prostration in	1 "

Remarks.—The statistics of this kind of obstruction are so few that, beyond the fact of its great rarity, no particular conclusion can be arrived at.

In each case, the small intestine was strangulated through a loop or rent in the omentum.

TABLE XII.—*Obturator Hernia.*

Whole number of cases	11
Females	10
Unknown	1
	<hr/> 11

Average age of 10 cases	67 years.
“ 9 females	53½ “
Age of 1 unknown	80 “
“ youngest female	30 “
“ oldest female	70 “

One female was mentioned as aged.

Part of Intestine affected.

Ileum was mentioned in	6 cases.
Small intestine in	3 “
Unknown	2 “
	<hr/> 11

Whole number not operated on	7 cases.
Died	7 “
Whole number operated on	4 “
Recovered	3 “

Gastrotomy was performed in	1 case.	Died.
Incision made below Poupart's ligament in	1 “	
Reduced by taxis in	2 cases.	
	<hr/> 4	

Symptoms.

Complete constipation: average number of days in 5 cases	10
Duration of attack: “ “ 4 “	10

¹ From perforation.

Constipation was mentioned in	10 cases.
Vomiting in	9 “
Fecal vomiting in	5 “
Diarrhœa at first in	1 case.
Abdominal pain in	7 cases.
Abdominal distension in	2 “
Retention of urine in	2 “
Early prostration in	2 “
Acute peritonitis, with gangrene, in	3 “
Hernia of right side in	1 case.
Hernia of left side in	2 cases.
Married female in	5 “
Unmarried	1 case.

Remarks.—In the causes of obstruction previously considered, we have found that males have generally prevailed in point of numbers. Here, however, we have a class where 10 cases out of 11 belong to the weaker sex, while the remaining one, unknown, was probably also a female.

What may be the cause of this? The changes which take place in the female pelvis, connected with pregnancy, would, of course, increase the tendency to crowd the intestine into this opening. In 5 cases the women were married, and but 1 was spoken of as single; nearly all, therefore, may be presumed to have been wives, particularly as none were under 30. The average age, however, 67, being past the child-bearing period, it would be necessary to presume a long-standing hernia in a good portion of the cases, to account for it by the pressure of a gravid uterus. It is at this advanced age, and after several pregnancies, that a descent of the small intestine into the pelvis most frequently occurs.

Out of 83,584 patients relieved by the London Truss Society, but 1 male and 4 females had obturator hernia.¹ Of 457 herniæ examined by M. Cloquet, 10 were obturator—7 occurring on the right side, and 3 on the left, and all being females but two.²

In our table, we find that one had gastrotomy performed and died; two were relieved by taxis, and a third by an incision below Poupart's ligament.

It would seem that the diagnosis of this form of obstruction is not necessarily very difficult. Only its infrequency can have prevented its detection in most instances, except, perhaps; where the patient happened to be very fleshy.

The small intestine, and probably the ileum, was strangulated in every instance.

TABLE XIII.—*Foreign Bodies, Calculi, &c.*

Whole number of cases	15
Males	4
Females	11
	<hr/> 15

¹ Lawrence on Hernia.

² Ibid.

Average age of 12 cases	26 years.
“ 2 males	16 “
“ 10 females	36 “
Age of youngest male	11 “
“ oldest male	21 “
“ youngest female	18 “
“ oldest female	63 “

Part of Intestine affected.

Transverse colon was mentioned in	3 cases.
Descending colon in	2 “
Jejunum in	2 “
Duodenum in	2 “
Duodenum and jejunum in	1 case.
Ileum in	1 “
Cæcum in	1 “
Rectum in	1 “
Unknown in	2 cases.

15

Not operated on	14 cases.
Operated on for artificial anus	1 case. Died.

15

The causes of obstruction were calculi in	11 cases.
The cause of obstruction was fish-bone in	1 case.
“ “ plum-stone in	1 “
“ “ vertebra of a bird in	1 “
“ “ impacted hair and string in	1 “

15

Symptoms.

Complete constipation: average number of days in 5 cases	10½
Duration of attack: “ “ 5 “	12
Constipation was mentioned in	10 cases.
Diarrhœa in	2 “
Diarrhœa and constipation in	1 case.
Vomiting in	13 cases.
Fecal vomiting in	5 “
Abdominal pain in	9 “
Little abdominal pain in	1 case.
Abdominal distension in	6 cases.
Retention of urine in	1 case.
No retention of urine in	2 cases.
Acute peritonitis in	3¹ “
No acute peritonitis in	1 case.
Tumour felt in	4 cases.
Early prostration in	4 “
Gradual prostration in	4 “

¹ Two of these were from perforation.

Remarks.—Females seem to have predominated here a little, but not enough to make it significant. The large and small intestines were about equally often the seat of obstruction.

Calculi prove to be decidedly the most common cause.

The average age is rather young than otherwise.

TABLE XIV.—*Intermural Obstructions.*

Whole number of cases	133
Males	69
Females	49
Unknown	15
	<hr/> 133
Average age of 117 cases	39½ years.
“ 59 males	38 “
“ 44 females	44 “
“ 14 unknown	9 months.
Age of youngest male	4 “
“ oldest male	75 years.
“ youngest female	3 months.
“ oldest female	84 years.

Part of Intestine affected.

Small intestine was mentioned in	30 cases.
Large intestine in	78 “
Both intestines in	23 “
Unknown in	2 “
	<hr/> 133

Artificial anus was formed in	17 cases.
Recovered	12
Died	5
	<hr/> 17

Gastrotomy was performed in	5 cases.
Recovered	4
Died	1
	<hr/> 5

Symptoms.

Complete constipation: average number of days in 32 cases	18½
Duration of attack: “ “ 20 “	7½
Constipation was mentioned in	98 cases.
Diarrhœa in	10 “
Diarrhœa and constipation in	4 “
No constipation in	1 case. ¹

¹ Vide Table III. Note.

Tenesmus in	9 cases.
Vomiting in	76 "
Fecal vomiting in	20 "
No vomiting in	9 "
Very little vomiting in	1 case.
Nausea in	6 cases.
Abdominal pain in	79 "
Very little abdominal pain in	2 "
Abdominal distension in	53 "
No abdominal distension in	1 case.
Retention of urine in	11 cases.
No retention of urine in	8 "
Dysuria in	1 case.
Suppression of urine in	1 "
Peritonitis, acute, in	15 cases. ¹
No peritonitis, acute, in	7 "
Tumour felt in	20 "
Early prostration in	20 "
Gradual prostration in	20 "
Convulsions in infants in	6 "
Infant healthy in	13 "
Infant not healthy in	6 "
Married female in	12 "

TABLE XV.—*Extramural Obstructions.*

Whole number of cases	100
Males	61
Females	35
Unknown	4
	<hr/> 100
Average age of 82 cases	39 years.
" 51 males	38 $\frac{2}{3}$ "
" 30 females	30 "
" 1 unknown	80 "
Age of youngest male	20 months.
" oldest male	83 years.
" youngest female	5 "
" oldest female	70 "

Part of Intestine affected.

Small intestine was mentioned in	68 cases.
Large intestine in	29 "
Both intestines in	1 case.
Unknown in	2 cases.

100¹ Two of these were from perforation or rupture.

Whole number operated on	10 cases.
Artificial anus was formed in	1 case.
Recovered	1
Gastrotomy was performed in	9 cases.
Recovered	1 ¹
Died	8
	<hr/> 9

Symptoms.

Complete constipation: average number of days in 41 cases	9
Duration of attack: " "	30 " 55
Constipation was mentioned in	90 cases.
Diarrhœa in	1 case.
Diarrhœa and constipation	7 cases.
Diarrhœa at first in	3 "
Little constipation in	1 case.
Vomiting in	78 cases.
Fecal vomiting in	34 "
No vomiting in	3 "
Nausea in	4 "
Abdominal pain in	71 "
Very little abdominal pain in	4 "
Abdominal distension in	34 "
No abdominal distension in	5 "
Retention of urine in	8 "
No retention of urine in	5 "
Suppression of urine in	2 "
Acute peritonitis in	27 ² "
No acute peritonitis in	5 "
Tumour felt in	3 "
Early prostration in	34 "
Gradual prostration in	6 "
Married female in	13 "

TABLE XVI.—*Total.*

Whole number of cases	258
Males	147
Females	92
Unknown	19
	<hr/> 258
Average age of 220 cases	38 years.
" 117 males	38 "
" 88 females	38 "
" 15 unknown	40 "

¹ Incision made below Poupart's ligament.² One of these was from perforation.

Age of youngest male	4 months.
“ oldest male	83 years.
“ youngest female	3 months.
“ oldest female	84 years.

Part of Intestine affected.

Small intestine was mentioned in	106 cases.
Large intestine in	119 “
Large and small intestines in	24 “
Unknown in	9 “

258

Whole number operated on	39 cases.
“ not operated on	219 “

258

Artificial anus was formed in	24 cases.
Recovered	18
Died	6

24

Gastrotomy was performed in	14 cases.
Recovered	5
Died	9

14

Incision below Poupart's ligament	1 case.
Recovered	1

Symptoms.

Complete constipation: average number of days in 83 cases	18
Duration of attack: “ “ 56 “	22
Constipation was mentioned in	208 cases.
Diarrhœa in	13 “
Diarrhœa and constipation in	12 “
Little constipation in	1 case.
No constipation in	1 ¹ “
Bloody stools in	22 cases.
Tenesmus in	9 “
Vomiting in	173 “
Fecal vomiting in	61 “
No vomiting in	12 “
Nausea in	12 “
Very little vomiting in	1 case.
Abdominal pain in	164 cases.
Very little abdominal pain in	6 “
Abdominal distension in	98 “
No abdominal distension in	7 “

¹ Vide Table III. Note.

Little abdominal distension in	1 case.
Tumour felt in	27 cases.
No tumour felt in	2 "
Retention of urine in	20 "
No retention of urine in	15 "
Suppression of urine in	3 "
Dysuria in	1 case.
Acute peritonitis in	45 cases. ¹
No acute peritonitis in	13 "
Gangrene (in intussusception and obturator hernia) in	9 "
Early prostration in	58 "
Gradual prostration in	31 "
Healthy infants in	13 "
Not healthy infants in	6 "
Convulsions of infants in	6 "
Females married in	25 "

For convenience of reference, we have arranged the following table, which comprehends nearly all the items of the preceding ones, excepting the symptoms:—

TABLE XVII.

	Whole number.	Males.	Females.	Unknown.	Average age.	Operated on.	Not operated on.	Artificial anus.	Recovered.	Died.	Gastrotomy.	Recovered.	Died.	Total recovered.	Total died.	Small intestine.	Large intestine.	Both.	Unknown.	Total.
Cancerous stricture	24	15	9	..	51	8	16	7	6	1	1	1	..	7	16	1	23	24
Non-cancerous stricture	46	18	28	..	43	11	35	10	6	4	1	..	1	6	40	2	44	46
Intussusception	59	34	10	15	18	3	56	3	3	..	13	46	23	11	23	2	59	
Intussusception with polypi	4	2	2	..	46	..	4	4	4	4	
Total of intermural	133	69	49	15	39½	22	111	17	12	5	5	4	1	26	106	30	78	23	2	133
Bands and adhesions	39	21	17	1	32	6	33	1	1	5	..	5	1	28	34	4	1	..	39	
Twists or displacements	18	16	2	..	35	1	17	1	..	1	..	18	3	15	18	
Diverticula	10	9	1	..	30	..	10	10	10	10	
External tumours or abscesses	5	2	2	1	31	1	4	1	..	1	5	3	2	5	
Mesocolic and mesenteric hernia	6	5	1	..	29	..	6	6	5	1	6	
Omental hernia	3	3	55½	..	3	3	3	3	
Obturator hernia	11	..	10	1	67	2	9	22	12	1	3	8	9	11	
Diaphragmatic hernia	8	6	2	..	41	..	8	8	1	7	8	
Total of extramural	100	61	35	4	39	10	90	1	1	92	1	8	4	96	68	20	1	2	100	
Intramural	15	11	4	..	26	1	14	1	..	1	15	6	7	15	
Unknown	10	6	4	..	47	6	4	5	5	..	1	1	..	6	4	2	5	..	10	
Total	258	147	92	19	38	39	219	24	18	6	152	62	9	39	219	106	119	24	9	258

Concluding Remarks.—We have now gone through with the different species of obstruction found in our 258 cases. It only remains to consider them in the aggregate, and draw such inferences as may prominently suggest themselves.

We find that intermural obstructions exceed the other two in number by 18, and the extramural alone by 33; while the obstructions from foreign

¹ Five of these were from perforation or rupture.

² In one out of these cases an incision was made below Poupart's ligament.

bodies are to the other two in the proportion of 1 to 15½. Intussusception occurs more frequently than any other species; non-cancerous obstructions, and those from external bands and adhesions, stand nearly on a par; while all the other varieties are considerably below in point of numbers, cancerous obstructions holding the next rank. Omental hernia is found the least often of all.

The proportion of males to females proves to be about as 1 to 1½.

39 out of the whole number were operated upon. 24 had an artificial anus formed in the side, two-thirds of whom recovered. This would seem to be an argument in favour of operating when the diagnosis is tolerably clear; but there are so many things to cause hesitation and delay, that the right moment is rarely taken advantage of. On the other hand, gastrotomy was performed 14 times, and but 5 recovered. The first of these five occurred in a case of cancer of the sigmoid flexure. Three inches of the diseased intestine were cut away, the arteries of the mesocolon tied, the extremities of the divided bowel united with Glover's suture, and the abdomen sewed up. Ten days after, a copious evacuation took place, and in eighteen days the patient was pronounced well. Twelve months after, however, he died from an extension of the disease. In the next three cases, the abdomen was opened and an intussusception withdrawn.¹ In the last case, the seat and kind of obstruction were not mentioned. Our statistics, then, do not offer much inducement for the operation of gastrotomy. It is surprising, however, what inroads upon the abdominal cavity may sometimes be made without causing death, as is shown in the first case above mentioned, and in many experiments upon animals.²

Notwithstanding the unsuccessful results of gastrotomy, the following question has occurred to us as deserving some attention. If a diagnosis could be made out in cases of simple twist, or of simple strangulation of knuckles of intestine through loops, rings, openings, or under bands, how far should we be justified in performing abdominal section? Evidently, in cases of twists, the answer would depend much upon the relative frequency of peritonitis; and in those of knuckles it would depend upon the frequency of peritonitis, adhesions, thickening, disorganization, &c., preventing a withdrawal of the incarcerated portion. In view of this, we have drawn up the following table of 34 cases:—

¹ These must be regarded as remarkable and exceptional cases, as it is well known that in intussusception the adhesions are generally too strong to permit withdrawal.

² "Brunner sewed a wound one inch and a half long, in the small intestine of a dog, with the glover's suture. The dog soon recovered; but it is difficult to say what he would not have recovered from, as, subsequently, milk was injected into his thorax, his femoral artery was tied, his spleen was extirpated, his pancreas cut away, and, finally, he was compelled to swallow a scruple of opium—and all without serious consequences, as he made his escape three months after the last attempt upon his life."—*Brit. and For. Med. Rev.*, Jan. 1847.

TABLE XVIII.

	Knuckles. Twists. Total.		
No. of cases where there was no peritonitis, and where it seems probable that the intestine might have been withdrawn or untwisted	7	6	13
No. of cases rather more doubtful, but where no mention was made of peritonitis or other obstructing cause	10	0	10
No. of cases where peritonitis was mentioned	4	3	7
No. of cases where the intestine could not be withdrawn	4	0	4
Total	25	9	34

Considering that a large portion, if not all, of the 10 more doubtful cases, in the second line of figures, *may* have properly belonged in the first line, and that peritonitis in all the 7 cases *may* not have existed two or three days before death, there remain but 4 cases out of the 34 where we are certain that no operation could have been performed. In one instance, peritonitis was spoken of as very slight; and, in another, it resulted from perforation. Indeed, it does not appear, from the preceding tables, that peritonitis is of very frequent occurrence in any class of intestinal obstruction.

We will leave these figures to speak for themselves, and let others judge of their value when brought to a practical bearing.

In intermural obstructions, the large intestine was affected more than twice as often as the small, while in the extramural it was just the reverse; thus rendering the aggregate nearly equal in this respect. The average duration of attack seems to be very much shorter in the intermural obstructions than in the extramural, that of the whole being about three weeks.

Rokitansky, perhaps the ablest writer on this subject, states that obstructions occurring from the causes enumerated under the different heads of bands and adhesions, diverticula, mesocolic and omental hernia, are most frequent in females, from the liability which the internal sexual organs in women have to contract adhesions. Our tables, however, do not go to confirm this opinion, but rather show a proportion to the contrary. In diverticula, especially, there occurs but one woman to nine men. Rokitansky also observes that intussusception affects equally the large and small intestines; but here, too, our statistics seem inclined to disagree, and give the preference to the small intestine.

In regard to the symptoms—constipation, vomiting, and abdominal pain and distension are the most prominent. Diarrhœa occasionally happens. Bloody stools, tenesmus, and convulsions are peculiar to intussusception, and generally to infants.

Retention and suppression of urine occurred about equally in obstructions of the large and small intestine, most frequently in those of the ileum and rectum. In the latter case, probably, the retention was most generally due to the vicinity of the obstruction to the bladder, and in the former, the dimi-

nution or suppression may have been occasioned by the high seat of stricture, and the consequent early interruption of nutrition.

Early prostration occurs decidedly more often than gradual.

Acute peritonitis is not mentioned as often as we might at first be led to expect; a circumstance exceedingly favourable in the question of operation.

Gangrene is found most frequently in intussusception and obturator hernia. We have not made any statistics of it in the other classes, though it may have been mentioned.

Strangulation is said sometimes to occur from an adherence of the appendix cæci in consequence of inflammation within that organ. In none of the eight cases of obstruction from such an adhesion was this inflammation mentioned. Since completing the list, however, we have met with one case of this sort, reported in the proceedings of the Boston Medical Improvement Society.

Three other forms of intestinal obstruction are mentioned by Sir Astley Cooper: hernia at the ischiatic notch, at the foramen Winslowii, and periæcal hernia; but, as no instances of these were found in our list of cases, we have made no recognition of them in the preceding pages.

It is much to be regretted that medical men have not taken more pains, in reporting their cases of obstruction, to give the symptoms in full, and, particularly, to give them in their order of occurrence. Thus, it is especially desirable, as a diagnostic sign, to know whether stercoraceous vomiting commenced early or late. If the former, we should be led to place the seat of obstruction high up, but low down, if the latter. The nine instances mentioned of early fecal vomiting, though good evidence, as far as they go, are by no means numerous enough to admit of drawing any solid deductions from them. Retention of urine is another interesting symptom rather rarely noted in our list, but which would be exceedingly useful in localizing the strictured part.

Unquestionably, many other numerical deductions might be made from our tables, but the most prominent and important ones are given, and a simple reference to the tables themselves will exhibit the others in a plainer and more concise form than we could detail them in elsewhere.

The subject of treatment, save a few statistics of operations, has not been referred to, nor is it appropriate to the particular purpose of this paper.

Finally, we would beg leave to remark that, though the preceding pages may not have brought to light any positively new facts, yet the facts themselves may be found to be presented in a form, and under combinations, which may be more convenient for reference.

ART. V.—*Medical Notes on Fort Merrill and Ringgold Barracks, Texas; from Reports to the Surgeon General U. S. Army.* By ISRAEL MOSES, M. D., of New York, late Assistant Surgeon U. S. Army.

HAVING received orders to report for duty in the department of Texas, I arrived at Corpus Christi, head-quarters, June 11, 1854, in a small boat, having left our steamer at the outer bay. This place, famous as the camp of the "army of occupation" before the battles of the 8th and 9th of May, consists of some thirty or forty houses of various sizes and degrees of comfort, built of adobe, or framed, and occupied as officers' quarters, trading houses, and drinking shops. These buildings are arranged around an indentation of the bay, on a low sand beach, with an occasional musquite, or Spanish bayonet, to relieve the dreary waste.

The weather was excessively hot during the ten days I remained there; sea-breezes, however, springing up about 9 A. M., and, blowing until sunset, somewhat relieve the scorching rays of the sun, while the nights are almost insupportable from heat and insects.

June 20th, I started for Fort Merrill, sixty miles west, on the river Nueces. The country is rolling, covered with fine grass and motts of musquite and oak, chapparal, and cactus—singly, or scattered in patches. There were great numbers of deer, partridges, and rabbits; mosquitoes were very troublesome, especially the first night, during which not an hour's sleep was obtained, though we had a mosquito-bar. In consequence of the excessive rains which have fallen everywhere in Texas during the spring, we were delayed by the rise of a creek, which ordinarily has but a few inches of water, though now too deep for our horses and waggons to cross, requiring us to make a long detour to the left, so as to ford the stream.

Fort Merrill was built in 1849, on the Nueces, four hundred yards from the right bank. The river, in its ordinary stage of water, is fordable, and about forty yards wide. The low bottom lands, on either side, are usually overflowed during the annual rise of the river in June, for miles in extent; and this was especially so during the last spring. The surrounding country is rolling, covered with rich grass, musquite, cactus, and clumps of oak; the banks of the river lined by rows of oak and cotton-wood, richly festooned by heavy mosses. There is fine hunting in the vicinity. Deer, wild turkey, geese, ducks, partridges, and rabbits abound. Catfish is the only fish found in the Nueces; trout exist in the small streams in the neighbourhood. The fort consists of officers' quarters, and barracks for the soldiers, of square logs, and plastered inside, a hospital capable of accommodating twelve patients, surgery store-room, &c., storehouses, offices, and camp women's huts—all roughly built, and unfinished. The garrison consisted of companies F and H, mounted

rifles—about eighty-five in all—four officers, one lady, and five laundresses. Some dozen or twenty families of settlers on the other bank of the river, and a small Mexican ranch, were our only neighbours. Our chief food was derived from the commissary and the field—beef, salt provisions, and game. Vegetables were very scarce, and the only fruit inferior melons. No gardens were planted at the post, and the high water had swept off and destroyed those of the farmers. Corn, and a few sweet potatoes, are the only articles raised; cattle being the chief source of wealth in this State, and especially in this neighbourhood. The large blue grape grows in great perfection in a wild state, creeping through and covering the trees on the river side. It makes a very fine wine and brandy. There are no other wild fruits. The temperature generally, during the three months I was on duty at the post, was very high, the thermometer averaging from 90° to 100° F. at 3 o'clock P. M., but tempered by a constant breeze; nights pleasant, enabling one to sleep delightfully, and awake refreshed. Rain has fallen in heavy showers for two or three hours almost every day, accompanied by thunder and lightning and cool breezes.

I herewith append the results of observations taken at the post from August, 1853, to October, 1854—a period of fourteen months:—

MONTH.	THERMOMETER DETACHED.				MONTHLY MEAN.			Rain.	Variation.
	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Mean.	Maximum.	Minimum.		
1853.									
August . . .	73.83	88.64	93.16	80.22	83.50	100°	71°	5.75	39°
September . .	70.76	62.35	89.46	77.26	80.11	99	62	5.86	37
October . . .	62.03	74.58	86.45	77.64	74.24	89	46	3.12	43
November . . .	56.16	66.53	78.10	64.26	67.13	87	40	1.40	47
December . . .	51.96	59.67	63.74	59.25	57.85	80	40	1.18	40
1854.									
January . . .	45.25	55.83	64.80	54.70	54.03	96	22	0.11	74
February . . .	50.92	61.82	66.78	59.50	58.85	88	31	1.99	57
March	63.70	81.54	87.29	77.51	75.48	93	40	0.05	53
April	62.96	78.86	88.13	76.23	75.55	97	52	1.16	45
May	71.01	83.30	90.26	78.30	80.67	97	66	7.16	31
June	78.76	83.06	93.93	83.53	86.35	101	74	4.70	27
July	74.74	90.48	96.74	80.03	85.80	101	71	5.46	30
August	74.35	88.22	94.99	80.00	84.66	102	66	3.13	36

REMARKS.

August, 1853. Mornings invariably damp and foggy, clearing to west about 7 A. M. Rain fell, 8th, 9th, 10th, 11th, 12th, 20th.

September. Rain on 3d, 4th, 8th, 9th, 12th, 20th, 27th, 29th. Fogs in the morning continue, and somewhat later in passing away to west.

October. Fogs continue in mornings. Rain on 11th, 12th, 13th, 16th, 17th, 21st, 23d, 26th, falling in heavy showers with intervals of $\frac{1}{2}$ or $\frac{3}{4}$ hour.

November. Rain on 18th, 19th, 20th. Heaviest fall on 20th, from 3 A. M. to 1 P. M.; quantity, 1.12.

December. Mornings very foggy and damp, clearing to west about 7 A. M. Rain on 6th, 7th, 12th, 13th, 20th, 22d, 23d.

January, 1854. Severe weather on 5th, with light sleet. On 7th, ice $\frac{3}{4}$ of an inch thick on water barrel. On 8th, at sunrise, very heavy white frost. Weather gradually milder until 20th. At 5 P. M. on the 20th, severe weather with cold sleet, followed by heavy mists.

February. On the 14th, snow made its first appearance this season, commencing at 3 P. M. of 14th, and ending at 10 A. M. of 15th, in sleet and very cold rain; quantity of snow, 1.11. Very heavy blow on 25th, at 4½ P. M., from N. N. W.; wind 9.

March. Rain on 2d, 9th, 29th. Fog at sunrise throughout the month. On 31st, at 7 P. M., very bright meteor shot through the eastern heavens, and, after travelling two-thirds of its course, seemed to branch like a rocket and disappear. Very heavy blow throughout the night; at 1 A. M., wind 7.

April. Comet visible on 1st of this month in western heavens, situated near the Pleiades, about 20° above the horizon; the nucleus much larger than a star of first magnitude; tail sweeping upwards, inclining towards N. W.; visible until April 4. On night of 6th, very heavy, wet weather; rain 1.15, with very vivid lightning.

May. This has been the wettest month on record at this post. Rain usually attended by thunder and lightning.

June. Rain fell more constant this month than in the month previous, but in lesser quantities. On the 2d, A. M., the Nueces River ebbed, after a rise of over twenty feet above its usual height, overflowing all the bottom lands, and doing much injury to the settlers.

July. This month has continued showery. No heavy fall of rain except on 26th; 1.60.

August. On 6th, thermometer 102° at 12½ P. M.; the highest this season. From 1st to 13th of the month, the weather continued to be oppressively warm. On 14th, a little rain, which tended to cool the atmosphere. Rain on 14th, 15th, 16th, 17th, 18th, 25th, 26th, 27th, 28th.

These observations, taken with care, may be considered the average temperature of year after year at this post. The past winter was rather colder than usual. Snow rarely falls; thus we find in the month of February (14th), snow fell to the depth of 1.11 inches, but soon melted. The coldest day was the 7th of January, when the thermometer stood at 22°, and ice formed three-fourths of an inch thick. Only once in February the mercury sunk below freezing point, during the prevalence of a severe norther. The warmest day was August 6, when the mercury stood at 102° at half-past 12 o'clock.

Rain falls every month in the year, but in greatest quantity in the month of May. Then the rivers and creeks become swollen and overflow their banks, but subside during the month of June. The Nueces rose *twenty-six* feet. Very little rain falls during the winter months. Variations in temperature are often very great and sudden, a cold norther springing up, and freezing one who, but a few moments before, may have been panting with heat. We find the monthly mean to be—

1853. August	83.50
September	80.11
October	74.24
November	67.13
December	57.85

1854. January	54.03
February	58.85
March	75.48
April	75.55
May	80.67
June	86.35
July	85.80

Hence, we have for the year from August, 1853, to August, 1854, a mean annual temperature of 73°.29.

Heavy fogs settle down during the night over the river and bottom lands, and are not dissipated until 9 o'clock.

Report of Cases of Fever and Bowel Complaint at Fort Merrill, Texas.

MONTH.	Febris continua.	Feb. int. quot.	Feb. int. tert.	Feb. int. quart.	Febris remittent.	Febris typhus.	Cholera.	Colica.	Diarrhea.	Dysent. acut.	Dysent. chron.	Mean strength.
1852.												
April	19	12	...	7	4	3	3	50	30	2	581
May	23	11	2	16	2	...	4	19	13	1	436
June	5	6	...	3	1	5	4	...	118
July
August	3	2	1	4	330
September	6	2	4	1	2	...	3	327
October	9	12	...	3	2	325
November	6	7	...	4	1	...	3	2	427
December	3	5	424
1853.												
January	3	3	132
February	3	5	1	1	137
March	3	2	2	1	...	124
July	2	1	1	93
August	21	4	...	2	...	1	...	1	1	...	73
September	8	4	...	6	1	1	...	3	73
October	1	8	4	51
November	6	5	62
December	1	6	69
1854.												
January	4	12	3	124
February	5	8	4	62
March	3	4	1	75
April	2	...	6	4	1	1	84
May	3	...	19	...	3	...	1	1	5	1	...	69
June	5	5	23	...	1	7	76
July	4	18	2	7	3	...	58
August	1	24	38	...	4	1	81
September	15	8	68

I have only given the report of fevers and diseases of the digestive system, they being most common, and forming the chief bulk of the sick report. Minor cases have been omitted. I here quote the remarks appended to the quarterly report, September 30, 1854, by assistant surgeon Head, who relieved me at the post on the 15th September.

"It will be perceived that, in July, with a mean strength of 79 (including therein scouting parties), there were 34 taken sick, or less than half the mean strength; while in August and September, the number of sick considerably exceeded the mean strength. The proportion of sick rapidly increased about the middle of August, and has not yet materially diminished. I have endeavoured to ascertain, as far as possible, the causes of this unusual insalubrity, and of its sudden increase. Early in June the river Nueces rose at this point to an unprecedented height (twenty-six feet). By the second week in June, the water had subsided, leaving exposed to the sun a recently overflowed level of considerable extent lying to the east of the post; the prevailing wind has been from the southeast. This rise and fall of the river was followed by an increase of the sick report, but much less than that which occurred in August. More rain, thunder, and lightning in July than June, and more in May than either. Scarcely any perceptible influence of these phenomena can be traced. About July 28, the post was stripped of many of its important medicines to supply General Smith's escort, taken chiefly from other posts. Among other supplies, all the quinia, except *two ounces*, was taken, and it became necessary to economize the expenditure. Until August 14, small doses of quinia and piperine (five grains each) were used; and afterwards the treatment of periodic fevers was almost wholly by table salt and Fowler's solution. It appears that the greatest increase of sickness coincided almost exactly with the discontinuance of the general use of quinia. During the thirty-five days from June 24 to August 28, there were—

Taken sick of malarial fever alone	29
Returned to duty	26

During the succeeding thirty-five days, from July 28 to September 1, there were—

Taken sick of malarial fever alone	70
Returned to duty	41

During the last two months, every officer (except one who had been but ten days at the post), every lady, every soldier, and every laundress, has been sick. There are not well men enough for the ordinary garrison duty; a single tour of guard is almost certain to send a man to the hospital."

The malarial poison appeared to be of unusual intensity, producing severer symptoms than ordinarily accompany intermittents; while they were prolonged in consequence of being deprived of quinia. Having but a small quantity, and an interval of one month occurring when I was obliged to await the arrival of a supply from New Orleans, I tried the effect of a solution of common salt, with success in less than half the cases.

I noted a marked increase in the number and intensity of the fever cases immediately after a rainy period. Military duty was exceedingly hard, and most of the men were broken down by heat and fatigue. If a number were sent on a scout, one-half or more returned sick; while of a camp of twenty men at Santa Gertrudes, only four remained well. Far from becoming acclimated, men grow less and less able to resist the excessive heats and fatigue of campaigning. A feeling of lassitude, and indisposition to mental or physical exertion, takes possession of all. A tendency to scorbutus showed itself

in a few, but was readily overcome by suitable remedies and diet. Vegetables were very scarce, but grapes and melons could be procured in abundance. Not a single death occurred at the post.

Ringgold Barracks, to which I was ordered from Fort Merrill, is situated immediately upon the left bank of the Rio Grande, or Bravo. This stream is about two hundred yards wide, shallow, and impeded by sandbars; navigable for twenty miles further up, as far as the village of Roma, and of an exceedingly tortuous course. The water, ordinarily, is good for drinking, but during and immediately after the spring freshets is thick, muddy, and offensive to the taste and smell. The surrounding country is miserably poor, covered with cactus and dwarf musquite, and a few ebony trees. Vegetation scanty, and gardens cultivated by the extremest care in a few sheltered and richer spots. The site of the post seems to have been selected from being opposite the small Mexican town of Camargo, four miles interiorly from the river, occupied by our forces during the operations on the Rio Grande in 1846; and immediately on the right, a few hundred yards distant, is the Rio Grande City, or Davis's Rancho, containing about six hundred souls—Mexicans, Americans, Germans, Italians, and, I might safely say, representatives from all nations. With few exceptions, the inhabitants are miserably poor. The men work in the fields, tend cattle and goats, and hunt wild horses. As in all Mexican towns, gambling is a vice indulged in by all classes. The females are generally comely, but not handsome; neglectful of their persons, and loose in morals, but far superior to the men. The foreign residents, Americans and others, are storekeepers, horse-traders, and gamblers. Some few years ago, Rio Grande City contained the worst population in Texas, but now has among its citizens several intelligent and respectable men. A large trade was done in smuggling across the river into Mexico by Americans and Mexicans, but since the revolution and complete discomfiture of the *filibusters*, all intercourse has been interdicted, and military law prevails. The foreign, and better class of native inhabitants, enjoy comparative comfort and cleanliness, but the majority live in miserable *jackales*, crowded together with dogs, pigs, and chickens, having a goatskin for a couch, sitting on the floor, and living, for the most part, on tortillas and chili. The complexion of the females is generally very dark; hair fine, and black; good teeth; rather stout, but well proportioned; polite, and agreeable in manners. The usual passion for dress and ornaments prevails, and they are skilful and tasteful in making articles of dress. The Mexicans are remarkably healthy, owing to their simple diet, and being much in the open air. The females attain the age of puberty at fourteen or fifteen years of age, and childbirth is the same in character as among other females in the same social condition. Children are very healthy and well formed, and run about naked until eight or nine years of age.

Ringgold Barracks is a small military post, built to accommodate two companies, but at this time was garrisoned by eight companies, and band. The

commanding officer's house occupies a knoll about eighty yards from the river bank, and the officers and soldiers' quarters on the right and left facing the parade, are framed and plastered, and comparatively good. Latitude $26^{\circ} 23' 17''$, longitude $99^{\circ} 2' 46''$. Height above the Gulf, 121.9 feet.

Abstract of Temperature of 1854.

DATE.	THERMETER DETACHED.				Mean.	Minimum temperature.	Maximum temperature.	Greatest variation.	Quantity of rain.	Prevailing winds.	REMARKS.
	Sun-rise.	9 A.M.	3 P.M.	9 P.M.							
1854.											
January	47.38	53.35	65.12	57.03	56.25	29° 8th	87° 20th	49° 1st	Ins.	S.E. & N.	Threenorthers during month.
February	54.53	61.78	71.92	61.28	63.23	31 4th	83 11th	28 4th	1.69	S. & N.E.	One norther.
March	66.64	74.16	83.22	72.38	75.00	40 11th	98 25th	35 11th	0.22	S.E.	Two northers during month.
April	65.36	79.53	87.80	73.83	76.61	49 2d	96 24th	38 16th	..	S.E. & N.W.	Comet visible from 1st until 8th.
May	72.48	82.06	91.42	81.06	81.91	63 1st	102 16th	31 6th	2.83	S.E.	Eclipse of sun 26th.
June	76.63	85.06	92.16	83.36	84.40	71 11th	100 9th	23 7th	10.98	S. & E.	
July	74.48	85.53	89.16	81.12	81.82	70 14th	97 26th	24 16th	4.06	S. & S.E.	Heavy storm on 28th.
August	74.51	85.83	90.48	81.22	82.64	70 29th	96 10th	24 29th	1.85	S.E. & S.	
September	73.86	81.93	89.13	80.33	81.50	64 23d	95 11th	25 23d	3.02	S.E.	
October	67.09	75.67	84.93	76.22	77.93	55 30th	89 3d	33 30th	0.92	S.E. & E.	
November	55.33	65.36	76.03	64.96	65.66	30 27th	86 24th	41 27th	2.10	N. & N.E.	
December	45.48	55.03	68.41	56.83	56.95	29 30th	85 3d	45 30th	0.68	S.E., E., N.E.	
Mean	64.48	73.79	82.48	72.46	73.66	50.08	92.83	33.00	29.05		

The above abstract shows a year of less excessive temperature than usual of both heat and cold, for, on reference to the record of observations of other years, I find the thermometer noted as high as 108° , though it did not reach higher in 1854 than 102° on the 16th May. The minimum was 29° , when a very thin sheet of ice was formed during the night. The northers were very mild, and less frequent than usual.

This is considered the hottest post in Texas. The winters are very mild, except when a norther blows. I have sat during the month of January, 1855, with windows and doors open until 9 o'clock P. M.; and on the first of March it was hot enough to dispense with flannels, and put on light linen clothing. The heat is constant for nine months of the year, and is excessively prostrating to the mental and physical energies.

The mean annual temperature is 73.66° , nearly the same as that of Fort Merrill.

Rain fell every month of the year, except April—in greatest quantity in June—and produced here the same unhappy effects as at Merrill, swelling the Rio Grande, and overflowing a vast extent of country on both sides as far as the mouth of the river, and causing unusual sickness.

Quarterly Report of the Sick and Wounded at Ringgold Barracks, Texas, for the Quarter ending December 31, 1854.

REMAIN- ING LAST REPORT.	MONTH.	FEVERS.				DISEASES OF THE ORGANS CONNECT- ED WITH THE DI- GESTIVE SYSTEM.				THE RESPIRA- TORY SYSTEM.				THE BRAIN AND NERVOUS SYSTEM.		THE URIN- ARY AND MUSCU- LAR STRUC- TURES.		THE FIBRUS AND MUNCU- LAR STRUC- TURES.		ABSCESSES AND ULCERS.		WOUNDS AND INJURIES.				ALL OTHER DISEASES.										Aggregate		Returned to duty.		Discharged service.		Dead.		Sick.		Convalescent.		Total.		REMAIN- ING.
		Febris intermittens quotidiana.	Febris intermittens tertiana.	Febris remittens.	Febris rem. congest.	Colica.	Diarrhoea.	Dysenteria acuta.	Obstipatio.	Asthma.	Bronchitis acuta.	Catarrhus.	Pleuro-pneumonia.	Pneumonia.	Cephalalgia.	Delirium tremens.	Mania potu.	Gonorrhoea.	Rheumatismus acutus.	Abscessus.	Amblyopia.	Amputatio.	Contusio.	Fractura, tib. and fib.	Subluxatio.	Vulnus laceratum.	Vulnus sclopeticum.	Debilitas.	Eclipsia.	Hæmorrhoids.	Morbi cutis.	Ostitis.	Scorbutus.	Morbi vari.	Total.	129	190	131	4	11	456	509								
13 15 28	Grand total	137	140	20	4	1	45	21	8	1	4	8	1	1	3	1	6	1	9	10	3	1	8	1	3	2	1	11	4	2	3	1	15	1	1	126	509	456	1	11	20	21	41							
	October	59	54	10	13	2	1	1	..	2	3	1	7	..	3	..	1	..	1	1	1	2	3	1	2	1	170	129	..	4							
	November	56	58	10	4	..	14	0	4	..	1	4	1	6	..	2	1	2	1	..	1	..	1	4	2	0	..	185	190	1	4							
	December	23	37	1	18	10	3	3	2	..	1	..	1	3	2	..	3	..	2	1	0	1	7	..	126	131	..	3							

MEAN STRENGTH.

		Officers.		Enlisted men.		Total.	
October	201	208
November	203	213
December	324	337
Aggregate		728	759
Average		242	253

Abstract of Sickness among Women and Children, Ringgold Barracks, Texas.

Quarter ending December 31, 1854.	Feb. int. quot.	Feb. int. tert.	Abortio.	Cephalalgia.	Catarhus.	Diarrhoea.	Dysenteria.	Disease of womb.	Total.	Total percent.
Ladies	2	2	1	1	6	6
Ladies' children	2	2	3	...	7	7
Camp women	5	6	1	1	1	1	15	17
Camp children	12	2	4	3	...	21	22
Total	7	20	2	1	4	7	7	1	49	52

Discharges and Deaths.

No.	NAMES.		Rank.	Regiment.	Com-pany.	Disease.	Discharged from service.	Died.
	Surname.	Christian name.						
1	O'Brien	Thomas	Private	5th infantry	B	Feb. remittens		Oct. 5, 1854.
2	Cromley	James	"	"	G	Pleuro-pneumonia		" 19, " 1
3	Krider	Geo. W.	"	4th artillery	D	Feb. congestive		" 19, "
4	Sugrue	Timothy	"	5th infantry	G	"		" 25, "
5	Monroe	Wm. H.	Corporal	"	K	Feb. remittens		Nov. 1, "
6	Wheland	Patrick	Private	4th artillery	D	Diarrhoea		" 3, " 2
7	Cain	Patrick	"	"	D	Feb. rem. congest.		" 6, "
8	Jones	William	"	Mounted rifles	H	Scorbutus		" 23, "
9	Karwosky	Gothief	Corporal	4th artillery	D	Pleuro-pneumonia		Dec. 2, "
10	Saddler	Richard	Private	5th infantry	Baud	General debility		" 14, " 3
11	Neuz	John	"	"	E	Chronic diarrhoea		" 30, "
12	Hackney	Joseph	"	4th artillery	D	Rheumatism, chron.	Nov. 5, 1854 ⁴	

On my arrival at this post (September 30), I found the same disease prevailing which had existed at Fort Merrill—a malarial fever of unusual severity. The disease, soon after my arrival, increased in intensity, and attacked a large majority of the garrison, and nearly every soul in the adjoining village, appearing in the different degrees from ordinary fever and ague to a low congestive form of remittent, closely approaching yellow fever. At Camargo, on the Mexican side, four or five miles distant, nearly one-third of the population died; more than thirty died at Rio Grande City. Along the banks of the Nucces and Rio Grande few escaped between Laredo and Brownsville, while at Monterey, Saltillo, and Mier, in the interior, the disease was comparatively mild; at Corpus Christi, on the other hand, it was of greater severity, and assumed the form of *black vomit*, or true yellow fever. Cholera prevailed in middle and northern Texas. The summer and autumn have been unusually hot and dry, and the winter exceedingly mild, frost having formed but twice or three times early in the day. During the month of June, an unusual quantity of rain fell. By reference to the above report, it will be

¹ Supervening on general debility and disease of the bowels.

² Broken down by intemperance and constant sickness.

³ Broken down by long-continued intemperance.

⁴ On surgeon's certificate of ordinary disability.

seen that no less than *three hundred and ten* cases of malarial fever occurred among the troops, which, with diarrhœa and dysentery, *sixty-six* cases, go to form the great bulk of the sickness. Out of fifty-two women and children, there were twenty-seven cases of fever and *fourteen* of bowel complaint. Out of a population of some five hundred in Rio Grande City, I attended about *two hundred*, of whom six died. Scarcely a man, woman, or child can be found who has not had some form of the disease. The symptoms, in all cases, were marked by great severity—the headache, pain in the limbs and back were excruciating; there was complete anorexia and insomnia, with prostration of the mental faculties and physical strength; a pale, bloodless hue of the face, often tinged with a dull yellow, attended even the milder forms, while, in the severer types, all these were intensified—the head hot; face either perfectly exsanguine or of a mahogany colour; conjunctiva injected, and of a deep yellow; tongue covered with a thick yellow fur; lips dry; sunken expression of the countenance; brain sometimes dull and oppressed, so that the patient could not utter his wants—in others clear and active—and again, in a state of raging delirium, with constant crying out in loud tones and shrieks. In females, a severe paroxysm of *hysteria*, and in infants *convulsions*, frequently ushered in the disease. The bowels rather disposed to be inactive, but towards the close of the epidemic diarrhœa prevailed, the dejections being thin and yellowish. Vomiting of thin, greenish-yellow matter occurred in nearly all cases, and in some was extremely obstinate. In fatal cases, especially of the congestive fever, the surface is pale, cold, and clammy; skin rugose, as if soaked in water; face pale, of a muddy or dull yellow hue; eye dull; conjunctiva yellow; pulse very rapid, and so feeble as scarcely to be felt; voice thick and inarticulate, and mind wandering. Patients who were apparently doing well at my ordinary visit at 8 A. M., in two or three hours after have been found in a state of collapse, from which the most active remedies could not revive them. The disease readily yielded to remedial agents, but the convalescence was tedious; complete lassitude and feebleness follow, irritability of temper, capricious appetite, irregular sleep, and indisposition to mental exertion. The complexion retains the dull yellow, and the lips their bluish exsanguine hue. Relapses are very frequent, recurring every seven or fourteen days, and, in some cases, it is impossible to break up the disease; fever and chill attacking the patient, without regularity, every two or three days, especially after the slightest fatigue.

I regret that my own illness prevented the post-mortem examination of all but two out of the five fatal cases in garrison. In both of these, however, I found the congested venous system and the bronzed liver; enlarged and softened condition of the spleen, first noticed by Dr. Stewardson, and confirmed by a large number of examinations at which I assisted while attached to the New York Hospital. This condition of the liver appears to result from the engorgement of the biliary and venous systems, while the contact of the intestines allows the gases secreted in them to come in contact with the

exterior surface of the liver, and give it a dark bluish tinge. Dr. Alonzo Clark thinks this is a peculiar deposit in the shape of small scales, and is now engaged in its examination by the microscope. In neither case were the stomach or glands of the intestines the seat of lesion.

In the treatment of the disease, I placed my chief reliance on quinia, aided by wine, brandy, and other stimulants. I never had occasion to take blood, except locally, to relieve the pain and sense of fulness about the head. Cathartics were only used to relieve constipation, and these were of the mildest character—a simple pill, castor oil, or laxative enemata. Not as much as a drachm of calomel was used in all the cases. When the skin was hot and dry, frequent sponging with vinegar and water; and, in cases of fulness and heat of head, a cloth wet with the same, or evaporating lotion, were used, and proved agreeable to the patient. Full doses of morphine were given to induce sleep, often without effect, this insomnia being one of the most constant and exhausting symptoms in many cases.

Quinia was used in full doses and without any regard to the fever; in the remittent form, according to severity, from *five* grains every four hours to *ten* grains every two hours. In intermittents, I gave it every way, and found all equally efficacious in divided doses between the paroxysms: in a *twenty-five* or *thirty* grain dose an hour before the paroxysm, or a single *thirty* grain dose as the sweating stage is subsiding, as recommended by an English army surgeon. I am inclined to prefer the last mode, for several reasons—it saves the medicine; it is least troublesome both to the patient and apothecary; the time is more marked; and, it is successful. A single dose, in many cases, is sufficient to check the disease. Brandy, wine, carbonate ammoniæ, beef tea, &c. &c., are necessary adjuvants in the different stages of fever.

At a period when I was almost without quinia, I resorted to other remedies; arsenic in pill, as recommended by M. Andral; Fowler's solution; nitric acid, as recommended by Dr. Bailey, of Indiana, and which was of decided benefit, and a cheap article for the poor. Both at this post and Fort Merrill, during a period when they had been stripped of their supplies, I employed a solution of salt in about fifty cases, and with success in about half the number; but I can regard it only as a *dernier resort*. There is great repugnance to its use on the part of patients; it produces nausea; is very uncertain; requires to be used for a week or more, and, in many cases, is rejected as soon as taken into the stomach. Hereafter, I propose giving a more detailed account of the remedy.

The most extraordinary feature of the report is the complete absence of syphilitic disease; the single case of gonorrhœa was contracted in the vicinity of Laredo. Not a case exists in town, nor among the troops; a fact without a precedent! That a Mexican town, in which there are about two hundred women, and mostly of Mexican morals, should exist without syphilis among them, is a wonder.

From the entire absence of vegetable food, *scorbutus* manifested itself in

many cases, although only fifteen are reported, of whom one died. The remedy made use of was the bicarbonate potassæ, in twenty grain doses, three times a day; opium grs. ij every four hours, and a wash of tinct. cinchonæ et myrrhæ. Two cases were treated with the juice of the maguey with benefit, but in others I was obliged to discontinue its use on account of its cathartic action on the bowels. Vegetables, oranges, and limes (whenever they could be procured), were given freely.

Four surgical operations have been performed, under the influence of chloroform, without unpleasant effects, and with success, viz: amputation of the arm near the shoulder-joint; for strabismus; amputation of the fore-finger for deformity consequent upon gunshot wound; amputation, or disarticulation of the knee-joint, a history of which will be reported elsewhere.

NEW YORK, June 1, 1854.

ART. VI.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. Army.

1849. WE now come to an epidemic which has as many heads as hydra.

Hume on the Causes of Yellow Fever in 1849.

"Produced by a vaporious poison, engendered in the soil, out of the decomposition of the animal and vegetable matter, aided by heat and moisture, and precipitated on the subject."

"To effect the decomposition of this organic matter, a certain elevation of temperature is necessary, say 74°, which the earth of our city acquires in August, while it increases to 75° in September."

"A certain amount of moisture is also necessary, which may be obtained from rain, wells, ponds, or capillary attraction of adjoining and subjacent water."

"The solar heat facilitates decomposition and diffusion in the atmosphere; while the nocturnal diminution of temperature produces condensation, and consequent concentration on the surface of the earth. In fine, yellow fever is engendered in Charleston, by the production, evaporation, and condensation of morbid vapor, proceeding from

Hume on the Origin of Yellow Fever in 1849.

"On the 18th July, 1849, the barque Numa arrived from Havana, and on the 6th August one of her crew (Sauraboni) was sent to the Marine Hospital, where he died that night of the black vomit. She lay at the Union wharves, foot of Pinckney Street. The Isabel and Merchant, both from Havana, arrived on the 25th July and 25th August, and lay also at the Union wharves. The engineer of the Isabel was attacked on the 7th, proving infectious on board these two ships at least. On the 20th August, the British barque Queen Victoria hauled into Patton's wharf, next above Union, from Hull, England, sound and pure, and the Huron, from a British port, lay in the dock below: thus the pure and the impure vessels were all in the vicinity of each other. On the 20th or 21st of August, the captain of the Huron was attacked with fever, but recovered. On the 28th, captain Laycock, of the Queen Victoria, was attacked with yellow fe-

organic matter in a state of decomposition, the result of a dense population, and set in action by the agency of heat and moisture."¹

ver, and died of black vomit on the 1st of September. Five of his crew were also attacked, but recovered."²

Arrivals from Foreign Ports.

Numa,	from Havana	July 18th.
Pessito,	"	"	.	.	.	July 18th.
Adela,	"	"	.	.	.	July 19th.
Isabel,	"	"	.	.	.	July 25th.
Merchant,	"	"	.	.	.	August 25th.
Huron,	"	Liverpool	.	.	.	June 18th.
Queen Victoria,	"	Hull	.	.	.	August 18th.

Dr. Hume did not give us a long list of arrivals, and he totally forgot to mention the arrival of the Huron on the 18th of June. Every one of these vessels arrived in healthy condition. During the months of July and August there were several arrivals from Havana, but we hear of no fever on any of the vessels.

Fever Cases.—Dr. Hume.

Numa	.	.	August 6.	Isabel	.	August 7.
Huron	.	.	August 20 or 21.	Queen Victoria	.	August 28.

The last two cases are not of much consequence, for yellow fever was up town when those on the Huron and Queen Victoria were attacked.

The first case in the city, then, according to Dr. Hume, was on the Numa. We will take six days for an average passage from Havana to Charleston, which merchants in the trade state to be a good run, and we find that this vessel was twenty-five days from the first-named port, and nineteen days at the Union-wharves, before a case of fever occurred. This vessel must have remained at Havana a sufficient time to become impregnated with contagion before sailing, in order to introduce yellow fever into Charleston—she did this, made the run, and lay at Union wharves nineteen days, before a single soul suffered from the contagion. It may be said that the patient died on the day of admittance, showing that he was sick before the day of his entrance into the Marine Hospital, but Dr. Hume gives no information on the subject. We will endeavour to be liberal, however, and will suppose that the patient had been sick three or four days, which leaves fifteen or sixteen days for the vessel at the wharves, in a most filthy part of the city, before there was a single case on board. It will be recollected that the strong leaders of the contagionists, Chisholm and Pym, state the period of incubation for the Bulam fever to be from two to four days; and Dr. Hume's contagious fever of 1849 must have been the Bulam fever, imported from Havana—because all agree (Chisholm, Pym, Dr. Hume, &c.) that the indigenous yellow fever of the West Indies is not contagious. Is it probable that so much time could be spent at Havana, on the passage, and in the filthy district of Union wharves,

¹ Charleston Med. Journ. for January, 1850; also same journal for January, 1853.

² Report to the City Councils, December 20, 1853; also Charleston Med. Journal for March, 1854.

without a case of fever until the 6th of August, and then only one case, if the disease were Bulam fever, or contagious? for Dr. Hume tells us of but one case, although he is careful to inform us that there were five cases on board the *Queen Victoria*, besides the captain. We hear no more of the *Numa*; she has one case of fever, and one only. We hear of no fever on board the other vessels from Havana, from the beginning to the end of the chapter, except the *Isabel*. The *Isabel* was at the wharf thirteen days before there was a case—ample time, in that section of the city, to produce yellow fever.

The *Huron* was at a different wharf, near the horrible Market Street docks, from the 18th June, over two months, exposed to most of the causes of malignant fevers which can be conceived of. The *Queen Victoria* lay at Walton's wharf, a considerable distance above the Union wharves, but in the same filthy part of the city, ten days before the fever occurred, a period of time amply sufficient to produce malignant fever. We do not suppose ourselves, more than physicians in general, particularly afraid of taking fever, but we would not have slept a night, from the 1st of July, on board a vessel at any wharf in this district, neither in East Bay Street, Hasel Street, Hard Alley, foot of Pinckney Street, Mott's Lane, Guignard Street, Market Street, nor Linguard Street, for any consideration. And now let us look at Union wharves.

They are situated in one of the worst and most filthy parts of Charleston. On the north are marshes, the filthy marsh lot at the foot of Society Street, and Bennett's mills, with all their marshes, filth, and rubbish; on the east, Cooper River and marshes, including the docks; on the south, Market Street and its terrible docks and wharves, and Linguard Street; and Meeting Street on the west. Within this district are included Anson Street, never in good police; Mott's Lane, which rivals Clifford and Hard Alleys in uncleanness; a dirty part of East Bay Street; Hasel Street, which this year was in an abominably offensive condition; the foot of Pinckney Street, always dirty, and this year in a horrible state; Hard Alley, leading from East Bay Street down to these very wharves, which needs only to be seen to be appreciated; Guignard Street, always filthy, and this year especially so; and that vile locality, Market Street, with its filthy docks and wharves. For a stranger to have slept in any of these streets, during the summer of 1849, would have been at the peril of his life.

This was one of the years in which extensive and deep excavations were carried on in midsummer, for various purposes, for laying gas pipes, opening and repairing drains, &c., and dearly did the city pay for these unwise measures. Hasel Street was completely upturned, like many other streets. Dr. Simons says:—

"In 1849, an extensive drain was opened in Hasel Street, excavating the most filthy and offensive materials, and likewise an extensive drain in Market Street from Church Street to the wharf. Yellow fever occurred earliest in these localities, and was more fatal. Again, this earth, so filthy and offensive, was transferred to King Street, from Horlbeck's Alley to Hasel Street, and in this

particular spot, there was sickness and mortality among a class of persons usually exempt."—*Chas. Med. Journ.*, May 1853.

Dr. Simons, in reply to Dr. Hume's article, says :—

"But he has omitted to state, that an extensive excavation for a drain, of the foulest materials, was opened on East Bay, from Hasel Street to Patton's wharf, that the same offensive materials were thrown up opposite to Union wharf, south, to form the foundations of the new custom-house—two sources of pestilential air, under the influence of which the contractor for laying the foundation of the custom-house was attacked with yellow fever. He has also omitted to state that the same offensive stuff was deposited in King Street, from Horlbeck's Alley to Hasel Street, and that in this portion of King Street there were many respectable persons who sickened and died : while there were none of that class affected in any other part of the street. He forgets, too, that the persons employed in these excavations were Irishmen, who are peculiarly susceptible to the disease, and were, in fact, its greatest victims. The doctor does not recollect, further, that so satisfied were the city authorities that this was the active cause of the disease, that all works requiring the excavation of earth in summer, such as laying gas pipes or opening and cleansing drains, were prohibited in 1853, and the period of elections changed—another exciting cause—during which year the yellow fever did not prevail."

Every street in the vicinity or near Union wharves was in a shocking state of sanitary police, and in many of them excavations were going on, as in Pinckney, Guignard, Linguard, and other streets. All of these streets were very unhealthy; not only was Market Street sickly, but the effluvia and malaria were carried by the prevailing wind to a neighbouring street (Hayne), producing, or assisting to produce, extensive disease. Linguard Street was ravaged by the fever, and the physicians who had a large amount of practice in this street during the epidemic, regard the extensive excavations as being the main cause of its malignity.

We will follow Dr. Hume's report of cases, from the shipping to the streets.

"If we proceed up the streets leading to these wharves, we find a case, on the 3d of September, at the corner of East Bay and Hasel Street (Betts) ; another on the 7th, east side of East Bay, one door above Hasel Street (Carroll) ; and on the 10th one at Bennett's mill, foot of Wentworth Street, two wharves above Patton's (Drew) ; one corner of Anson and Market Street on 9th (Behlin) ; and one in Linguard Street, on 10th (Collins)."

Mr. Betts, of the firm of Stevens & Betts, corner of East Bay and Hasel Streets, was taken with yellow fever on the 3d, and died on the 7th of September. He had been to the north, and had just returned to Charleston from New York—had been here about one week when he was attacked with the fever. Had not been to the shipping, but simply passed between the store and his residence, in Hasel Street. This account may be relied on. It is known that an attempt was made to suppress this case, by denying that it was yellow fever.

Mr. Betts always slept at his own house, which is at a considerable distance from the shipping, and he doubtless fell a victim to those sinks of corruption in his own street, the excavations. Every one of Dr. Hume's cases was in one of the worst districts of the city, just where we would expect malignant fever. The one at Bennett's Mill, September 10th, a considerable

distance above Patton's wharf, occurred the day after the one at the corner of Anson and Market Streets, and on the same day with the one in Linguard Street. Sahlman, who "went to the disease, brought it home, and transferred it to his neighbours," was taken on the 26th of August. He "had been into one of the vessels (which?) at Union wharves, to purchase cigars on the day before his attack." Quick work this, and the period of incubation is rather latitudinarian, comparing this case with that of Hewes in 1843, twenty-four hours, and two months, without considering his residence, at the corner of East Bay and Elliott Streets, in one of the most filthy parts of the city. Wm. Martin lived in Stoll's Alley, and kept a bakery in Church Street (an unhealthy street) between Broad and Elliott, was taken on the 2d September. There is not a particle of proof that Martin visited Union wharf, Sahlman, or any person sick with yellow fever. The fact of his living in Stoll's Alley is sufficient.

Dr. Robertson, who keeps a register of cases, dates, &c., has kindly given me his first cases of yellow fever in this year. First case, August 21, same date as on the Huron, in Hayne Street, a clerk who slept up stairs over the store. Second case, September 6th, three days after Betts, and one day before Carroll, corner of Market and Meeting Streets. Third case, September 7th, same day as Carroll, Hayne Street, near case one. Fourth case, September 8th, Hayne Street. Fifth case, September 9th, same day as Behlin, at the corner of Anson and Market Streets, Hayne Street, same store as case three. Sixth case, September 10th, same day as the case at Bennett's Mill and Collins, in Linguard Street, same store in Hayne Street as case three. Seventh case, same day, September 10th, in Hayne Street, same locality as case one. First fatal case, black vomit. Eighth case, September 11th, lower end of Hasel Street. Ninth case, 11th September, same location. Tenth and eleventh cases, September 12th, both in the same house in Market Street. Twelfth case, September 15th, Meeting Street nearly opposite Hayne. Thirteenth case, September 20th, same building as case one, bad case, recovered. Fourteenth case, September 22d, Meeting Street opposite Hayne. Fifteenth case, September 26th, State Street. Sixteenth case, September 26th, Hayne Street. Seventeenth case, September 26th, Hayne Street. Eighteenth case, September 27th, corner Market and Meeting Streets. Nineteenth case, September 27th, same as case eighteen. Twentieth case, September 29th, Beaufair Street. Disease general.

Dr. Hayne had a case at the Almshouse, August 25; and Dr. Hume states that Holman came from Savannah on the 24th, entered the house 25th, but we are not informed if Dr. Hayne's case was Holman. Dr. Hume supposes that Holman may have come from the West Indies, but does not produce a particle of proof, nor is it of the least consequence, yellow fever being already in the city at different points. Second fatal case at the Almshouse, attacked September 7th, same day as Carroll and Dr. Robertson's third case. Third fatal case, taken September 14th; localities not given. Fourth fatal case, labourer for Charleston Gas Company, taken September 16th. Fifth

fatal case, taken September 14th, Elliott Street. Sixth fatal case, taken September 20th, Elliott Street. Seventh fatal case, taken September 22d, City Guard.

On comparing Dr. Hume's cases with those of Dr. Robertson and the fatal cases given by Dr. Hayne, it is seen that the fever had several points of origin, and did not gradually extend from the Numa and Isabel, as might be expected of a contagious disease. Dr. Robertson states his conviction, that the patients in Hayne Street contracted the fever in a great degree from Market Street, in this way: the great value of property renders it necessary for one or more clerks or assistants to lodge in the stores, and in order to render their rooms more comfortable at night, they were in the habit of admitting the breeze freely through the open windows. The consequence was, that the prevailing wind, blowing directly from Market Street, the dreadful sanitary condition of which has been described, brought humidity and malaria in abundance from the very fountain of disease, and at the most dangerous of all hours.

Taking everything into consideration, particularly the circumstances of the Numa, the length of time which had elapsed since her departure from Havana, the time she lay at Union wharves, in the most filthy part of the city, &c.; we conclude that the yellow fever of this year was not imported, but, on the contrary, was of domestic origin; and that, so far from the Numa and Isabel giving the disease to the inhabitants of Charleston, they received it from the filthy wharves, docks, lanes, and alleys of the town.

1852. As yet, we have seen no causes of this epidemic assigned except such as make it of local origin. As yet, we hear of no imported contagion from Africa, the West Indies, New Orleans, or any part of the globe, though in process of time some vessel may be sought out which introduced the contagion. But no such vessel has yet made her appearance.

The causes which produced the epidemic fever, both in Charleston and on Sullivan's Island, were local. It is not necessary that a consideration of the causes of this epidemic should again be entered into, and we will only cite one or two passages from Dr. Simons, *Chas. Med. Journ.*, May, 1853.

"In 1852, the same thing occurred (opening drains and other works, and transferring the earth to different portions of the city); and, added to this, there were a great number of emigrants from Ireland, who arrived here during the epidemic, and soon became victims of the disease. Destitute of means, they were compelled to do laborious work, such as excavating the earth, paving the streets, &c., and having miserable accommodations. At the new custom house, a great number of Irishmen were employed in excavating the earth and piling, of whom a great many were taken sick and died; and the sale and distribution of the earth through the different portions of the city had a baneful effect. I may mention that the contractor for the piling was also taken sick, and died from exposure in inhaling the poisonous and malignant air of the locality for the new custom house." (The new custom house is next south of Market Street dock.)

"I have the most sanguine hope that the experience of 1849 and 1852 will prevent the public authorities from excavating the earth, or cleansing drains,

or disturbing the earth in any manner, or changing it from one place to another, or paving in the summer months. It was very conclusive to my mind that there were active and efficient causes of the epidemics connected with the individuals who performed those duties."

Commencement of the epidemic. Dr. Simons says, same number of the Journal:—

"The first case occurred on the 16th August, on board of a vessel lying at Vendue Range wharf (foot of Queen St.), which broke out a short time after her arrival from Boston. It was the wife of the captain. The vessel had on board some damaged cotton, taken from a ship which had been on fire and scuttled; and it was supposed from this, with other circumstances, that the case was sporadic. On the 20th, 30th, and 31st, a case of death on each of those days occurred, which the attending physician, uncertain of the character of the disease, reported as bilious fever."

The case at Vendue Range wharf was considered the first case of yellow fever in the city, but it appears that certain other sporadic cases had previously appeared, to which we will refer.

If the barque Clara C. Bell, of Boston, at Vendue Range wharf, on board of which Capt. Cales's lady died, or was taken sick, on the 16th of August, had come from Havana, what a subject for the contagionists! If this vessel had arrived from the West Indies, or New Orleans, we doubt if even Dr. Hume himself would have forgotten to describe the epidemic; and instead of the figures, 1852, merely, we would have had a full account of the contagious fever, the last and most important of all, the Clara Bell sailing bravely into port as the flag ship, the Lord Glenelg, Burmah, and Numa, being mere tenders.

Dr. Fitch had a case as early as the 20th of July, Lewith, N. W. corner of State and Queen Streets, which terminated fatally on the fourth day, "and but a short period elapsed after death before the whole surface became intensely yellow. In comparing his symptoms afterwards with those cases of yellow fever then occurring, I could not but be struck with their malignancy."

"The next case occurred August 15th, in the person of Mr. Tom, blacksmith, who had been overheating himself. Residence in State Street, No. 42."

"The next case was in East Bay Street, August 31st." An Irishman died on the 14th of August, in Church Street, between Cumberland and Market Streets, but the case was not reported as yellow fever. It was a well marked case.

Capt. Cales's lady died on the 16th August, as already stated.

Dr. Lebby informs me that Gilcuddy, blacksmith, was taken on the 19th August: Meeting Street, opposite Reynolds & Co.

"From this period," says Dr. Lebby, "cases continued to occur in a certain section of the city bounded by Market Street on the north, the wharves on the east, Queen Street on the south, and Meeting Street on the west, with a few cases in King Street. From 20th August to 2d September, I treated fifteen cases, and lost three. The fever then assumed an epidemic character, and spread generally."

Dr. Robertson has kindly given me his first cases for this year. First case, August 30th, Hayne Street. Second case, August 31st, corner Church

and Queen. Third case, Dr. R.'s son, place of business corner of Church and Queen. Fourth case, September 3d, East Bay. Fifth case, September 3d, corner of Market and Meeting. Sixth case, September 3d, Hayne Street. Seventh case, September 4th, Meeting Street, traced to E. Bay. Eighth case, September 4th, Anson Street. Ninth case, September 5th, corner Hasel and Meeting. Tenth case, September 6th, Meeting, nearly opposite Theatre. Eleventh case, September 6th, Charleston Hotel. Twelfth case, September 6th, Market Street. Thirteenth case, September 7th, Hayne Street. Fourteenth case, September 8th, Charleston Hotel. Fifteenth case, September 8th, Hayne Street.

This will serve to show how rapidly the disease became general. The fever also commenced early in the little dirty street called Philadelphia, better known as "Cow Alley," and was very malignant.

Dr. Cain would kindly give me the first cases in the Marine Hospital; but they do not assist in pointing out localities. The Dr. informs me that most, if not all, of this class of patients were unwilling, or unable, to give their previous residence and history; many of them were brought to the hospital in the last stage of the disease when moribund; often insensible and speechless; so that no information could be obtained. Dr. Cain gives a curious and unexpected fact, that he had, if my recollection serves, not more than 38 or 40 cases of yellow fever in the Marine Hospital during the epidemic; certainly there was much less of this disease among mariners than could have been anticipated.

We will once more refer to the Clara Bell, on board of which Capt. Cales's lady died. This vessel arrived from Boston, "sound and pure," on the 29th July; she therefore lay at her wharf not more than eighteen days, one day less than the Numa, in 1849, and in a better district, certainly not in a worse one. Allow three days' sickness, for the case was a violent one, and the death on the 16th August, and we find that the fever originated fifteen days after the arrival of the vessel from a healthy port. Might not yellow fever originate on board the Numa, in 1849, in a more filthy part of the city, and on the Isabel (at the same wharves with the Numa), in thirteen days, as well as on board the Clara Bell in fifteen or eighteen days? Yellow fever originated on board the Ellen Goldsborough, at Accommodation Wharf, in 1853, in five days. We care not if there were 50 cases of yellow fever in Charleston before the death of the lady on the Clara Bell: this case was completely isolated.

Dr. Lebbv has kindly given several cases, relative to contagion, the origin of this epidemic, etc. One of them must be noticed in this place, as serving to reveal the *fons et origo mali* at one point.

"A student of medicine, who had not resided in Charleston from childhood, and graduated in the S. C. College, Columbia, the winter previous, visited the Roper Hospital almost daily, and accompanied me in my private practice to the chambers of yellow fever patients. He contracted the disease after repeated visits to houses in Queen Street, between Church and State Streets, one of the points of origin, and where the disease was perhaps most malignant. In this district of disease, in two houses not fifty feet apart, I had 14 cases; and in the

house opposite, occupied by fifty or sixty persons, the disease originated. One of the rooms in this building was occupied by a family of seven, father, mother, brothers, and sisters, and five fell victims to the fever. When a portion of them were removed from the dwelling, there was one dead and another dying; and the floor was covered with black vomit. The building is a large four story edifice, with a deep cellar, which was covered with water; and the cellar was filled with rice chaff, by order of our efficient city register, as soon as this state of things was discovered."

"The cases are selected as illustrative of the non-contagiousness of yellow fever, and also as strong instances of the origin of the disease in our very midst, when such causes existed, as did this season, to generate this form of fever.

"I have seen, during my professional career of almost a quarter of a century, a large amount of fever, and I must confess that I have not seen a solitary instance of yellow fever that has induced me to regard it as contagious. On the contrary, I am, from personal observation and experience, a decided believer in the non-contagiousness of the disease."

Dr. Lebbv has shown the origin of the disease at one point. Here we find crowding together of persons and families, bad ventilation, filth, humidity, and almost everything which could generate malaria and yellow fever. Could the most stringent quarantine have prevented the suffering and disease in this horrible locality of Queen Street?

It has already been fully shown that the yellow fever on Sullivan's Island, this year, was not brought from Charleston, but that it unquestionably originated on the island.

Yellow fever did not entirely cease in Charleston until the severe frost, on the 15th of November, although no reports were made by the Board of Health after the 1st of the month, and some of the city newspapers had an announcement that the disease was ended. Mr. William N. Lucas, a native, returned from the north, took the fever, and died on the 8th of November; and there were other deaths from yellow fever between the 1st and 15th of the month. In truth, Charleston was not a fit place for strangers until the 15th. November 16th, the following communication was made to the officer in command of Fort Moultrie:—

"Since yesterday morning, it is, in my opinion, perfectly safe for officers and men to remain in the city of Charleston, both day and night. Previous to yesterday morning it was not, in my opinion, safe for officers and men to remain long in Charleston, particularly at night."

The year was unhealthy. Immediately after the fever subsided, I find the following entry: "December 5th. Colds and coughs prevail a good deal on Sullivan's Island, and there is dysentery." "December 12th. Colds and coughs are prevalent, and, taking the whole country together, there is an epidemic influenza."

"December 26th. Since the 21st it has been rainy and foggy, cold and warm. The last three days have been warm, the weather unpleasant, and the dampness excessive. There is some cholera in the city, in consequence of which, I officially recommended, December 24th, that none of the men should be permitted to spend the night there.

"December 31st. The weather has generally been warm and damp, with frequent fogs. There has been considerable cholera in and around town, so much that some alarm is exhibited, and country people have left."

During this time, or from the middle of December to the middle of January, 1853, diarrhoea was prevalent on Sullivan's Island, especially among the blacks, as also cholericine, and one case of malignant cholera; all showing that there existed a disposition to the same type of disease as prevailed in Charleston.

ART. VII.—*On the Anomalies of Arteries.* By C. E. ISAACS, M. D., Demonstrator of Anatomy, in the University of New York.

It is difficult, for one extensively engaged in the details of practical anatomy, to keep a record of every anomaly which may fall under his observation, and a few will sometimes escape notice. I am, therefore, unable always to state how often I have met with a certain deviation from the normal standard, or the frequency of its occurrence, in a given number of cases. Where this has been ascertained it will be mentioned, and where it has not been practicable to do so, it has, however, appeared to me that some of those facts, which have been occasionally or frequently observed, are of sufficient surgical interest and importance not to pass unrecorded.

I shall first allude to some *anomalies* of the *arteria innominata*. I have seen three cases in which this artery gave off three large branches, viz: 1. The right subclavian. 2. The right carotid. 3. The left carotid. In one of these subjects, the innominata gave off also a middle thyroid artery to the thyroid gland. In two instances, no *arteria innominata* existed; but the arch of the aorta gave off: 1. The right subclavian. 2. The right carotid. 3. The left carotid. 4. The left subclavian. I have seen seven cases in which the middle thyroid artery arose from the innominata, and was distributed to the thyroid gland. In two subjects it was remarkably large, and its division in tracheotomy would have occasioned a very profuse hemorrhage.

Of the third portion of the *subclavian artery*, or that which extends from the outer edge of the *scalenus anticus* to the lower margin of the first rib, I have found this portion of the artery to vary very much in the height to which it rises in the neck; in some subjects passing high above the clavicle, in others sunk behind that bone, and, therefore, much more difficult of access. In one instance, it was so covered by a plexus of large veins that it would have been impossible to have placed a ligature around the artery without previously tying some of these veins, and dividing between the ligatures; a condition somewhat similar, but not to the same extent, is not very uncommon.

The tubercle of the first rib, upon which many surgeons depend as a *sure guide* to the subclavian artery, is not always easily to be felt in a deep wound. In some subjects it is very imperfect, and occasionally can scarcely be said to exist.

I have seen four cases in which the phrenic nerve was carried out-

wards, and to the external edge of the scalenus anticus, apparently by the proximity of origin and of the internal mammary artery to that muscle. This would suggest the necessity of great caution and delicacy in the use of the aneurism-needle in that situation.

In a recent dissection, I noticed the subclavian artery of the left side, lying in the midst of the brachial plexus; two of the nervous cords composing it lying above the artery, and three below it. Such a case might explain the mistake which has sometimes been made even by experienced operators, where the ligature has been placed around one of the nervous cords instead of the artery, which they would naturally search for upon the surface of the first rib.

I have sometimes found the omo-hyoid muscle of uncommon size, flattened out, and occupying a very large space between the edge of the trapezius and that of the sterno-mastoid. This condition of things might perplex one who was unaware of its occasional occurrence, as well as that which is sometimes seen in very muscular subjects where the edges of the sterno-mastoid and trapezius almost touch each other.

In three cases, the *axillary artery* divided just before reaching the tendon of the *teres major* into radial and ulnar. These arteries were united just below the bend of the arm by a branch which, in the three instances, averaged the size of a crowquill. If, in any of these subjects, brachial aneurism had existed during life, and one of these vessels in the arm had been ligated for the brachial artery, the other vessel would have kept up the supply of blood to the aneurismal sac through the transverse branch; and the operation, in all probability, would have been unsuccessful.

I have made notes of ten cases in which the *vertebral artery*, instead of entering the foramen in the sixth cervical vertebræ, as is usual, passed into the fourth, sometimes the third, and, in one case, into the second cervical foramen, lying in its course upon the anterior face of the bodies of the cervical vertebræ, and, therefore, being as much exposed to injury from incised or punctured wounds, as is the common carotid.

Wounds of the vertebral have, indeed, been mistaken for those of the carotid, and ligatures have been placed upon the last-named vessel to arrest hemorrhages, which afterwards upon dissection were ascertained to have been caused by a wound of the vertebral. (See Mott's *Velpeau*, vol. ii. p. 228, where several such cases are related.)

Of the Carotid.—The usual point of bifurcation, as is well known, is opposite to the superior margin of the thyroid cartilage. It is not uncommon, however, to find its division somewhat below this point, or it may be above, opposite the cornu of the os hyoides. In one subject, I found the division of the common carotid at the lower margin of the cricoid cartilage, and I have notes of three cases, and remember to have seen a few others, where the carotid bifurcated just opposite the angle of the lower jaw.

The origin of the branches of the *external carotid* is not unfrequently anomalous. I shall merely mention, that among my notes I find one case

recorded in which the *inferior* thyroid came off from the right common carotid, one inch from its origin from the innominate. Also another, in which the *superior* thyroid arose from the right common carotid three-quarters of an inch above its origin from the innominate. In one subject, the principle of compensation, so frequently seen in the arterial system, was well exhibited. The *superior* thyroid came off from the common carotid three-fourths of an inch from the bifurcation. There was no *inferior* thyroid on that (right) side. On the left, there was no *superior* thyroid, but a very large *inferior* thyroid. These deviations from the normal standard are interesting, with reference to ligature of the thyroid arteries, which have sometimes been tied in cases of bronchoecle. I have several times seen the superior thyroid arising by a common trunk, with the lingual or the facial.

Of the Circle of Willis.—In a few instances I have found this circle imperfect, in consequence of the absence of the posterior communicans artery, or, more rarely, of the anterior communicans—at the same time, the vertebral artery of one side was very small. It has sometimes happened, after ligature of the common carotid, that the patient has shortly afterwards been attacked by delirium, convulsions, hemiplegia or partial paralysis, terminating in a few days by death. On dissection, softening of the cerebral substance has been found. This condition, arising from defective arterial circulation in the brain, may probably have occurred in persons in whom the above anomalous arrangement existed in the cerebral arteries.

Of the Primitive Iliac Arteries.—These, when normal, extend from the bifurcation of the aorta on the lower part of the body of the fourth lumbar vertebra, to the sacro-iliac symphysis. I have seen a few cases in which these arteries were only about an inch, or an inch and a half in length, and have met with one in which the abdominal aorta gave off at once, on the right side, the external and internal iliac, *there being no common iliac* on this side. On the left, the common iliac was only half an inch long, and divided into external and internal iliac. Both obturators arose from the external iliac.

Of the Epigastric Artery.—This is usually a branch of the external iliac. Its relation to the neck of a hernial sac is exceedingly important, so that the operator should be constantly on his guard against wounding it, particularly in femoral hernia. The obturator artery ordinarily arises from the internal iliac. It unfortunately sometimes happens, that in cases of the anomalous origin of the obturator by a common trunk with the epigastric, that the obturator, descending in its course to the thyroid or obturator foramen, almost *encircles* the neck of the hernial sac, so that the operator is liable to cut the artery in whatsoever direction he may divide the stricture. It is, therefore, interesting and important to know in what proportion of cases we may expect to find this dangerous anomaly. I have carefully kept a record of three hundred and fifty-three subjects, in which number I found the anomalous variety of the obturator artery to occur in eighty-eight, which is one in four.

However, supposing femoral hernia to have been present in the cases where the arterial arch or “circle” existed, the hernial sac would not have passed

under the latter in every instance; on the contrary, I found, on careful examination, that the hernia would rather have pushed this circle to the outside, and, at the same time, depressed it in some degree, so that the artery would have escaped in the division of the stricture.

The proportion of cases, therefore, in which there was every reason to believe that the artery would really have *encircled* the sac, was only one in eight. However, as it is impossible to know, previously to the operation for strangulated hernia, whether this dangerous variety of the obturator does or does not exist, and as the vessel is liable to be wounded, by cutting the stricture in any accessible direction, it is recommended, by high authority, to merely *nick* the stricture at several points very cautiously, and only to such extent as may be absolutely necessary to relieve strangulation.

In conclusion, it is hoped that the preceding facts may be of some slight value to the cause of science and humanity.

ART. VIII.—*Case of Extra-Uterine Pregnancy.* By SAMUEL L. KURTZ, M.D., of Phoenixville, Pa.

ON the 12th of July, at 9½ o'clock P.M., I was summoned in great haste to see a young woman who was suffering violent pain, and was thought to have cramp of the stomach and bowels. The patient was a German woman, twenty-one years of age, and married last New Year's day.

More than three months since, her catamenia disappeared, since which time they occurred but once, which was on the 2d of July, while on a visit to Philadelphia. The discharge, which was profuse, lasted but twenty-four hours, occurred at the regular period, and resembled her former courses, differing only in quantity and length of time of discharge. About two months since, she began to have irregular pains in the right iliac and pelvic regions, occasioning considerable uneasiness, and which were much augmented by unusual exercise, laughing, coughing, &c. With this exception, she enjoyed tolerably good health up to 4½ o'clock P.M. of the 12th of July, when she was suddenly taken with the pain in her side. During the day, she had attended to her household duties, done a small wash, had been to the store, and appeared lively and cheerful.

On my arrival at her bedside, I found her very much prostrated; deathly pale; her skin cold, and covered with a cold, clammy sweat; her pulse, at times, was imperceptible, and, when it could be detected, was weak and fluttering, numbering from 120 to 130 beats per minute; her breathing, although not difficult, was hurried, and she felt nausea, and had vomited once.

The pain, which extended from the epigastric to the right iliac region, at first was intermittent, and assumed the characteristics of labour pains, but was now continuous, and much increased by pressure. Her abdomen was full and somewhat tympanitic. Her most easy position was on her left side, with her thighs flexed, occasionally turning on her back, to resume her former position in a very short time.

Before my arrival, she had passed a large quantity of hard, impacted feces, without materially increasing the pain.

By examination per vaginam, I found the uterus in its proper place, with the os tincæ dilated to the size of a shilling, but no discharge. Opiates were given to allay the pain, and an enema was ordered. I called again at 3 o'clock next morning, and found her growing worse. Her pain was even more violent than when I had left her; her breathing now was painful and difficult, as well as hurried; pulse imperceptible; had vomited once since my former visit, and her skin continued cold and perspiring. Large doses of morphia were now given at intervals of twenty minutes, which apparently afforded her some relief. At 4½ o'clock she turned on her right side for the first time, and fifteen minutes afterwards was a corpse.

A *post-mortem* examination was made by Dr. L. Quick and myself, nine hours after death. Having carefully dissected the anterior parietes of the abdomen, we found its peritoneum lining without any appearance of inflammation. Dividing the membrane, the abdomen was found entirely filled with blood and bloody serum, the clots of blood filling the interstices of the viscera. After about three pints of blood were removed, the omentum was seen to be somewhat injected. Removing the clotted blood from the pelvis, the fœtus, with the membranes perfect, was seen floating immediately above the uterus. Pushing this aside, a large tumour presented itself in the right pelvic region, which proved to be the enlarged Fallopian tube, which had contained and nourished the fœtus up to the time of its bursting.

The walls of the tumour, which was very vascular, on one side were thick and strong, on the other thin and extenuated. The ovarium of the right side was somewhat enlarged, and the tubes apparently elongated. The obstruction occurred in the external half of the tube.

The fœtus was well formed, measuring 8 inches in length when extended, and 3¾ when flexed. The membranes containing the liquor amnii were perfect, and the amount of fluid quite as much as usual at that period of intra-uterine gestation. Judging from the size and formation of the fœtus, as well as from the woman's history of herself, we thought she had gone between three and four months.

The womb, which was larger than the non-gravid womb, was apparently healthy, measuring 4½ inches in length, 3¼ in breadth, and 1¼ in thickness. The canal of the cervix was filled with a ropy and tenacious fluid, and, by pressure on the womb, it discharged a claret-coloured mucus. The left ovarium and Fallopian tube were unchanged and healthy in appearance.

PHENIXVILLE, July 14, 1855.

ART. IX.—*Case of Diaphragmatic Hernia*.—By CHAS. W. CHANCELLOR, M. D., Alexandria, Va.

ON Monday, Sept. 3d, 1855, I was called to see J. P——, ætat. 6; delicate frame; light complexion; strumous habit. His mother informed me that the boy had been unwell, to her knowledge, from the Friday morning previous, complaining of pain in the left shoulder and side, with occasional vomiting, and had had no evacuation from the bowels since Wednesday, August 29th, for which calomel and oil had been given, and retained without producing any effect. He had taken no food, except a piece of bread, which was immediately ejected. Water could be retained only in small quantities. His appearance at this time was quite natural, with but little expression of suffering; he still complained of pain in the left side, which was slightly increased by pressure under the margin of the ribs of that side; there was no

pain elicited on pressure elsewhere. His abdomen was very much distended and tympanitic throughout its whole extent; skin hot and dry, tongue furred and coated with a light brown deposit; pulse accelerated, but otherwise normal; respiration slightly hurried but easy, no cough, and but little thirst.

On questioning the mother of the boy, she stated, "that on the day previous to his complaining, he had in a scuffle with a play-fellow been thrown across a plank, on the abdomen;" but there was no external evidence of injury. I ordered an active purge, to be followed by a purgative enema.

Tuesday, Sept. 4th, the enemata had produced quite a large evacuation, but there was no improvement in the general condition, showing that the discharge had come only from that part of the intestinal canal below the point of obstruction. Thinking the case to be one of invagination, I proceeded to treat it *secundum artem*. The countenance daily grew more anxious and dejected, and all the symptoms more aggravated, until death furnished relief. His mental faculties remained unimpaired throughout the disease, and intelligent—I might say precocious—answers were given to questions, until the hour of death, notwithstanding a large amount of opiates had been used to palliate suffering and quiet the stomach, which had become very irritable; but at no time was there stercoraceous vomiting. Nothing was ejected but the articles swallowed, mixed with the mucus of the stomach. About twelve hours before death, which occurred on the 8th of September, nine days from the period of attack, the pulse became slower and softer, and the surface covered with a profuse perspiration: the extremities retained a pleasant temperature.

Having examined all of importance connected with the onset and progress of the case, we will now proceed to the pathological condition furnished by a *post-mortem* examination, which was conducted in the presence of several distinguished professional friends, fifteen hours after death.

The only marked abnormal appearances externally, were a slight general emaciation and great discoloration of the integuments of the abdomen. On opening the cavity of the abdomen, the bowels, viewed *in situ*, as may be inferred from the foregoing, were very much distended. The stomach and liver were in a healthy condition, the latter natural in size and colour, and the gall-bladder well filled with bile. The bloodvessels of the intestines were considerably injected. Continuing our examination, by tracing up the colon from the ileo-cæcal pouch, we discovered that the large intestine was perforated just at the upper part of the angle which it makes in forming the transverse colon. The perforation was about seven-eighths of an inch in length, sufficiently large to permit the escape of the contents of the intestine, which had been poured out into the peritoneal cavity: about two and a half inches to the left of this perforation a knuckle of the transverse colon, and also of the jejunum near its junction with the duodenum, had passed through an artificial opening in the middle of the left leaflet of the diaphragm, and were tightly constricted. The contents of the bowels, finding an obstacle to its progress in every direction, had pushed the diaphragm high up, considerably diminishing the thoracic cavity. The diaphragm was then cut loose from its attachments with the ensiform cartilage and ribs, and the viscera of the thorax exposed. Two or three inches of the colon and jejunum were found to be included within the cavity of the chest in a gangrenous state, and adherent to the inferior lobe of the left lung, which latter was also attached firmly to the upper surface of the diaphragm.

Remarks.—This was a case of unequivocal phrenic hernia, of a very uncommon, if not wholly novel character; for it occurred not at the weak tri-

angular space of the ensiform cartilage, but at a point which in health is the most impervious of the muscular septum—a point not weakened by deficiency of muscular substance, nor perforated by natural openings. Could this hernia then have been the result of congenital weakness or malformation of the part? Could a fall or blow upon the epigastric region, sufficient to rupture this muscle, have been received without producing great, if not fatal concussion of the solar plexus?

Another cause of wonder is, that more disturbance of the respiratory function did not exist, since the left lung and diaphragm generally were so much involved.

ART. X.—*Hydrocele permanently cured by the Injection of Iodine and a Seton combined.* By ISAAC LEFEVER, M. D., of Anderville, Perry Co., Pa.

ON reading an article in relation to operations for hydrocele in the last number of the *Journal*, the following cases were brought to my recollection:—

CASE I.—Was called on in 1847, by a gentleman labouring under hydrocele of the right side of the scrotum. On the 17th of June of that year, I operated by means of a trocar and canula, and removed about $\frac{3}{4}$ of fluid; immediately thereafter I injected several ounces of port wine and water. In a very short time the fluid again collected, and on the 4th of March, 1848, I performed a second operation. On reflecting on the nature of the case, I concluded that the reason why operations so often fail to effect a cure is because the fluid is not entirely evacuated, and the sides of the sac consequently, from its presence, are prevented from coming completely together; and that if some plan were devised for keeping the orifice open, so as to allow of the escape of the retained fluid, and what little might be formed immediately after the operation, success would more frequently follow the measures adopted. The idea of introducing a seton, after removing the fluid by the trocar, presented itself; but fearing it might not be satisfactory in the end, I operated as follows: The fluid, about $\frac{3}{4}$ vi, was evacuated as before, after which I injected about $\frac{3}{4}$ j tinct. iodini. In a few moments the tincture was allowed to pass out, and I introduced, through the canula, the end of a cord, formed by twisting ten or twelve strands of ordinary linen thread together, and upon which I had made a pretty large knot, into the cavity of the sac. The canula was then withdrawn, and the other end of the cord left hanging out of the orifice. Along this cord there was a little discharge, slight suppuration occurred around it, and after a few days it passed out. Some inflammation ensued, but was easily subdued. A cure followed this operation, as there has been no return of the disease.

CASE II.—In this case, several previous operations had been performed by others, but were not effectual in curing the disease. Twice, if I mistake not, the fluid had been evacuated by the trocar and canula; once the scrotum had been freely incised, and the cavity of the sac exposed; and, I think, the gentleman informed me a tent was also inserted. On the 3d of June, 1848, I operated in the same manner as in the second operation in Case I., removing about $\frac{3}{4}$ viii of fluid. But little inflammation followed, as in the former instance, and a like satisfactory result ensued.

REVIEWS.

ART. XI.—*Yellow Fever, considered in its Historical, Pathological, Etiological, and Therapeutical Relations. Including a Sketch of the Disease as it has occurred in Philadelphia from 1699 to 1854. With an Examination of the Connections between it and the Fevers known under the same name in other parts of Temperate, as well as in Tropical Regions.* By R. La Roche, M. D., Member of the American Philosophical Society; of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine, and Foreign Associate of the Medical Society of Emulation, of Paris; of the Academies of Sciences of Turin, Copenhagen, Stockholm, Nancy, and New Orleans; of the Medical Societies of Naples, Marseilles, Lyons, etc. Philadelphia: Blanchard & Lea, 1855. 8vo. 2 vols. pp. 1472.

To a large body of American physicians, the yellow fever continues to be a subject of deep and absorbing interest. Occurring, as it does, almost annually, or, at most, at comparatively short intervals, in numerous localities within the United States and upon its immediate borders; and being productive often of a mortality truly appalling, everything connected with its etiology, pathology and therapeutics must necessarily command the serious attention of the profession. Not merely of those physicians who reside within the localities to which the disease is ordinarily endemic, but of those, also, who, although residing beyond these localities, may be called upon to treat the disease, under certain contingencies, the occurrence of which, as is proved by the experience of the past, is by no means problematical.

Numerous treatises upon yellow fever are extant; many of them particularly valuable. We know of none, however, which can be received as a complete monograph of the disease, or as presenting even a good digest of all the facts known in relation to it, with the deductions to which these facts legitimately lead.

What we want is, not an account of the disease drawn up from the history of a single epidemic, or from its repeated occurrence in the same localities—a theory of its origin and pathological character derived from partial and limited observations; but a cautious examination into its causes, character, symptomatology, and treatment, based upon a full and candid analysis of all the well-authenticated facts, in reference to these several points, which have been collected during the various occurrences of the disease, in different portions of our own and other countries, as we find them recorded by those who have had the best opportunities for its investigation. A series of partial, or one-sided observations, may lead to conclusions the most inaccurate, in relation to the causes and character of yellow fever, which can be corrected only by a patient examination and comparison of all the observations to which we have access, bearing upon the particular points at issue.

The work of Dr. La Roche appears to us to present all that could reasonably be expected in a monograph on yellow fever, and to supply very fully a

deficiency in our medical literature which has every year been more sensibly experienced.

With an extent of research, and a closeness of analysis seldom surpassed, the author has collected and collated and arranged all the materials presented by antecedent and contemporary authorities, so as to present a clear and accurate exposition of every point connected with the disease, as well in reference to its origin, its pathological characteristics and symptomatology as to the remedial measures that have been found best adapted to conduct it to a favourable termination. It is, unquestionably, the most complete and satisfactory monograph on yellow fever that has yet appeared, and will, we are persuaded, be received with no slight degree of favour by the entire medical profession.

There is scarcely a work, on the disease, of any value—scarcely a communication presenting a fact worthy of note, scattered through the medical periodicals of Europe and America, that has escaped the notice of Dr. La Roche. The originals have, with a very few exceptions, been consulted. The references made by the author, and the facts adduced by him, are, therefore, of unquestionable accuracy, being rarely derived from secondary sources. Hence, simply as a complete and faithful digest of the facts heretofore collected in relation to the yellow fever, the work of Dr. La Roche is one of no trifling value. But it is not merely as a digest, however reliable, of the accumulated observations of the profession on the disease of which it treats, that the treatise before us recommends itself to the notice of the profession; it has a much higher and more important character. It presents one of the fullest and most accurate delineations of yellow fever—its origin, invasion, semeiology, course, and terminations; the modifications it experiences from concurrent diseases; its diagnosis and prognosis; its pathological characteristics, prophylaxis and treatment—with which we are acquainted.

Every question connected with the disease is examined in detail, and so fully as to satisfy, so far as the present state of our knowledge is concerned, every rational inquiry in respect to it. On certain points, the author may, perhaps, be accused of dwelling to an unnecessary length; but when the importance of the subject in all its details, and the disposition that has recently been evinced to substitute, in reference to the etiology and pathology of yellow fever, hypothesis for facts, the necessity of the full and extended examination in which Dr. La Roche has entered, will be perceived.

On controverted points, including the entire question of the causes productive of the disease; its relationship to the other fevers endemic to the localities where it occurs, and its true pathological character, while the utmost candor is exhibited, the facts adduced by all parties being fully and fairly presented, the close analysis to which these facts are subjected by the author, and the strictly logical character of his deductions, give to the views he advocates a force almost irresistible.

Upon everything, in fact, connected with the subject of yellow fever, the work of Dr. La Roche will be at once received and long remain a standard authority.

The treatise commences with an excellent historical sketch of the epidemics of yellow fever which occurred in Philadelphia between the years 1699 and 1762, and a more detailed account of those of 1793 and '94; of 1797, '98, and '99; of 1802, 3, and 5, and of 1820, and 1853; prefaced by a description of the medical topography, climate, and population of the city at those different periods. In the ninety-seven pages devoted to these "preliminary observations," numerous prominent facts are developed having an

important bearing on the general subject of the disease. This commences with the consideration of its medical history; and first, of its altitudinal and geographical ranges.

As a general rule, the disease may be regarded as one of low, flat, and level localities, and as appertaining more especially to hot latitudes. It never shows itself beyond a certain elevation, and, in this respect, is more restricted than ordinary paludal fevers.

"The inability," remarks Dr. La Roche, "of the yellow fever to be generated at a high elevation above the level of the sea, depends in part on the greater elasticity and purity of the air, and diminution of atmospheric pressure, and on a more thorough ventilation. But the main cause is the absence there of the degree of atmospheric heat, which, as we have seen, is indispensably necessary for the elaboration of the morbid agent to which the disease is due. For the same reason, in part, though not exclusively, its geographical limits are restricted within certain bounds in a northern direction; while, in a southern, the same effects are produced, as it would seem, by an excess of heat and a variety of influences of a meteorological and telluric nature. Be the causes, however, what they may, on one point there can be no doubt—that the yellow fever has geographical limits, beyond which it does not appear—and that, within those very limits, there are many places where its usual apparent cause would seem to exist, but where, nevertheless, it has never shown itself, or has done so very seldom. The West India Islands, and part of the coast of South and North America, constitute its proper soil. From Brazil to Charleston in one direction, and from Barbadoes to Tampico in another, the causes of this form of fever are in constant, though unequal force, in regard to different seasons and localities. It prevails often, though not very generally, in some places somewhat more north than Charleston; visits, occasionally, the Atlantic cities of our Middle States, and has ascended as far as Boston; while, in the Mississippi Valley it has prevailed, as already seen, as high as Memphis, perhaps Gallipolis, or even higher. In an eastern direction, but within the same latitudes, it has extended to Cadiz, Xeres, Carthagen, Malaga, Alicant, Seville, Barcelona, and other cities of the coast and the interior of Spain. It has prevailed several times at Gibraltar, once at Rocheford, once at Lisbon, and once at Leghorn. Hence, we find it embracing a considerable portion of the earth's surface. In its fullest latitudinal extension, it reaches to between the twenty-second and twenty-third degrees south of the equator, and, on the other side, to the forty-second degree on the Atlantic coast, to the thirty-fifth degree on our Western waters, and to the 8.56° on the Pacific. Considered only in reference to its legitimate longitudinal boundaries, it stretches from about the sixtieth to the ninety-seventh degree of west longitude; while, if we take into account those points in Europe most distant from us, where it has occasionally appeared in an epidemic form, it will be found that its eastern limits may be traced to the second degree of longitude east of Greenwich. Its true area includes the Caribbean and other islands called the West Indies and Bahamas; the contiguous coast of Colombia and Guatimala and the extensive shores of the Mexican Gulf, sweeping from Cape Catoche on the west, to Cape Sable on the east, and running thence along the coast of America to Wilmington, N. C., Norfolk, Baltimore, Philadelphia, New York, Boston, and intermediate towns; in some of which places it is an occasional, not annual, or even frequent, visitor.

"Until recently, the river Amazon, which divides Brazil from Guiana, formed the boundary of the disease south of the equatorial line; for, although it is said to have prevailed at Olinda from 1687 to 1694, and to have shown itself as far as Montevideo, in the beginning of the present century, the latter circumstance is open to some doubt; while in Brazil, from the close of the 17th century to the middle of the present, the disease was not observed. Since 1850, it has invaded Rio Janeiro, Bahia, Pernambuco, and other places of that country. It is, in greater measure, a stranger to the Pacific, having prevailed but once at Panama, twice at Guayaquil, and once at Callao. It does not appear

in the East Indies; the fever described by Wade, Fontane, Lind (of Windsor), Johnson, Twining, and other writers, appertaining to the class of common bilious remittents. It has never prevailed in China, Singapore, Siam, Ceylon; it has prevailed only occasionally on the African coast, Senegal, and the Gold Coast, and has but three times, in the space of eighty-six years, showed itself in Cayenne.

"Within those limits, it has in some one or more places originated and prevailed to a greater or less extent, occasionally or frequently, either as an endemic or as a mild or wide spreading epidemic. Beyond these it never shows itself; and though—whether north or south, east or west—it does not reach the point at which common malarial fevers stop; it approximates to these diseases, so far, especially, as its northern or western extension is concerned, it being circumscribed within certain bounds; for they, too, have their limits. The effect, in both instances, is due to modifications in the same morbid agencies."

While the yellow fever does not vary, either in regard to its pathology or its pathognomonic phenomena, in whatever region or at whatever period in the same place it may show itself, whether sporadically or in an epidemic form, it, nevertheless, like many other diseases, assumes, in different places, and at different periods, and often in the same place and during the same season, diversified aspects or characteristics, proceeding, not from any specific difference of nature, but from various degrees of reaction; from a tendency to depression in the vital force; from the predominance of certain symptoms, appertaining or not to the disease, and varying according to the greater or fewer number of the vital organs involved in the derangement of the body, or from other causes; the whole depending in peculiarities of constitution, temperament, habits, or conditions of health of those attacked; in a difference of intensity in the morbid agent, and on numerous contingencies resulting from the nature of the locality, and the degree of temperature, humidity, and other phenomena in the surrounding elements.

In his description of yellow fever, Dr. La Roche, after presenting an account of the symptoms by which the disease is characterized, in the progressive order in which they present themselves—attention being paid to the stages into which the attack is divided; that of reaction, that of calm or remission, and that of exhaustion—indicates the several leading types under which the disease may occur, namely: first, the inflammatory, which he describes under three different grades; the intense, the mild, and the ephemeral; second, the congestive form, having four grades, the aggravated, the adynamic, the walking, and the apoplectic.

The symptomatology of the yellow fever, as delineated by Dr. La Roche, is marked by extreme accuracy, precision, and clearness. Studied in connection with the very full examination into which the author enters of each of the characteristic phenomena of the disease—its premonitions and mode of invasion; the disturbances in the circulatory system by which it is attended; the condition of the skin and of the digestive organs; the state of the tongue and respiration; the character and location of the pain so generally present; the characteristics of the countenance; the state of the urine, and of the muscular and nervous systems; as correct an idea of yellow fever will be acquired as it is possible to derive simply from any mere description, however minute and faithful, unassisted by clinical experience.

In regard to the state of the blood in yellow fever, a question upon which some discrepancy of opinion exists, Dr. La Roche, after a very full survey of the facts recorded by different medical observers, as well as from a careful collation of these with the facts that have fallen under his own notice, arrives at the following conclusions:—

"1. In some instances, the blood appears at first unaltered as regards colour, proportion, firmness of the crassamentum, power of coagulation, quality of the serum, &c. and continues so, till the accession of the second stage.

"2. More generally, and even in cases in which, judging from its general aspect, it would seem to preserve its usual characters, the blood is found more or less—often very considerably and as often completely—deprived of its power of coagulation; the crassamentum, when the separation takes place, being generally found broad, flat, thin, of soft consistence, and diffuent.

"3. But this inability or reluctance to coagulation, though generally exhibited, is not universal, and cases occur in which the blood coagulates with as much readiness and firmness as in other diseases.

"4. In many cases, the power of coagulation, though not lost, is tardy in manifesting itself; often it does not do so until the blood has been for some time removed from the body and exposed to the action of the atmospheric air.

"5. In many instances the blood, from the onset of the attack, though, more generally, after the accession of the later stages, not only manifests a reluctance to coagulate, but remains fluid, and, as it were, dissolved.

"6. In some cases, during most epidemics, and very generally during others, the blood is found sily, and even cupped, and presenting, more or less exactly, the appearance described by Dr. Rush.

"7. More frequently the blood does not present a buffy character, and is neither sily nor cupped.

"8. In some instances—during some epidemics and seasons, very generally—the blood, at the onset of the disease, is of a florid, brilliant red or scarlet colour. This appearance is more common when the circulation is more than usually rapid.

"9. In all cases, whatever be the appearance of the blood at the onset of the attack, it becomes dark, and even black as the disease advances and passes to the last stage. In many instances, and during some epidemics, in the large majority of cases, it assumes a dark hue throughout the whole course of the disease; and in all instances it is found so after death.

"10. In some instances, according to Warren, Hillary, Moultrie, Stevens, and Caisergues, the crassamentum presents dark blackish spots on its surface.

"11. It is not unfrequent when the blood is dark, while issuing from the body, to find it becoming more florid as it flows, or after exposure to the air.

"12. In some instances, the blood presents a mixed appearance, as if the vein had contained two differently coloured fluids—the one bright red, the other almost black.

"13. In some instances, the blood has been found, even at the onset, quite fluid and of thin consistence.

"14. In other cases, it is represented as thicker, in the early and after stages, than in health, and as having the appearance of molasses. The same character of the blood is noticed after death.

"15. In some instances, there is uncommon reluctance in the flow of blood, although the pulse may feel strong and full.

"16. As regards the arterial blood, Dr. Hacket (*Med.-Chir. Rev.*, xvi. 291) found it, in the living subject, to present its characteristic bright scarlet hue. But that such has not always been the case, is fully proved by the statements of older and modern writers. Towne observed that not only the venous, but the arterial blood also, was dark, and became 'a lazy, stagnating, and inactive puddle.' Dr. Stone found it black, at Woodville, in 1844, and at Natchez, four years after. Pariset affirms that the arterial blood presented the same characters as the venous, in regard to colour and consistence, in Barcelona, in 1821. We have also seen that similar observations were made by Dr. Stevens, of Santa Cruz; and every dissection, those of Dr. Hacket not excepted, shows that, after death, the blood found in the left cavities of the heart differs in nothing from that contained in the right.

"17. The increased heat of the blood beyond the healthy standard, in the early stage of some forms of yellow fever, is placed beyond a possibility of doubt, and cannot be matter of astonishment. It is, of course, not peculiar

to the disease, and is generally, though not always, found in other fevers and the phlegmasia.

"18. It has been seen that, while changes are effected in the character and appearance of the blood generally, and in the consistence and colour of the crassamentum, alterations of equal importance and extent are noticed in the serum, both in respect to quantity and coloration.

"a. In many instances, the number varying at different seasons, there is no separation into crassamentum and serum, the blood drawn retaining its homogeneity, and this, whether it coagulates or remains fluid.

"b. In many cases, when the separation into crassamentum and serum takes place, the quantity of the latter is much smaller than in the normal state, especially in the early stage of the disease.

"c. On the other hand, Drysdale remarks that in some cases the quantity of serum was in excess. Dr. Imray, too, states that, at Antigua, in 1838, the serum was in larger quantity than natural, even at the onset of the attack; so, likewise, Gillespie and Moultrie.

"d. Like Dr. Rush, Dr. Drysdale states that in some cases the serum had the appearance of water in which meat had been washed. Dr. Caisergues makes similar remarks as to the blood issuing from the outlets of the body.

"e. The serum is usually found of a yellow or an orange colour.

"f. In some instances, a portion of the red or black extraneous matter is precipitated to the bottom of the cup, or the serum mixes intimately with these, and acquires thereby a bright scarlet, or dark or muddy hue, as the case may be.

"19. It remains to remark, in reference to the state of the blood in yellow fever, that it has been found to emit a peculiar smell, which differs from that emitted by the blood in other fevers—plague, typhus, typhoid, &c.—and is probably produced by a volatile salt of ammonia.

"The foregoing facts and statements," says Dr. La Roche, "can leave no doubt in our minds that the blood in the yellow fever approximates, to a great extent, to the condition it presents in other pyrexia of the zymotic class. It exists, in varying degrees, in all malarial diseases, from the simple intermittent to the malignant remittent, as well as in typhoid, typhus, and relapsing fever, to say nothing of cholera, the plague, and eruptive fevers."

The hemorrhages which occupy so conspicuous a place in the list of the symptoms of yellow fever, are treated of at some length by Dr. La Roche. Though not peculiar to the disease, they constitute one of its main characteristics. They occur in many mild or moderate cases; are absent in but few of the violent, malignant, and fatal ones; and issue from a greater variety of parts than is the case in most other complaints. They may proceed from the skin, the lining membrane of the nose, the gums, tongue, mouth, fauces, conjunctivæ, ears, œsophagus, stomach, large intestines, or bladder.

The tendency to hemorrhage in yellow fever is not equally conspicuous in all places and at all times. In one epidemic, also, a certain form of hemorrhage will be often encountered, which, at another season, seldom shows itself, while others are predominant. At one period, some hemorrhages tend to a favourable issue, which at other times fail more or less completely in this respect, and point to an opposite result.

Besides these external hemorrhages, blood is often extravasated in the substance of the tissues or organs, producing petechial spots, ecchymoses, and intermuscular or cellular collections.

The facts in elucidation of the foregoing positions, Dr. La Roche has collected and arranged with much clearness.

The source of the jaundiced hue of the skin in yellow fever, and its relative value and import as a symptom of the disease, are questions of no trifling interest, and, as such, are examined at considerable length by Dr. La Roche.

"Natural, however," he remarks, "as it might be to infer, from the very general adoption of the name of yellow fever, that the symptom in question is a constant attendant on the disease, experience has amply shown that such is far from being the fact, and that, in a large number of instances, the case proceeds to a favourable or fatal issue without being characterized by the phenomena in question."

"Nothing, as has long since and repeatedly been pointed out, in this country and elsewhere, is more evident than the impropriety of the name of yellow fever, inasmuch as the peculiar discoloration from which that name is derived is not pathognomonic of the disease; for not only is it, as already said, absent in very many cases throughout the whole course of the attack, but, as we shall see, it often does not show itself until after the death of the patient, or at a period when the true character of the disease has already been fully made out; while, again, yellowness of the skin is an attendant on other complaints having little, if any, pathological and etiological connection with the yellow fever."

"As regards the proportionate number of cases in which the jaundice occurs, we possess but few data upon which to form a positive conclusion. From all that we can gather on the subject, we may infer that the number varies in different places during the same or different seasons, being limited in some, and considerable in others; that the same difference is found to occur in the same place during different seasons; and, moreover, that the proportion varies according to the issue of the disease and the rapidity of the attack."

"To this it may be added that the frequency of jaundice varies at different periods of the same epidemic. In some seasons, it is more common at an early period, and gradually becomes less so as time advances; in others, the reverse is the case. The early cases rarely present the icteric coloration, at least during life; while later the skin becomes yellow in almost every instance." "As a general rule, jaundice may be stated to present itself much less frequently in those who recover than in those who die." "Its occurrence also varies in point of frequency according to the rapidity of the cases, whether these terminate in recovery or death. In this country, when the disease is cut short, by art or the efforts of nature, in a short time, or when it carries off the patient before the third day, yellowness is of rare occurrence. Statements of the same kind are found, so far as concerns the West Indies, in the writings of Dr. R. Jackson and others."

"After what has been said of the frequent absence of jaundice in cases that recover, we must naturally be prepared to find it more frequently in the malignant form than in mild cases of the disease, death being more usually the result of the former. Such, however, is the case, whether they terminate favourably or fatally."

"Some difference exists, in various places or seasons, as also in various cases during the course of the same epidemic, in regard to the particular hue of the skin—the shades varying from a pale straw or muddy to a dark-reddish, or brown mahogany, or copperish colour, embracing between the two extremes the different modifications of yellow, orange, or saffron, light and deep. It is generally of a uniform, but sometimes of a mixed and variegated character. These various shades of colour do not appertain individually to any special region, but have each been found to occur, under particular circumstances, in all parts where the fever has prevailed—sometimes at different seasons, at others during the same season. Such has been the case in this city, where, however, the deep tints mentioned seem to have more generally been observed."

Some difference of opinion appears to exist as to the degree of importance to be attached to the jaundiced coloration of the skin in yellow fever, in forming an estimate of the probable issue of the case. In reference to this point, Dr. La Roche examines the symptom relatively to the time of its appearance, the shade it assumes, as well as to the degree, extent, and rapidity of its diffusion.

While by a few writers a general yellow suffusion of the surface is considered of little importance in yellow fever, by the larger number of authori-

ties it is maintained—and the experience obtained in this city, as elsewhere on this continent, may be adduced in their support—that the early appearance of jaundice is indicative of danger, and that this danger increases in proportion to the earliness of its accession.

“That,” remarks Dr. La Roche, “the appearance of jaundice must generally be viewed as a sign of serious import, especially when manifesting itself early, no one can deny. Under most circumstances, it seems to show that the cause has made a strong impression on the system, marking, as Dr. Musgrave says, the intensity of the disease and its degree of danger. But every physician who has seen the yellow fever must have noted not a few cases, even of a serious character, and when the jaundice has appeared early, or attained considerable intensity, which nevertheless have terminated favourably. Examples of this kind are mentioned or referred to by competent authorities on the subject. We know, also, that in the mild forms of the disease, which often, if not generally, end in recovery, jaundice is not unfrequently an attendant symptom.” “So far as regards the early and middle periods of the disease, every physician—with few exceptions, perhaps—agrees in regarding jaundice, when it appears after the sixth, but more particularly the seventh or eighth day, as of little import, or even as a favourable or critical sign.”

In regard to the connection between certain shades of discoloration of the skin and the several varieties in the form and the degree of malignancy of the disease, Dr. La Roche lays it down as an established general rule that, all things being otherwise equal, the severity and danger of the attack is proportioned to the deepness and general diffusion of the discoloration; at the same time, cases not unfrequently occur—in some epidemics very generally—in which the light lemon-colour of the skin is an index of greater danger than a dark yellow.

Nor do the period at which the jaundice appears, and the particular hue it assumes, alone furnish us with indications as to the issue of the disease. Considerable importance is due to the degree of rapidity with which it spreads and increases in intensity. The danger of this rapidity in the diffusion of jaundice has been at all times noticed; while the comparative innocuousness of the discoloration, and even the benefit resulting from it when it occurs gradually, have not been overlooked.

On the subject of the cause of the discoloration of the skin, much difference of opinion has and still continues to exist among physicians conversant with the disease. By one party it is ascribed to a redundancy of bile, and the regurgitation and absorption of a part of it, either before or after its discharge from the liver, owing to some obstruction to its passage into the duodenum, from spasm of the duct or other parts; gall-stones, or simple inflammation, and consequent thickening of the mucous membrane; from severe contraction due to violent vomiting and straining, forcing the fluid into the vena cava; or, as is more recently believed, to the retention in the blood of the constituent principles of the bile, arising from a deficiency of hepatic secretion and elimination.

By others it is maintained that the discoloration is the result, in part at least, of the yellowness of the serum, arising from a colliquation or dissolution of the red globules, its separation from these, and admixture with their colouring matter and subsequent effusion under the cuticle; or, in great measure, of an *error loci* of the diseased globules in the white vessels, or the cellular tissue, as in ecchymosis; in other words, of the existence of a congestive state of the subcuticular capillary network, akin to that occurring in the mucous membrane, and giving rise to hemorrhage.

The doctrine of the strictly bilious origin of the jaundice in yellow fever is founded—

"1. On the analogy and supposed identity of the yellow with other fevers in which jaundice is undeniably due to bilious admixture, from absorption or other causes. 2. On the diffusion of the yellowness. 3. On its peculiar hue, and the close analogy between the latter and the discoloration in ordinary jaundice, in which the effect is due to such absorption (in the event of the existence of obstruction), or to the retention in the blood of the constituents of the bile. 4. On the concomitant deficiency of the bile in the *primæ viæ*, and the presence of the fluid, or of its colouring matter, in the products of the emunctories—more especially in the urine, judging from the colour of that fluid, and its becoming greenish on the addition of an acid. 5. On the ordinary association existing between its presence and the act of vomiting, which often brings on a jaundiced condition of the skin. 6. On the occurrence of the same discoloration, from an analogous cause, in cases of disease arising from the action of animal viruses or mineral and vegetable poisons. 7. On the rapidity with which it often disappears during convalescence. 8. On the impossibility of referring the lemon or deep orange hue, which the surface often assumes, to any other cause. 9. On the frequent existence of inflammation of the gall-bladder, biliary ducts, and duodenum, as revealed on dissection, a condition of parts which often gives rise to jaundice, and can only produce the effect by obstructing the passage of bile. 10. On the peculiar condition of the liver, which puts an impediment to the secretory functions of that organ, and, preventing the elimination, through its agency, of the constituent materials of the bile, causes it to be thrown on the tissues. 11. On the fact of the pre-existing discoloration of the serum of the blood, which discoloration arises from the presence of the constituents of bile.

"Such a discoloration, says a recent writer (Levacher), must not be ascribed to the yellow transition of the general ecchymosis, for it takes place at the same time with the latter, and does not succeed it; while, again, it does not occur in other diseases in which ecchymosis presents itself—as in cholera. If the yellowness were the effect of ecchymosis, the skin should first be dark; then it should become yellow, as the ecchymosis disappears. It must depend on some other cause; it exists alone sometimes, and is complicated with effusion of dark-coloured blood, for the production of the bronze and blackish yellow alluded to. Finally, the colour disappears in milder cases, when the skin is of a pale saffron hue, without passing through those gradations observed on the surface during the clearing of sanguineous transudations and ecchymotic extravasations."

Those who refer the discoloration of the skin in yellow fever to a sanguineous origin, support their view by the following arguments:—

"1. The discoloration does not always appear first in the eyes, as is the case in jaundice, but shows itself as frequently, if not more so, in other parts—the face, neck, &c.—and in its progress usually follows the course of large vessels.

"2. It is often partial or in streaks—much darker in some spots than in others. It is a subject of common observation that the bronze or mahogany hue disappears when pressure is made with the finger, but slowly returns when the pressure is removed—a result which does not obtain in bilious jaundice.

"3. The discoloration of the skin in yellow fever sometimes, nay, often, does not make its appearance till after death, when all redness in the capillaries has subsided, and the skin is in a condition similar to that in which it is placed when death takes place, after a part has been severely contused. This appearance of jaundice after death, and its instantaneous development to such a degree as to impart a yellow tinge to all parts of the system, are incompatible with the idea of reabsorption, for the power by which this process is executed must be lost with the death of the parts concerned. With the extinction of life there is a cessation of all organic functions.

"4. The rapidity with which this discoloration is diffused over the body, and with which it assumes a dark, often the darkest hue, is incompatible with the idea of its originating from the common cause of jaundice, which, whether arising from ordinary morbid conditions of the liver or annexed organs, or connected with bilious remittent fever, is of slower progress in both these respects.

"5. The biliary organs never, or very rarely, participate in a marked degree in the symptoms of yellow fever, which progresses, from first to last, without any increase in the quantity or alteration in the quality of the biliary secretion; and though jaundice is generally associated with a deficiency of bile, in many cases of the disease attended with that symptom, the secretion is not invariably deficient in quantity, while the fluid, when secreted, remains generally of natural quantity.

"6. The colour of the surface in the yellow fever does not closely resemble that observed in jaundice or remittent bilious fever; or at least does so in exceptional cases only. The dingy or pale muddy-yellowish tint so common in the former, is found in neither of the others, nor indeed in any disease attended with yellowness due to bilious derangement; while, in many instances, the colour approximates much more to that observed in a contused part, when the blood extravasated under the cuticle is being removed through the process of absorption. The dark brown, yellowish-red, the bronzed or mahogany hue of the skin; the violet or black tint; the marbled or clay-coloured appearance; the dingy gray and dirty olive, are all far removed from any colour resulting from effusion of bile, or of the bilious colouring principle, and noticed in any form of jaundice, but are all readily explained on the supposition of an altered condition of the blood or bloodvessels of the parts—of a change produced by the extravasation or tardy circulation of serum of deep yellow colour, tinged with dissolved red or dark colouring matter, while the dark spots or blotches, discovered in different parts, are evidently and admissibly due to a sanguineous extravasation.

"7. Sometimes, as remarked by Dr. Blair, ecchymosis of the eye, and yellowness of a circumscribed character, co-exist and extend *pari passu*, the yellowness keeping one-eighth of an inch in advance of the ecchymosis. After the absorption of blood in convalescence, a deep orange colour is left in the bloodspots. It may be added, that stains of an ochre colour in the brain and kidneys, in instances in which it may be inferred blood had been extravasated, were found, on chemical analysis by Dr. Davy, to depend on the presence of peroxide of iron.

"8. In jaundice, the urine is coloured by bile. Such is not the case in yellow fever, even at the time that the skin is highly discoloured. Cases doubtless occur in which the urine is found tinged, but these cases are rare, and are found connected with bilious jaundice, which, sometimes, though not necessarily, supervenes in the course of the disease.

"9. The jaundice of the yellow fever is connected with a deranged condition of the blood—imperfect power of coagulation, or a complete destruction of that power, with a yellow or even orange colour of the serum, mixed, in many cases, with a portion of red or dark colouring matter. This is associated with a relaxed condition of the extreme vessels, as evinced by the disposition to, and frequent occurrence of hemorrhage from the mucous membranes and the skin itself. When, with these circumstances in view, we observe the peculiar coloration of the skin in yellow fever—a coloration inexplicable on the supposition of a bilious agency—we may readily attribute it to the cause in question.

"10. The yellowness, which, as we have seen, first occurs in the eyes or face, succeeds to a red turgescence of the skin; as this turgescence, which is generally great, subsides, the dusky tinge is perceived gradually spreading, and becoming dark in proportion as the tone of the vessels diminishes and the alteration of the blood increases. The redness coincides with the existence of the florid appearance of the blood and activity of circulation, and the yellowness with the dark colour of the fluid and lensor of the circulation.

"11. The effect of animal venoms, as well as of vegetable and mineral poisons, in inducing a discoloration of the skin not unlike that of yellow fever, may be adduced in further illustration of a sanguineous origin of the latter, for the action of such poisons, in instances of the kind, is evidently exercised upon the blood, and the yellowness thereby produced is the effect of changes therein occurring, and not the result of a bilious admixture.

"12. Yellowness of the skin, not very unlike that which occurs in some forms or stages of yellow fever, is an attendant on diseases in which the biliary organs

and secretions are not necessarily or often implicated, but in which the blood is invariably morbidly affected—chlorosis, typhus, relapsing fever, &c.

“13. The jaundice of yellow fever, whenever formed, subsides much in the same way as the discoloration admitted to be produced by subcutaneous sanguineous effusion, and this subsidence is not promoted by those means which are found beneficial in jaundice from bilious suffusion.

“14. Few physicians, at all experienced in the yellow fever, will refuse to admit that the dark ecchymotic appearance of the skin is due to the presence of blood under the cuticle. If such be the case, we can see no reason to deny a like origin to the mahogany and bronze appearance of the surface, gradually down to the dark, dusky, and finally dull, pale yellow hue which it assumes, all of which are known to be producible by such a cause, and not to arise from bilious suffusion; particularly when we bear in mind that the ecchymotic appearance never exists alone, but is always combined with a jaundiced discoloration of the skin.

“15. Jaundiced suffusion, in diseases in which it is the effect of bilious absorption, or of the retention and accumulation of the elemental constituents of the bile in the blood, and the want of their elimination therefrom, whether those diseases be of a febrile character or otherwise, is in itself a harmless symptom. In the yellow fever, on the contrary, it is, as we have seen, except under particular circumstances, and when it shows itself at an advanced stage of the attack, the harbinger of formidable danger, and, at an early period, the almost certain sign of a fatal issue.”

Dr. La Roche, while he admits that the doctrine of the sanguineous origin and nature of the yellow discoloration of the eyes and skin in the yellow fever possesses great advantages over the opinion which would ascribe the discoloration in question to a bilious suffusion, or to any particular condition of the hepatic functions, doubts whether we can adopt it in all cases to the exclusion of the other.

He believes that those instances in which the skin is of a uniform saffron or lemon colour, and especially those in which the suffusion assumes the character of a critical change, or takes place at any very late period of the disease, when the powers of the system are already recruiting, or during convalescence, may, without risk, be referred to a bilious origin. The discovery of the constituents of the bile in the urine, in cases of yellow fever attended with jaundice, lends additional support to the opinion. Still more to the purpose are the facts adduced, in which the perspiration imparts a yellow or saffron colour to the substances impregnated with it. We have seen also that the presence of the constituents of the bile is discovered in the blood of yellow fever. It is found in the serum obtained from under the cuticle in blistered surfaces, as Dr. La Roche has himself observed, on several occasions, in the presence of some medical friends. The same is found in the yellow fluid of vesicles occurring in the course of the disease. Bile is also found in the serum of blood drawn from the veins or arteries, in the pellicle formed over the crassamentum, as well as in the fluid taken from the heart or large vessels after death.

“The analogy,” remarks Dr. La Roche, “in this respect, between the yellow fever and ordinary jaundice is striking; and if, in the production of the latter, the bile is admitted to play an important part, it is difficult to refuse our assent to the opinion which ascribes as important an agency to the same substance in the causation of the yellow discoloration of yellow fever. There is no absorption or regurgitation of bile already formed, for, in by far the largest number of cases, none is secreted. There is merely retention in the blood of those elementary or constituent parts of that fluid which ought to have been eliminated from the system, but of which, from its peculiar morbid condition, the liver has not been able to rid the system. This view reconciles, to a certain

extent, both doctrines; for though the jaundice is doubtless due not to bile, properly so called, but is a peculiar condition of the blood, the latter owes the power of discoloring the tissues to biliary elements; and if that discoloration differs in some respects from that of ordinary jaundice, the difference, in all probability, depends, as long ago surmised by Warren, on the attenuated, dark, and diseased condition of the blood, and on the sluggish manner in which it circulates in the capillaries. Indeed, there can be no doubt that the bronze and mahogany hue, the mottled and other unusual appearance of the skin described in the preceding pages, are due to a sanguine exhalation, or to a mixture of tints—of the yellowness arising from the jaundice proper, on the one hand, and, on the other, from the dark or violet red of the skin produced by the altered and almost stagnated blood contained in the capillaries of the skin. The correctness of this opinion is shown by the fact that, if a part so coloured is pressed with the finger, the bronzed or mahogany hue disappears, more or less completely, but the yellow colour remains unchanged, and as the blood returns in the capillaries, the dark colour in question returns with it, and the skin resumes the peculiar appearance it had before.”

It will be impossible for us to follow Dr. La Roche in his very minute and extended examination of the nature and source of the black vomit in yellow fever. In two former numbers of this journal the subject is treated of by him in a paper which forms the basis of the chapters devoted to it in the chapters before us. From that paper our readers have been enabled to judge of the author's manner of discussing the pathology of this very common and serious symptom of the latter stage of the more severe forms of yellow fever, but one by no means confined to that disease, much less pathognomonic, as it has been regarded by some, and of forming a correct idea of the views advocated by him in relation to its character.

With the most accurate and authoritative of the more recent observers, Dr. La Roche refers the matter of black vomit to an effusion of blood from the capillaries of the inner surface of the stomach; the blood being altered by the action upon it of an acid, the morbid product of the diseased stomach. The difference it presents in its physical appearance depending on the manner in which the blood is effused into the stomach—whether drop by drop, or in a stream—and on the degree of acidity of the gastric secretion, or the quantity of serous fluid it meets with in that organ.

The facts connected with the pathological anatomy of yellow fever have been carefully collected and compared by our author. From these we learn that the cerebro-spinal organs, the brain and spinal cord—and the ganglionic system, are often free from any appreciable lesions; while the morbid changes occasionally discovered in these parts are not to be considered as peculiar to, or characteristic of yellow fever.

The same remarks may be repeated in relation to the respiratory organs. Neither does the heart present invariably any pathological changes which can be set down as peculiar to the disease.

“Of all the organs of the body,” remarks Dr. La Roche, “the stomach may justly be regarded as the one most generally and seriously implicated in the yellow fever, and in which the marks of disease are most frequently discovered after death. Externally, it is sometimes found of a yellow colour, but in other cases—and, indeed, generally—it retains its normal appearance. On opening the organ, it is almost uniformly found to contain a greater or lesser quantity of matter similar to that thrown up during the latter stage of the disease, and known under the name of black vomit. In some cases, the contents of the stomach consist of blood more or less pure, with or without coagula, and generally combined with a portion of glairy matter, and substances swallowed a short time before death. In some instances, the mucous coat is smeared over with a dark, adhesive, jelly-like substance, containing portions of blood. Under this

substance, and sometimes when it does not exist, we find a layer of grayish matter, not unlike a mixture of linseed meal. In some cases, again, the lining membrane, when cleansed from these various coatings, is found to present its normal appearance, and to be perfectly free from any appreciable morbid lesion. Indeed, in not a few instances, it is even whiter than in the normal state.

"The integrity of the mucous membrane in such cases—the absence of capillary congestion or inflammation—has been ascribed to the effusion of the matter of black vomit or of blood, a quantity of which is nearly always found in the viscus. But this opinion, which would seem to receive support from the known origin of that matter, and from the fact that the quantity is often less in cases where the membrane is found most diseased, is supposed to be negatived by the fact that, in some cases, the quantity is just as large when the membrane is diseased as when healthy, and that in some, in which the stomach was filled with black fluid, the vessels of the mucous membrane were found literally gorged with blood. Such instances, however, are, after all, seldom encountered, and the simultaneous existence of capillary congestion in the mucous membrane, and sanguineous effusion in the cavity of the stomach, is no proof that the latter does not usually relieve the former, but merely shows that, in cases in which it occurs, the hemorrhagic tendency has been so strong that the blood effused from the engorged capillaries has been rapidly replaced.

"The number of cases in which the membrane is exempt from appreciable morbid change is generally limited, and varies in different places and seasons. They are encountered more particularly in malignant cases in which arterial reaction is but feeble, or in rapid cases of other varieties.

"Much more frequently, the mucous membrane is found more or less diseased; and presenting changes, which, though regarded by some as the product, in most, if not all, cases of capillary congestion, of mere staining, or, indeed, of any other morbid process than inflammation, are admitted by a large number of pathologists as indicating—not all, but some—the prior existence of the latter diseased condition. Hence, by most writers on the yellow fever, the stomach is stated, in positive terms, to present after death decided marks of inflammation; or alterations are described which are known to be the effects of that process.

"That some of these changes are of a nature to justify the opinion of those who deny the truly inflammatory character of the local disease in all cases, there can be no doubt. Nor is it improbable that many of those who have assumed that the changes found are due exclusively to inflammation, have predicated that opinion on appearances which more ample experience would have shown to be the result of a different morbid process. But it is not less true that, in a large number of individuals who have died of yellow fever, the lining membrane of the stomach bears the most unequivocal marks of inflammation. On both these points, the following enumeration of the morbid changes found in that organ will leave no doubt:—

"A. In some cases, the stomach is found contracted; in others, it is distended.

"B. The longitudinal rugæ are enlarged. The surface has often a vermicular appearance, being corrugated and thrown into numerous folds.

"C. The capillary vessels are, to a greater or less extent, injected with blood. Sometimes these vessels are literally gorged; to such degree, indeed, that portions of the membrane, 'cut and dried, have formed very perfect preparations, exhibiting the ramifications of the vessels into their minutest divisions in a very beautiful manner.' (*S. Jackson*, p. 21.)

"D. The redness is, in some cases, of a very intense and dark hue, and has been compared to the redness produced in that organ by the ingestion of acrid poisons. In other instances, the colour is lighter, assuming a rose, claret-brown, ruddy, pale ash, greenish, or yellowish hue. In other instances, again, the colour is leaden, livid, or even nearly black.

"E. This discoloration is either uniform in appearance or in the form of arborizations; extending over a large portion or the whole of the membrane, or, as oftener takes place, it is limited to the cardiac or pyloric orifices, and to the larger curvature, and presents itself in patches of greater or less size, and more or less numerous. In addition, whatever may be the tint of the mem-

brane, we discover streaks or spots of a purple colour, spreading in various directions over both the altered and healthy parts. These spots assume frequently the appearance and characteristics of true ecchymosis—varying in size and number. At other times, with or without those large patches, are found numerous small, dark red, or violet round spots superadded to the rose hue of the surface, with which they form a distinct contrast. They resemble flea bites, and have the same appearance as the petechial spots noticed on the external surface of the body.

“F. The membrane is the seat, besides, of abrasions, or small depressions, or pits, like holes or furrows, which convey the idea of a portion of the tissue having been removed.

“G. It very frequently is found mammillated, even to a remarkable degree. Louis found it so in two-thirds of his cases.

“H. The membrane in such cases, or even when no mammillated elevations exist, is found more or less considerably thickened or opaque.

“I. In some of the latter cases, or even when not thickened, the membrane is softened, and easily detached, especially about the great *cul-de-sac*. In a few cases, it is the seat of ulceration, or of gangrenous changes.

“K. In some instances, more particularly when the disease was of a malignant or congestive character, an effusion is discovered under the tissue in question.”

The appearances detected in the œsophagus and small intestines do not differ materially from those exhibited in the stomach. They are generally, however, less marked, and perhaps more frequently absent, in the small intestines than in the stomach.

A change in the appearance of the liver, in subjects who had died of yellow fever, has been noticed from an early period. It is described as being of a light yellow, nankeen, fresh butter, coffee and milk, gum yellow, buff, gamboge, light orange, or pistachio colour. Louis, from the constant occurrence of this appearance of the liver in the cases examined by him during the epidemic of Gibraltar in 1828, was led to regard it as peculiar to the yellow fever. The more recent dissections made in this city and country have fully confirmed the statements of Louis and others as to the very frequent occurrence of this discoloration. In some cases, it occupies the whole surface and pervades the entire parenchyma of the organ; while in others, it extends only partially over both, giving to the organ a marbled appearance—presenting throughout patches or regular strata, and alternating with others of a dark green colour. It is limited occasionally to a single lobe, usually the left. The pale yellow liver is more common and marked in women and children; in the latter assuming, after a short maceration, the paleness of box-wood. It differs, as regards frequency, in different seasons, and even at different periods of the same season.

“Frequently, however,” Dr. La Roche observes, “as this peculiar coloration of the liver has been observed, it is far from being universally so; cases occurring in which the organ is found of a different hue—dark yellow, brown, red, purple, bluish, slate, chocolate, or livid. It has been described as of brick colour, and compared to rhubarb or to Peruvian bark. (*Arejula*.) In other cases, again, it retains its natural appearance externally and internally, and is otherwise healthy. The parenchyma, when divided, is often found hard, dry, tough, and sometimes dry and brittle, and more or less devoid of blood; while, in some cases, the viscus is more or less gorged with blood, and softer in texture than natural. In some cases, the biliary pores contain bile, but more frequently there is no indication of biliary secretion.”

“Though often more or less enlarged, and at other times shrunken, the liver seldom exhibits traces of inflammation, and, if these are discovered, they must be viewed simply as the effect of complications, while the alteration in, or sup-

pression of, the secretory function of the organ, may justly be referred to some cause different from that morbid state."

Dr. Budd throws out the suggestion that the pale colour of the liver in yellow fever may depend upon the presence of fat, without, however, its being present in such quantity as to increase the size of the organ, and cause the striking appearance of the extreme fatty liver of phthisis.

"A few facts," remarks Dr. La Roche, "observed antecedently to the publication of Dr. Budd's work, but evidently unknown to him, and many collected subsequently in this country by pathological inquirers, who at the time had not seen that work, go far to confirm the views of its author, so far, at least, as they relate to the yellow fever."

In the liver of a patient who died of yellow fever in New York, Dr. Alonzo Clark discovered, upon a microscopical examination, "a fatty state of all the secretory epithelial cells, and an abundance of free fat globules," which induced him to query whether the change so constantly observed in the livers of those dying of yellow fever is not an acute fatty degeneration.

"Scarcely," says Dr. La Roche, "a few months had elapsed, when we had in this city the fullest opportunity of confirming the discovery of our very distinguished and accomplished countryman; for, in all the examinations made during the epidemic of 1853, the peculiar change in the liver to which he called attention was detected. These examinations amounted to fourteen at the Pennsylvania Hospital. Several more were made at the St. Joseph's and other hospitals. In the summer of the next year (1854), the livers of those who died with the disease at the first of these establishments—six in number—were in like manner examined under the microscope, and *in every instance, without a single exception*, in both seasons, the results were the same. The number of livers examined was too large, the results obtained were too constantly the same, to allow these to be ascribed to mere coincidence. The changes observed must appertain to the disease, and, as they are not noticed in the livers of individuals who have died of other fevers, they must be placed among the anatomical characters of the yellow fever."

After having examined with care the pathological anatomy of yellow fever, Dr. La Roche enters upon the inquiry as to the connection which exists between the changes discovered after death and the symptoms of the disease, with the view of determining, if possible, any that appertain in a special manner to it, and serve to distinguish it from other febrile diseases. In other words, to discover, if we can, whether dissection reveals any anatomical characters especially belonging to the yellow fever, and imparting to it a distinct nosological position.

In regard to the peculiar discoloration of the liver, and the fatty condition of the organ to which that discoloration is due, Dr. La Roche concludes, that all we can say in the present state of our knowledge is, that they are more frequently noticed in the yellow fever than in other diseases, and approach nearer to what may be regarded as its true anatomical characters, than any morbid change that has been noticed.

While he would be reluctant to regard any of the changes that have been detected in yellow fever, when considered singly, as characterizing the disease, he thinks that we can have no hesitation in admitting that when several of them—a dark mahogany colour of the skin, petechial condition of the gastric-mucous membrane; black matter in the cavity of the stomach and bowels; a yellow discoloration and fatty degeneration of the liver; an albuminous state of the urine, &c.—are found associated together in the same case, there will be no difficulty in forming an opinion as to the nature of the disease.

"In the yellow fever," he remarks, "more perhaps than in any other disease, it is difficult, if not impossible, to connect as cause and effect the symptoms observed during life with evidences of textural changes discovered after death. That the morbid conditions of the parts, giving rise to the changes in question, must and do manifest themselves during the progress of the disease, by external and recognizable signs, is not to be denied; but it is not the less true that the main and characteristic symptoms cannot always be attributed to any particular discoverable pathological changes in the organs in which they are seated; that they may exist, whether or not those changes are discovered after death, while it may even happen that these are found when the symptoms thought to be due to them had been either absent or of trifling import."

The chapter which follows on the subject of critical days and critical efforts in yellow fever, comprises a very fair digest of all the facts which bear upon the subject. The question in regard to the type of yellow fever comes next under discussion.

With all the best and most authoritative writers on yellow fever, Dr. La Roche describes the disease as one of a single regular paroxysm, of about seventy-two hours duration, more or less, succeeded by a period of calm, or complete cessation of fever. Nothing bearing any analogy to a similar paroxysm recurs after the cessation of the period of calm in question.

"This remission, or rather sudden cessation of fever after one long paroxysm, the only one which occurs, may be regarded," says Dr. La Roche, "in the light of a well-ascertained fact. It is very seldom, if ever, succeeded by symptoms of vascular reaction, but by an apyretic condition, during which, in severe and aggravated cases, the more malignant phenomena present themselves. It constitutes a marked and distinctive characteristic of yellow fever, and is pointed out as such by every writer, and in every clime. As already stated, it seldom fails to present itself; the exceptions to the rule are rare, and occur in mild cases in which the symptoms of the first stage alone, more or less prolonged, are noticed, or in the violent and congestive forms of the disease, when those of the last stage are not preceded by a period of reaction; while, on the other hand, it is never observed in other forms of febrile complaints—the oriental plague, perhaps, excepted. So true, indeed, is this, that the occurrence in question is of itself almost sufficient to enable us to diagnose the case, while its absence may, unless under the circumstances mentioned, justify a doubt as to the genuineness of the disease, or lead us either to view it as the offspring of a different poison, or to suspect the existence of a complication."

From all the observations recorded in relation to the yellow fever of this and other regions, Dr. La Roche concludes that, in many of the milder cases, particularly when the febrile stage extends beyond the usual time, the disease presents sometimes a slight paroxysmal type, and is subjected to diurnal exacerbations and ameliorations; but that, as a general rule, in well-marked, pure, and malignant cases, the type during that stage is continued, and remissions, if they present themselves at all, are obscure, imperfect, and irregular, consisting, in fact, more in diurnal diminutions of the symptoms, or abatements in the morbid feelings, than in remissions, in the true medical acceptance of the term. Such has been, Dr. La Roche remarks, the case in all the instances of uncomplicated yellow fever which have fallen under his observation.

A most important chapter is given on the complications of yellow fever. The facts and reasoning adduced by the author in reference to this subject are of deep interest. The various modifications which the disease may experience from the influence of morbid agencies independent of those to which it owes its existence, require to be well understood and kept constantly in mind. By not paying due attention to these modifications, the result of

causes that may operate on the system in conjunction with, or antecedently to, that by which the yellow fever is produced, errors of diagnosis are very liable to occur. From this cause much confusion in the history of the disease has resulted, while it has led to more than one serious error in regard to its true pathological character and type.

We cannot follow the author in his consideration of the duration of the disease, its convalescence, and the question as to the occurrence of relapses.

The chapter on prognosis is full, and, so far as the subject will admit, clear and explicit. The various circumstances to be taken into consideration, in our attempt to form a probable judgment as to the favourable or unfavourable termination of the disease, are examined in detail, and their relative value carefully examined.

We pass over the chapter on the mortality of yellow fever, which admits of no satisfactory analysis. The subject is one of deep importance, and is treated by the author with the same ability which he has devoted to the consideration of every other question connected with the disease. He has collected with his usual industry all the available facts and statistics calculated to lead to definite results. From all the data he has been enabled to collect, approximate results, however, can alone be derived in relation to the comparative malignancy of the disease, and the destruction of life produced by it, at the several periods of its prevalence, and at the different points at which it has prevailed in this country, as well as in respect to the difference in this point of view between the disease as it has shown, and continues to show itself in the United States and in foreign lands.

In regard to the true pathology of yellow fever, after passing in review the several theories that have been advanced in elucidation of this important and confessedly intricate subject, and adducing the reasons which induce him to reject these theories, as inconsistent with the known facts in relation to the character, phenomena, and usual course of the disease, Dr. La Roche, from a consideration of the entire phenomena of yellow fever, of the nature and mode of introduction of the agent to which it owes its origin, of the condition of both solids and fluids, and the result of post-mortem examinations, arrives at the following conclusion:—

“The cause of this complaint, however it may have originated, is a *special poison*, which having penetrated the system and entered the bloodvessels in a manner well known, is carried everywhere through the instrumentality of the blood. It produces in that fluid changes, to which attention was called in a former chapter, and others of a more intimate kind, which escape the cognizance of our senses; and by means of the polluted blood, occasions important morbid changes in some of the abdominal organs. It produces, besides, a morbid impression on the nervous centres, and these, as well as the former, in their turn, induce, by virtue of the numerous sympathies which bind together the various components of the system, diseased modifications in various organs and tissues, and thereby give rise to phenomena of a more or less peculiar kind, or essential to the manifestation of the fever, and varying somewhat in number and violence in different individuals and under different circumstances. The morbid actions thus produced may be combined with opposite conditions of the vital forces, and may or may not be accompanied with inflammation of the parts primarily or secondarily affected; these and the other differences alluded to depending on age, habits of body, and numerous other circumstances connected with the individual; on the degree of combination and violence of the efficient cause; on the peculiar character of the epidemic constitution of the atmosphere; on the nature of the localities, and numerous other modifying agencies. Hence, in some cases—under some circumstances, in the majority, if not all—the morbid action of the poison is such as to destroy

at once the powers of life, or, in less rapidly fatal instances, to greatly diminish them in all the parts to which it penetrates, and by that means, as well as by the diminution of fibrin or other changes occasioned in the blood, to impede or annul the force of reaction inherent in the system, impairing or destroying the vitality of the tissues and organs, checking the secretions, modifying and vitiating the power of nutrition, lowering the tonic force and vital energy of the bloodvessels, promoting the formation of capillary and venous congestions, facilitating the escape of blood from or in the substance of the tissues, &c.

"The disease, in such cases, presents itself clothed in the garb appertaining to the asthenic and congestive forms described in a former chapter, in which there is, strictly speaking, no fever, but in which the identity of the complaint is made out by peculiar symptoms which present themselves from the onset to the close of the attack. In other instances, which in certain seasons and places constitute the majority, the impression of the poison is soon followed by reaction, which becomes more or less marked—continues for a longer or shorter time with the usual symptoms of such a pathological condition—vascular excitement and often florid blood—and is succeeded either by recovery, or, after from two to four days, by a state of collapse and disorganization akin to that which in the other forms occurred from the outset.

"The yellow fever, in such cases, assumes the inflammatory character, and, like the asthenic or congestive form, is susceptible of being classed into several varieties. In many instances, especially of the inflammatory, though sometimes of the asthenic form, the disease shows itself in combination with local inflammation of various organs—particularly of the gastro-intestinal mucous tissue. In other instances, on some occasions in the larger number, those local inflammations are replaced by a greater or less degree of congestion of the capillaries, of the above and other tissues, and of the larger vessels. In others, again, both conditions are combined, and in all severe and fatal cases, the capillaries, at an advanced period, lose their tone, and become the seat of passive hemorrhages. In a word, the modifying agencies to which I have alluded, may so interfere with the action of the poison as to give to the fever it produces, a character altogether different from that which it usually presents, just as a peculiar habit or a peculiar idiosyncrasy will sometimes enable the individual to resist the reception, or at least to throw off or eliminate the poison when received, and escape its deleterious action altogether."

The *diagnosis* of yellow fever, the circumstances which distinguish it from the plague, the cholera, the causer or inflammatory endemic, the scurvy, the typhoid fever, the sweating miliary fever, the relapsing fever, and the bilious remittent fever, constitute the subject of the two concluding chapters of the first volume of the treatise. The circumstances which distinguish the yellow fever from the several diseases enumerated, are considered at great length, and the fact of the peculiar or specific character of the former very clearly demonstrated.

The second volume commences with an investigation of the etiology of the yellow fever. The morbid influences productive of the disease are considered under four general heads: 1. Causes depending on the individual; 2. Hygienic causes; 3. Contagion; 4. Infection.

The first subject discussed is that of acclimatization. The fact that the natives of, or those long resident in those countries of which yellow fever is a prominent endemic, enjoy a remarkable, almost certain, exemption from an attack of the disease, is very fully established.

This acclimatization, so far as relates to the fever in question, consists, according to Dr. La Roche, in great measure, in the change produced in the system by long continuance of high atmospheric temperature, aided by peculiar hygrometrical and other meteorological conditions—barometrical pressure, electrical currents, &c., and, in no small measure, by the permanent or frequently renewed impression of malarial impurities floating in the air, and

constituting the usual sources of febrile infection. In tropical regions, where the temperature varies but little in the different seasons of the year, and in such of our southern cities where the winters are mild and of short duration, and followed by long and hot summers, this permanence exists to the degree required. Hence, individuals accustomed by time or otherwise to the climatic conditions they there meet with, become secure against the disease. But in Philadelphia and in temperate regions generally, the same permanence does not obtain. While the summer climate of this and of neighbouring cities may be assimilated to that of tropical regions, and the heat thus experienced, and the morbid conditions of atmosphere created during the summer and autumnal months are amply sufficient to produce the yellow fever; yet this torrid temperature is of comparatively short duration, and is succeeded by long and severe winters, and by a new train of meteorological phenomena, well calculated to destroy the telluric or other causes of the disease when they have existed, to revolutionize or renovate the system, and to prevent the establishment of those organic modifications which require time for their development, but which, when effected, render the individual who undergoes them insusceptible to an attack.

"To this must be added, that the efficient causes of the fever, whatever they may be, are not of annual occurrence, and the system cannot become accustomed to their agency. Natives and residents of temperate regions then find themselves, during such visitation of the disease, in the same position as the creolized or acclimatized inhabitants of warm regions, who lose the protection they enjoyed by a long absence in a colder and more salubrious climate. The low temperature and healthier atmosphere of a long winter destroy or prevent any change that might have been induced by the high thermometrical range of preceding seasons; and on the reappearance of the disease, individuals so situated are as prone to an attack as if they had never been exposed to its causes."

"As to the time required to obtain protection through the process of acclimatization," Dr. La Roche remarks, that "no positive information has yet been obtained. We have seen that, in the French West Indies, a residence of four years is regarded as sufficient to secure the troops from the fever. According to Dr. Doughty, however, a residence of that duration does not secure the British soldiers at the season when, and in situations where, it most prevails, if, previous thereto, the constitution had not been exposed to the operation of the cause; in other words, the individual must, in order to be well secured, have become acclimatized to the place infected. Dr. Barton is of opinion that there is no period of residence, in New Orleans, that will *certainly* exempt one from the fever, and adds, in confirmation of this, that the cemetery records show the fact that people die of it after having been in that city five, ten, or more years. We have seen that in Mobile, a residence of fifteen or twenty years has not been sufficient to protect individuals against the fatal impression of the cause. But these, as well as the occurrence of the disease among resident natives, are exceptions to a rule, and I feel disposed to believe, from all the information I have been able to collect on the subject, that, in the West Indies and our southern yellow fever cities, while such exceptions are sometimes seen in ordinary sickly seasons, and comparatively often in periods of violent epidemics, immunity from residence, with or without the fever, is equal to that afforded by congenital or native creolism, that it is usually acquired in less than ten years—often in five—not unfrequently sooner, and that it is much more perfect when the individual has resided in the place during the prevalence of an epidemic, especially when the latter has been of a severe and malignant character, than when he has done so during a succession of healthy seasons."

In regard to the liability of one who has sustained an attack of the yellow fever, to a second occurrence of the disease, Dr. La Roche remarks that when

he calls to mind the facts that have fallen within the sphere of his personal observation, and weighs the testimony adduced in support of both sides of the question, in the West Indies and Europe, as well as in the southern section of our country, it is impossible, he thinks, to withhold the conclusion that it would be little short of absurdity to deny that, as a general rule, an attack of the yellow fever exhausts the susceptibility of the system to further attacks, or renders it less liable to be severely affected by the poison of the disease. Instances of an exceptional kind, he admits, are encountered: although, therefore, the immunity in question may not be as absolute as some of its advocates fancy it to be, he nevertheless considers the conclusion inevitable, that second attacks, in individuals who have passed through the disease, are somewhat rare, and that the immunity obtained by that means is much more secure than that derived from simple acclimatization. Second attacks constitute, therefore, exceptions to a rule, and are perhaps but little more frequently met with than repetitions of other complaints, through which the system usually passes but once. Like attacks of the fever in persons acclimatized by long residence, they occur more especially in times of severe visitations, when the epidemic influence is powerful and wide spread.

Dr. La Roche points out the fact that the persons most prone to the disease are those of a sanguine temperament—the robust, strong, and plethoric, and whose mode of living is one calculated to keep up that temperament. That females, as a general rule, are less obnoxious to the impression of the poison productive of yellow fever than individuals of the other sex; and when attacked have the disease in a milder form. That the disease affects in preference individuals of adult age—sparing, to a greater or less extent, young children, as well as persons advanced in life; that experience everywhere teaches that the disease, without completely sparing, particularly under peculiar circumstances, the individuals of African birth or origin, whether in its sporadic or epidemic forms, affects more generally and severely the white race.

The remarks of the author on the influence of the passions and emotions; the state of sleep; intemperance in respect both to food and drinks; a sudden change from a generous to an abstemious diet; immoderate evacuations; fatigue; and venereal excesses, as predisposing and exciting causes of the disease, are pertinent and instructive; equally so is his examination of the influence of trades, occupations, and conditions of life in relation to their predisposing or exciting tendency, on the one hand, or their apparent tendency on the other, to place the system in a condition best fitted to resist the action of the efficient cause of yellow fever.

The fact of exposure to cold or wet, especially when the body is heated or perspiring through the effect of exercise or other cause, as also exposure to the coolness and chilliness of night, constituting a frequent exciting cause of the disease, is very properly enforced.

Having disposed of those causes which have reference to the particular disposition or condition, natural or acquired, of persons exposed to the disease, Dr. La Roche passes to the consideration of the *circumfusa*, including the various conditions of the atmosphere, such as temperature, the electric and hygrometric states, and the nature of the aerial currents. The influence of each of these in the production of yellow fever is separately and very fully considered.

All the facts in relation to the localities of which the yellow fever is endemic, the seasons at which it prevails, and the conditions under which alone the disease occurs in temperate regions, show the necessity of a tropical tem-

perature of some continuance for its development. Heat must, therefore, be regarded as playing an important part in the production of the yellow fever, whether we view it as a necessary, exciting, or predisposing cause, or as aiding in the development of agencies of a more efficient nature. That the disease cannot be ascribed, however, to the action of a high degree of atmospheric temperature alone, is inferred from the following facts and considerations:—

“A. In some countries as hot as, and hotter than, those subject to the yellow fever—where the average tropical heat is of longer duration—the disease has never been known to occur; while in others it has only done so occasionally and to a limited extent.”

“B. The prolonged action of heat on the system is not necessary for the production of the disease, which often breaks out when the individual has been exposed but a few weeks, a few days, or even only a few hours to the atmosphere of an infected spot.”

“C. In countries subject to the yellow fever—where it prevails endemically, or at long intervals—seasons occur in which, though the temperature is as high as, or higher than, during sickly periods, the disease either does not show itself at all, or does so to a limited extent or in a mitigated form.”

“In countries subject to the yellow fever, whether endemically, or epidemically at distant intervals, some localities are more or less severely visited; while neighbouring cities, or districts and situations, even at no considerable distance, where the temperature is equally high, are exempted—these at times becoming affected while the others remain healthy.”

“In all our pestilences, the disease, as remarked—and the same holds good in most, if not all places—is circumscribed in its sphere of action. Some street, some portion of the city is infected—such sickly parts varying in different visitations—while the rest, where the temperature is equally high, remains free from the disease.”

“Were the localities thus infected hotter than others that remain healthy, the circumstance could not be adduced in proof of heat being the efficient cause of the disease, for individuals who are unaffected so long as they remain aloof from the sickly spot, and take it by visiting the latter, are not so from the greater heat they encounter, inasmuch as the risk of infection is not as great in the hottest as in the coolest part of the twenty-four hours.”

“Were the heat the principal or efficient cause of the disease, we ought to find the prevalence and severity of the latter, during a sickly season, to be proportioned to the elevation of the temperature. So far from this being the case, it is a well-known fact, that when our cities are visited epidemically with the yellow fever, the disease is not always more particularly rife during the hottest month of the season; but some time after, when the average temperature has lowered in a notable degree; and that so far from its diminishing as the weather becomes cooler, cases are often multiplied daily till the temperature reaches the freezing point, when the occurrence of new cases ceases at once.”

“Long continued and excessive heat has occasionally put a stop to the epidemical extension of febrile diseases; at any rate, they cease long before the cessation of hot weather—when, indeed, the temperature has reached its highest point.”

The leading positions thus stated are enforced by a strong array of facts. The conclusions at which Dr. La Roche arrives, from a careful review of these, is, that—for the production of yellow fever—

“Heat may be, and is, to a certain extent, requisite to promote the formation of other agencies; it is doubtless essential to further the evolution of the poison which gives rise to the disease, as everything connected with the appearance of the latter proves—its production in hot weather—its absence in cold, and its appearance on the accession of frost. Heat may act, besides, as an exciting cause; but alone, it cannot occasion the peculiar form of fever under consideration. It requires materials to act upon, and from which, aided by

other influences, it may extract an efficient cause. That high and long continued heat may, and does often, by its action on individuals unaccustomed to its effects, produce fever, is doubtless true, and perfectly well known to all physicians acquainted with the complaints of hot climates; but the disease thus produced is different from true malarial fevers, and especially from genuine yellow fever. More frequently it gives rise to other groups of morbid phenomena more or less distressing, violent and dangerous—cerebral inflammations, or congestions, visceral inflammations, inflammatory angiotonic fevers; but these phenomena, I repeat, are no ways analogous to those characterizing the regular and specific pyrexia under consideration.”

In regard to the influence of the intense light of tropical climates and that of our summer months as a predisposing or exciting cause of yellow fever, our author believes it to be very slight; though it is probable it may contribute to place those exposed to its influence, especially the unacclimatized, in a condition favourable to receive the impression of the efficient cause, or it may hasten or excite an attack.

Electricity, Dr. La Roche admits, may, and no doubt does, act as an exciting cause of yellow fever by its excess, and as a predisposing one, sometimes by this excess, and more frequently by its deficiency and modifications. It is possible, also, he believes, that by a derangement of the electric force, or its deficiency, and by the absorbing effects of certain winds which often prevail in epidemic times, the formation and diffusion of the remote cause of the disease may be promoted. He holds, however, that the idea of looking to electricity for the remote or efficient cause of the yellow fever is not tenable. The sphere of prevalence of the disease is often circumscribed within such narrow and well-defined boundaries, that it is difficult to perceive the propriety of regarding it as due to any modification in an agent of that general and wide-spread character necessarily possessed by the one under consideration.

Dr. La Roche admits the morbid influence of an undue increase or diminution of atmospherical pressure, as well as of those violent commotions of the barometer which sometimes occur. But he believes that these changes in the weight and density of the atmosphere can do no more than place the system in such a condition as will predispose it to the deleterious impression of some more efficient cause, especially when we find that the same condition of the barometer exists as well when the yellow fever does not, as when it does prevail.

The subject of atmospherical humidity as a cause of yellow fever is discussed at great length by our author. The facts adduced by him afford, he thinks, a convincing proof that the disease is produced under opposite hygro-metrical conditions of atmosphere; and that those who ascribe its origin and prevalence to excess of atmospherical and terrestrial humidity in all places, and under all circumstances, err as greatly as those who refer it exclusively to a dry or drougthy state of the air.

“Nevertheless,” he remarks, “while the fever breaks out and spreads in some localities and seasons during the prevalence of rain and of atmospheric humidity, we may infer from what has been stated, that it does so often under the influence of a different state of weather. The experience of this city would appear to favour this view of the subject. It will be found that the disease usually breaks out in the latter part of summer—that it prevails until arrested by the accession of frost, and that, during the greater part of this period, the weather is ordinarily dry.”

When, however, humidity of atmosphere exists in any country, or at any season, the fever may be expected to prevail more certainly and extensively than it would otherwise do.

"We discover," says Dr. La Roche, "in this fact one of the causes of the habitual prevalence of the disease in tropical climates and in portions of our Southern States, where the humidity is great, and the heat, in consequence, oppressive. In the Middle States, and the South of Europe, where the humidity of the atmosphere is much less considerable, and exists in excess only at particular seasons, and where the heat is less frequently and continuously oppressive than in tropical regions, the fever is of rarer occurrence, and requires for its production and epidemic diffusion a combination of circumstances seldom encountered together—as well, perhaps, as a primary cause of greater intensity than that which usually occasions the disease in climates where the predisposing influence in question paves the way to its spread."

While Dr. La Roche rejects the idea that either heat or moisture, when acting separately, can be productive of yellow or kindred fevers, he considers equally objectionable the belief that the disease arises from the combined influence of these two agencies, either unassisted by another cause of a more efficient kind and peculiar character, or with the aid of some agent calculated only to render the system more prone to the impression of the other. Neither can he admit the propriety of referring the efficient cause of yellow and kindred fevers to vicissitudes of temperature—the succession of cool or cold nights to hot days, nor to the sudden exposure of the body, at any period of the twenty-four hours, to a low degree of temperature after it has been placed, for a greater or less extent of time, under the influence of a high degree—however efficient such vicissitudes may be as exciting causes simply.

An examination of the question as to the agency exercised by particular winds in the production of yellow fever, upon which so much has been written, leads our author to the conclusion that, inasmuch as facts show a diversity of result to arise from the same wind, and a sameness of effect from currents of different character, no particular wind can be said, with any show of reason, to constitute, by itself, the necessary and efficient cause of the disease; and that, whenever any of them exercise any agency as regards the origin or diffusion of the fever, it derives that power not from the fact of its coming from any particular quarter, but from the temperature and hygrometrical conditions of the moving column of air, and more especially, perhaps, from the injurious effluvia it raises from the localities and surfaces over which it passes, and which are carried along with it.

"The fact," remarks Dr. La Roche, "is proverbial, that the yellow fever usually breaks out and is most rife in places noted for want of due ventilation—narrow, close streets and alleys. Here, whether in hot climates or in our latitudes, during the tropical heats of summer, the air becomes close, oppressive, and hot to a degree beyond what might be expected from the height of the mercury in the thermometer. This condition of atmosphere occasions in many a wakefulness, and in others of particular constitutions, a variety of disagreeable nervous sensations; and from this it may be readily conceived to render the system impressionable to the action of the efficient cause of the fever, while, at the same time, it tends greatly to promote the evolution of the latter, and concentrate and aggravate its effects." But, however frequent may be the stagnation of air in question, and however injurious its effects, it cannot, Dr. La Roche remarks, be held as essential to the production of the disease, seeing that the latter often breaks out and prevails extensively under circumstances of an opposite character, and where the ventilation is no less free and perfect than in healthy seasons, while, at other times, this stagnation, attended with its usual effects on the system, presents itself frequently during seasons noted for their salubrious character, and unmarked by a single case of fever.

Previously to entering upon a consideration of the nature and source of the immediate and efficient cause of the yellow fever, the question of the contagious or non-contagious character of the disease is very fully and thoroughly discussed. In which question, as Dr. La Roche very truly remarks, that of the origin and propagation of the fever, here and elsewhere, resolves itself.

“For if the latter is imported, *per se*, through the instrumentality of individuals labouring under, or convalescing from it, it must be endowed with contagious properties. If it be so endowed, it is not likely to arise from domestic causes, diseases of this kind being seldom if ever contagious, and must, therefore, be ascribed to importation from places where it more usually prevails. In that case, all epidemic scourges, and even sporadic cases, may naturally be referred to a foreign source; and we can find no difficulty in ascribing the diffusion of the fever, when so introduced, to the communication of the well with the sick, or with substances impregnated with the poison. If, on the other hand, the fever is not endowed with the power of transmissibility alluded to, it can scarcely be presumed to have been imported in the way contended for by the contagionists. It must hence be referred to the operation of domestic causes, unless it can be shown that this, or any other fever, can either lose or acquire that power by a change of locality and the agency of external circumstances, and its propagation must be admitted to take place independently of any influence on the part of the sick.”

To this most important point in the etiology of yellow fever, Dr. La Roche has given all the attention that its importance demands. Every fact and consideration calculated to throw light upon it is carefully scrutinized, and its true bearing and value in the settlement of the question cautiously and candidly tested. No argument of any weight adduced on either side is passed unnoticed. The chapters devoted to its discussion occupy nearly four hundred pages of the second volume of the treatise. The facts and arguments that have been advanced in proof of the contagious character of the yellow fever are first presented at full length. Giving to these all the weight they deserve, Dr. La Roche nevertheless rejects, *in toto*, the doctrine of the contagiousness of the disease; his arguments in favour of such rejection are arranged under the following heads. To present anything like a satisfactory enumeration of the facts and reasoning of the author in reference to each of these would extend the present notice to a most unreasonable length. All we can do is to enumerate his series of arguments, referring to the work itself for their elucidation.

The proofs of the non-contagiousness of yellow fever adduced by Dr. La Roche, are: 1. The appearance of the disease at determinate periods of the year. 2. The occurrence of the fever under the influence of certain meteorological states, and usually associated with the prevalence and increased prevalence of malarial fevers, which is not the case with contagious diseases. 3. When the yellow fever is epidemic, it often absorbs or supersedes other diseases, which contagious diseases do not. 4. It is a disease of hot climates and hot weather only, and is arrested by cold. 5. It is influenced in its prevalence and severity by atmospheric vicissitudes. 6. The malignancy of the disease is increased by continuance of residence in the infected localities. 7. The epidemic influence productive of the disease is felt by those who avoid exposure to the sick. 8. Its outbreak is often preceded, accompanied, or succeeded by certain phenomena in the vegetable or animal kingdom. 9. The system becomes inured to the effect of the efficient cause of the disease. 10. Yellow fever is local in its habitation—the area of the sickly locality being sometimes very circumscribed—and, beyond this infected locality, the disease is not communicated. 11. The fever is not communicated in the

puer air of the country. 12. Removal of the population from infected districts puts a stop to the further increase of the disease—excepting in the cases of persons visiting such districts, after all the sick have been removed, who are liable to be attacked. 13. The fever is more prevalent, and of worse character in some parts than in others of an infected place. 14. It originates in low, and in or near impure localities. 15. The agency of upturning of earth, and the influence of made ground in the production of yellow fever. 16. The connection of inundations, and of extensive fires, with its occurrence. 17. Yellow fever does not always occur in seaports. 18. Its local origin is proved by its occurrence on board of ships. 19. The beneficial effects of expurgation is, also, a proof of the local origin of the disease. 20. It is not communicated to neighbouring towns, cities, or plantations. 21. It is not communicated in hospitals, barracks, &c. 22. It is not conveyed by individuals or otherwise to ships in the vicinity of infected localities. 23. It is not communicated by fomites. 24. It is not communicated by handling the sick or the dead, and by dissections of the latter. 25. Seclusion affords no safety from attacks of the disease. 26. Quarantine is ineffectual in preventing the outbreak of the disease in sickly places and seasons. 27. Altitudinal and geographical ranges are different in yellow fever and contagious diseases. 28. The effects of night air as an exciting cause of the fever. 29. The desultory course of the disease. 30. The proteiform character of the disease. 31. The classes, races, sexes, and ages affected by it.

In the course of the foregoing discussion, Dr. La Roche shows that the fact of the yellow fever not appearing annually is no proof of its contagious character; that its prevalence in tropical regions coinciding with its occurrence in this country is no proof of contagion and importation; that exemption of other parts from the disease when it prevails in some places, is no proof of its exotic origin and contagious character; that the local origin of the disease is proved by the occurrence of sporadic cases; and that exemption from a second attack of yellow fever is no proof of its contagious character.

The hypothesis of a contingent contagion in yellow fever—that, on the one hand, the disease requires for the manifestation of its contagious properties the influence of an impure atmosphere, or, on the other, that a contagious property not necessarily appertaining to the disease, is generated by foul air, is very thoroughly examined, and, as it appears to us, entirely refuted.

Another doctrine in reference to the transmissibility of yellow fever is that the poison of yellow fever—which some regard as a miasm produced by materials having no connection with the living body—and others as a contagious virus evolved from the sick—is carried from place to place, in ships, merchandise, clothes, about the persons of travellers, &c., and, as the contagionists aver, in the system of the sick; and, when so imported, is multiplied and extended epidemically by assimilating to its own nature something it meets with there. In other words, the peculiar poisonous effluvium, which, if imported into a perfectly clean locality, would occasion no evil effect; acts very differently when brought during hot weather into a place replete with materials from which issue morbid exhalations; for it then plays on those materials the part of a ferment, and through their agency is enabled to reproduce itself, or out of them to give origin to a substance of the same nature, and endowed with identical properties, as yeast is generated during the vinous fermentation which yeast has set in motion.

Dr. La Roche shows that this hypothesis is unsustained by a single well accredited fact or plausible analogy—that the property in question is not an attribute of any known variety of non-contagious febrile poisons.

Dr. La Roche refers the production of yellow fever to infection, or that morbid agent or poison which results from the decomposition of dead animal and vegetable substances, or other putrescent materials, if such exist, and through means of which a morbid state is induced in the system of individuals exposed to its action; and hence places the disease in the same category with endemic remittent and intermittent fevers. This doctrine of infection, as applied to yellow fever, is confirmed, he believes, by all the facts recorded in the work before us.

“But, while entertaining these views respecting the origin of the yellow fever from local sources of infection, he does not wish to be understood as meaning that the poison producing the disease is precisely similar to that giving rise to ordinary remittent and intermittent fevers, and that it differs from it only in regard to degree of intensity. In a preceding chapter, reasons were assigned in support of the opinion that the yellow fever, though belonging to the same class or family of diseases as the other febrile complaints mentioned, nevertheless differs from them to a sufficient degree in regard to phenomena as to justify us in regarding it as constituting a separate and distinct complaint. If such be the case, the effluvia giving rise to it may reasonably be viewed as constituted of different elements produced by the decomposition of different materials—or, perhaps, of different proportions of the same elements proceeding from the decomposition of similar materials, but under the influence of peculiar external agencies.”

“Be this, however, as it may, certain it is, that the yellow fever, though of miasmatic origin, is not the offspring of the effluvia of ordinary marshes, or issuing from the margin of streams, ponds, lakes, open flat countries or savannas, or of swampy or humid soil generally, to some or all of which, remittents and intermittents are usually traced. For, in many places where such localities abound, yellow fever is unknown; while, on the contrary, the latter prevails where such characters of localities are absent. In a word, it will be found that the several forms of fever do not necessarily manifest themselves in the same situations. Thus, in this country, while the yellow fever is a disease of cities and towns—generally of only a portion of these—common remittents, and especially intermittents, spread on the skirts of these, and more particularly in the open country, and seldom, if ever, penetrate, to any extent at least, within thickly built and populous habitations; and when they do so, we very generally discover that they spare those peculiar locations where the yellow fever is rife. This is true, whether we turn to the southern, southwestern, or middle sections of the country.”

The same general fact, also, obtains in every country where the yellow fever prevails as an endemic, or as an epidemic at long intervals.

“The malarial effluvia giving rise to the yellow fever may be inferred to differ from those occasioning remittent and intermittent fevers, not only from the difference existing in the phenomena of those diseases, and the character of the localities they visit, but from the fact that they are apparently governed, on many points, by different laws. It is admitted by Dr. Chervin, a strong and uncompromising advocate of the unity of all miasmatic fevers, that although the effluvia occasioning the yellow fever are, like those giving rise to remittents and intermittents, wafted by the wind, still, as regards the former, the deleterious effects they produce do not extend as far as is the case with the latter. But it may be doubted whether the extension is even as great as this gifted writer was disposed to believe; for while the malaria of common remittent and intermittent fevers is wafted to a considerable distance—even several miles—over land, and has been asserted by some, though denied by others, as regards some localities at least, to extend to a greater distance over the surface of water, facts show that the poison of the yellow fever extends but little beyond the sources of its origin—attacking one or a few streets, or one side of a street, ship, &c.—and not unfrequently progresses in a contrary direction to the wind. Again, while the miasm of remittents and intermittents ascends to a considerable height—some-

times 1,000 feet—and exhibits a tendency to that effect, the poison of the yellow fever creeps along the surface of the ground, selecting the lower stories of houses and hospitals, and leaving the upper uncontaminated, and does not manifest a tendency to be wafted to any great elevation above the surface of the place where it is evolved.”

“Whatever be the nature of the morbid agent producing the yellow fever, or the materials from which it is derived, and however effective this agent may be, when the requisite meteorological conditions are present to generate the disease, certain it is,” remarks Dr. La Roche, “that its existence cannot alone suffice to account for the epidemic diffusion exhibited by the latter at certain periods. Every one who has investigated the subject of the history of the yellow fever—and the same remark applies equally well to other diseases which take on the epidemic garb, whether contagious or otherwise—knows that, while in some seasons the number of cases is limited, the disease prevailing within very circumscribed boundaries, making slow, if any, progress, and assuming either a sporadic character, or one scarcely deserving the name of epidemic, in other seasons, marked by meteorological conditions and peculiarities of locality differing but little, if at all, from those exhibited in the former instances, the fever, originating from seemingly small beginnings, in one or few spots of circumscribed extent, soon spreads far and wide, rages fiercely and fatally, covers in a short time an enlarged surface of ground, and there attacks a large number of individuals—in a word, assumes the character of a wide-spreading pestilence. The history of the disease shows, as has been pointed out in various parts of the present work, that, in most of the cities of this country, epidemics of the disease have appeared, at intervals of greater or less extent—breaking out violently and spreading widely in some seasons, and sparing the same places for several, and even many, successive seasons, the meteorological phenomena and sources of infection remaining the same.”

The particular influencing agency which thus facilitates the diffusion or increases the severity of a local cause or contagious virus, either by promoting its generation and aggravating its virulence, or by increasing the susceptibility of individuals exposed to its morbid impression, and which, besides, modifies the character of the prevailing disease, is invisible and widely spread, and, owing to its depending apparently upon some peculiar alteration of the surrounding air, has received the name of epidemic constitution of the atmosphere. In what it consists, and to what it is due, are points which, so far, have not been positively ascertained. Its existence can only be inferred from effects that can be accounted for in no other way.

The foregoing is a simple announcement of the views of Dr. La Roche, in relation to the efficient cause of yellow fever. To follow the author in his exposition and defence of them, and in his examination of the nature of the materials giving rise to the peculiar poison productive of the disease, whether it be the product of vegetable or animal decomposition, or of both, would extend our notice of the work before us—already, perhaps, of unreasonable length—to the exclusion of a due notice of others that claim our attention. For the same reason, we must forego even a passing comment upon the treatment of yellow fever, so ably and satisfactorily discussed in the following chapters, as well as upon the interesting chapter with which the second volume closes, devoted to the prophylaxis of the yellow fever—a chapter that might be studied with profit by all such as are intrusted by law with the sanitary police of our cities.

In an appendix an analysis is given of the facts connected with the rise and progress of some of the more prominent among the innumerable epidemics that have been adduced here and elsewhere in illustration of the foreign origin, and of the power of transmissibility of the disease through various channels.

We have endeavoured to present to our readers some idea of the general arrangement of the treatise of Dr. La Roche, and of the manner in which he has treated the more prominent of the questions involved in the history, etiology, and pathology of the yellow fever. In so doing, we have attempted nothing beyond an analysis, interspersed with frequent quotations. We have found no occasion for criticism, none for an expression of dissent from the views advanced by the author. His facts are all accurately and fairly stated, and his deductions bear throughout a legitimate and truthful character. We feel convinced that the work will be favourably received by the entire medical profession. Merely in the light of a complete, faithful, and well-arranged digest of the leading facts in relation to the yellow fever, the work is of great value and interest; while, as a complete scientific treatise on the disease, it is superior to any with which we are acquainted. It is one calculated to confer a tone and character upon American medical literature, and, unless it meets with the favourable reception we anticipate for it, we shall distrust the desire, so constantly professed, for the encouragement of native professional writers.

D. F. C.

ART. XII.—*Elements of Medicine: a Compendious View of Pathology and Therapeutics; or the History and Treatment of Diseases.* By SAMUEL HENRY DICKSON, M. D., LL. D., Professor of the Institutes and Practice of Physic in the Medical College of the State of South Carolina. 8vo. pp. 752, 1855. Blanchard and Lea, Philadelphia.

WE shall not inquire whether there existed any actual demand for another systematic treatise on the practice of medicine, or whether those we already had were not sufficient to supply all the wants of the profession. We would rather encourage every effort on the part of American physicians, endowed with the qualifications necessary for the proper accomplishment of the task, to increase our stock. The works of this class with which we have been already furnished by the practitioners of our own country, are unquestionably of a very high character, and in the multiplication of such books, notwithstanding the necessary sameness of much of the matter contained in them all, we can see no evil, but much good. The difference in the manner in which similar truths are presented, illustrated, and enforced by different writers; the greater extent to which certain points are dwelt upon by one than by another, adapt the several treatises to the diverse wants of that large class of professional readers by whom they are constantly consulted, and of whose libraries they constitute the most important volumes.

The author of the treatise before us is already very favourably known to the medical profession by his *Essays on Pathology and Therapeutics*, published in 1845, and his very admirable report on the *Blending and Conversion of Types in Fevers*, made to the American Medical Association in 1852.

Of the volume of essays, the present treatise is simply a new edition with some difference in its arrangement and many additions. Having given to the former work a somewhat extended notice shortly after its appearance (*See No. XIX. of this Journal, July, 1845*), it will be unnecessary for us to go over the same ground again. The commendation bestowed upon the essays is alike applicable to the work before us, while the errors and deficiencies we

took the liberty of pointing out as in our opinion existing in the former publication are equally apparent in this.

The first part of the work is devoted to the subject of general pathology. A very excellent and beautiful outline is presented of the leading subjects embraced under this head. On the several points connected with the etiology of disease, the views advanced by the author are particularly sound, and in accordance with well attested facts and observations. When, however, he treats of the contagious property of certain diseases, he, in a measure, deserts the domain of facts and close deduction for that of pure hypothesis.

That any disease known to result ordinarily from local causes of evidently limited extent can, under any circumstances, become capable of assuming a contagious character is still to be proved—while it is opposed by all analogy and the entire series of facts bearing on the etiology of disease with which we are acquainted. Of the wide diffusion of known contagious affections by some inscrutable epidemic influence of the atmosphere—some peculiar morbid constitution of the air—over districts more or less extended, there can be no question, but we deny that even this epidemic agency is capable of conferring a contagious property upon diseases which do not, at all times, and under all circumstances, when they occur, exhibit that property.

Dr. Dickson is inclined to refer the production of disease by contagion, to the introduction into the human body of certain organic germs, and suggests, as at least a reasonable conjecture, that in the animal exhalations, infinitely abundant and varied, which are collected about the dense population of a crowded city, these organic germs, whether fungous or animalcular, may find occasionally all the elements essential to their germination and growth, and may propagate and multiply themselves in an atmosphere thus saturated with the pabulum adapted to their support and development.

"Thus," he remarks, "whether we regard the *materies morbi* of epidemic contagions as of animal or vegetable character, there is no difficulty in comprehending their propagation and extension promptly, actively, and widely in dense populations and crowded cities—favoured generally as we notice by heat, moisture, and atmospheric stillness, and repressed more or less by the opposite conditions of dryness, heat, (?) and ventilation, by winds or dilution of air. Nor is there any more difficulty in explaining why a pestilence, usually sporadic, should thus be occasionally epidemic, than in pointing out the causes which make one season prolific in familiar fruits and insects, and another as remarkable for their comparative scarcity."

There would be more plausibility in Dr. Dickson's suggestion as to the cause of contagious epidemics, if these were invariably, or very generally confined to the dense population of crowded cities, and occurred only during seasons marked by heat, moisture, and atmospheric stillness.

In the category of contagious diseases the author includes the Asiatic cholera, the Eastern plague, and the yellow fever, and much of his reasoning in reference to the subject of contagion is based upon the histories of these diseases. Now, if he really believes these several diseases to be contagious, we can have no objection to his endeavouring to sustain his opinion by a presentation of the facts upon which it is based; but we must deny his right to assume as an established truth what is denied by the great majority of the highest medical authorities, and is opposed by an array of facts considered by these as conclusive, and especially to adduce the circumstances connected with the histories of those diseases in illustration of the character and laws of contagion.

Dr. Dickson maintains that all contagions are contingent. Some are

limited by season, others by climate, others, perhaps, by race, while others again seem to bid defiance to all *known* contingencies. Certain unknown circumstances, however, favour, we are told, on the one hand, and on the other, impede their extension and progress. While all races, in all climates, and at all seasons are liable to their attack, still, individual differences of liability are strongly marked. Inoculation with smallpox fails not unfrequently; nor is every one seized who is exposed by attendance on the sick. Every physician must recollect his frequent annoyance by failure of his attempts to vaccinate.

The supposed contingency of certain contagions on race, climate, and season, is predicated on the assumed contagiousness of the yellow fever, dysentery, plague, dengue, yaws and sivvens. But when we turn to smallpox, measles, and scarlet fever, all of which are very generally recognized as contagious, we find their propagation to take place under all circumstances and in all places—thus bidding defiance to all known contingencies. They have, therefore, an absolute contagious character, altogether independent of those circumstances which may favour or impede their extension or progress, or of an occasional insusceptibility in particular individuals to be attacked by them. Contagiousness is a quality inseparable from them, and independent of all contingencies. Their propagation or extension by a specific virus may be promoted by certain circumstances or conditions, but no circumstances or conditions can confer upon any disease the power of secreting a specific virus by which it is capable of reproducing itself in other individuals to whom that virus is communicated, by direct contact or through the medium of the surrounding atmosphere. Hence the contagiousness of a disease cannot, with propriety, be said to be ever contingent. As well remarked by Dr. La Roche in his work on the yellow fever (vol. ii. p. 576):—

“Contagious diseases require for their production the agency of *specific causes*. Each of them is the effect of a special cause which is essential to its production, and gives rise to no other complaint. Again, as Dr. Caldwell remarked long ago, there are no half truths in medicine any more than in religion. Hence, a disease is contagious, or it is not. A complaint contagious only under certain circumstances is not contagious at all. ‘If the yellow fever is contagious,’ says Dr. Barton, ‘it is a law of the disease. This it must carry into all places and under all circumstances (like smallpox). A *contingent contagion* is a medical misnomer, is void of a precedent, and has no parallel in the annals of the science.’ In other words, no disease is ever contagious unless it has originated from contagion, and contagious disease can only be produced by their respective contagions.”

“Rules have been offered,” Dr. Dickson remarks, “for the determination of the contagious character of disease. Inoculation will decide where any palpable matter is formed. Where there is no visible product, we must be guided by rational inference from observed facts; such as:—

- “1. Repeated spread among those surrounding a sick man.
- “2. The occurrence of repeated cases upon exposure to varied fomites; these are circumstances which give obvious reasons for the belief of the contagiousness of any maladies of which they are predicable.
- “3. Progressive extension from a first observed locality.
- “4. A decided preference for dense populations.
- “5. Repeated migration with travelling persons or bodies.
- “6. A preference for the ordinary thoroughfares by sea and by land.”

These criterions of the contagious character of disease are expressed with too much looseness to constitute them positive data for a correct diagnosis. The repeated spread of a disease among those surrounding a patient affected with it, in an infected locality or during the prevalence of a severe epidemic, would

afford no conclusive evidence of its contagious character. When such is the case after the patient has been removed to a healthy location, and beyond the sphere of the epidemic, it becomes then a very certain evidence of contagion.

A gradual extension of disease from a first observed locality is very commonly, though not always, the indication of its propagation by contagion. A disease of acknowledged contagious character may, however, when epidemic, spread with great rapidity beyond the locality where it first appeared.

Contagious diseases must necessarily prevail to the greatest extent, and spread with most rapidity among a dense population. On the other hand, however, they make their appearance as often, perhaps, and exhibit as great a degree of virulence, in localities sparsely populated. The smallpox and scarlet fever have appeared, of late years, even more frequently in the interior of our State than in the city of Philadelphia, and, in proportion to population, have been attended by as great an amount of mortality.

If by the fifth rule is meant that contagious diseases may be carried from place to place in the persons of travellers, and become in this manner disseminated, the rule is a correct one. But, at the same time, we are not to be led into a very common error of supposing that a disease is necessarily contagious because its appearance in a place is either simultaneous with, or occurs immediately after, the arrival of individuals from localities where it prevailed at the time of their departure. To prove the origin of the disease from contagion derived from their persons or effects, the first case or cases that occur must be clearly traced to this source.

Although we cannot agree with Dr. Dickson in all his reasonings and deductions in reference to the causation of disease—though we are persuaded that many of his views are purely hypothetical, and not in strict accordance with well-established facts—still we must admit that the section devoted to etiology contains, with few exceptions, a very excellent and well-digested abstract of the present state of our knowledge on the subject.

The third section treats of the seats of disease. The subject is very briefly discussed, but, at the same time, in an instructive and able manner.

The fourth section is devoted to a consideration of the phenomena of disease. These are rapidly but very skilfully sketched as they present themselves in the circulatory, digestive, respiratory, sensorial, motory, and excretory systems. In this section are also discussed the subjects of irritation, congestion, and inflammation. On these, the views of the author will be found to be in general sound and in accordance with well-established facts and observations. He very properly, we think, considers "every theory of inflammation insufficient in its own nature, and untenable, which shall proceed upon the supposition of a mere increase or abatement, enhancement or diminution of action, excitement, power." Inflammation, he maintains, is a condition essentially morbid, not in degree only, but in its very nature; and this is made equally clear, whether we refer to its consequences, its symptoms, or its causes. These causes, infinitely varied as they are, not only affect the force or intensity of action in the part which they impress, but go mainly to determine the mode which that action shall assume, and the results which it shall produce; an influence which is exerted as well by the exciting, occasional, or accidental causes, in many instances, as it is in all by the predisposing or constitutional. In the meanwhile, the normal or physiological actions are suppressed, subverted, and substituted by the new and diseased actions, and these may be either more or less forcible than the former.

Dr. Dickson is unwilling to admit the correctness of the distinction so commonly received—and, indeed, now made part of our ordinary technical lan-

guage—between acute and chronic inflammation. The essential history of each, he remarks, is the same; the ultimate results or consequences the same. It seems unscientific, then, if not idle, to attempt the establishment of any pathological distinction upon the mere difference of time occupied by the several steps of the processes gone through. While, however, it may be admitted that, in all their essential pathological characters, the acute and chronic forms of inflammation are identical, still, when we consider the striking difference which exists between their phenomena, especially between the general or constitutional symptoms peculiar to each, there appears to be a propriety of distinguishing between them. The distinction will be found important in reference to diagnosis, as well as to practice.

The author thinks that we are now prepared to appreciate the existence of two great sources of periodicity as a pathological phenomenon—

“1. Conformity to, or harmony with, the innumerable movements and changes of universal nature which surround us, and of necessity impress us; and 2. The special or specific trains of movement belonging to, and inherent in, diseases themselves, and constituting an essential portion of their nature. Each has its own characteristic period of incubation; its own time and mode of access; its own duration and termination or decline; if paroxysmal and recurrent, its own latent period.”

All this is very true, but we cannot see that it advances us one step towards an acquaintance with the actual cause of periodicity in disease, and of its varying phenomena.

In treating, in the next section, of the tendencies of disease, Dr. Dickson sets out with the position that “the tendency of *all the various forms of disease* is essentially and in their own nature to death; death either of a part or of the whole body, according as the morbid affection has been general or local.” Now, we believe that the experience of every observant physician of extensive practice will bear us out in the opposite statement, that the tendency, in the majority of diseases, is to a spontaneous cessation. Until destruction of structure has taken place in the organs of the living body, there is a constant tendency manifested by them, when the seat of disease, to return to their normal state and to the resumption of their normal functions. How numerous are the cases of actual disease that recover fully and perfectly, and often with great promptness, without any other treatment than the institution of such hygienic measures as are calculated to remove the patient from the influence of their causes. This, in fact, the author himself acknowledges.

“Happily for us,” he remarks, “few of the causative agencies of the multiplied forms of disease are permanent in their application or influence.

“The occasional excitement is removed or passes away, or the predisposition, the susceptibility to its effects, wears off or is exhausted, the morbid impression, if not renewed, undergoes a gradual effacement by the ordinary processes of use, waste, and supply, and is ultimately obliterated, the system returning to its original condition of health, or approaching it more or less closely.”

Again he remarks, speaking of *disease* as occasioned by, or depending upon, the presence or influence of some special agent, interfering, in its own defined mode, with the regular play of the organization by which the functions are performed:

“This cause ceasing to act, *the disease produced by it*”—the essential and natural tendency of which, he affirms, is unto death—“*necessarily comes to an end*, and the train of natural and healthy movement goes on again.”

It is well known that many diseases are self-limited; they acquire a certain intensity, and then gradually or promptly decline, and finally disappear—some

with perfect regularity and within certain periods. These diseases will cease, and the patient return to his accustomed health, without the interference of the physician, provided that, from causes by no means essential to them, but depending upon predisposition or some pre-existing latent disease in the patient, or his exposure to a new morbid influence, no important organ shall become affected, and thus, by its complication, endanger life, unless promptly relieved by an appropriate treatment.

In a considerable class of cases, the entire office of the physician is, in fact, restricted almost entirely to watching and guarding against this tendency to, or occurrence, from any cause, of disease in one or other of the organs, little else being required on his part to insure their favourable termination.

The second part of Dr. Dickson's treatise is devoted to the subjects of special pathology and therapeutics.

The several diseases treated of are considered in the following succession, as they prominently affect—

1. The Circulatory or Vascular System, including the organs of secretion and absorption, as well as of sanguineous distribution and nutrition.
2. The Digestive System, including the collatitious viscera, as well as those engaged directly in the solution and conversion of food.
3. The Respiratory System, in which, with Good, Dr. D. merges the Vocal, because not separable practically.
4. The Sensorial System, which coincides very nearly with the Neuroses of Cullen, Parr, and others.
5. The Motory System, including the whole mechanism of locomotion, as well passive as active: muscles, bones, joints, &c.
6. The Excretory System. Under this head are comprised the affections of the cutaneous integuments and the diseases of the urinary organs.
7. The Generative System.

The first class of diseases treated of, as of the circulatory or vascular system, are the fevers. In his brief discussion of the general pathology of fever, the leading views presented by the author are at least plausible, if occasionally hypothetical, and have all a direct practical bearing.

In regard to the doctrine of the natural tendency of fever to run a certain course, and to terminate "in the restoration of health," and to the question so frequently agitated, whether it is possible or proper to arrest or cut short its course, Dr. D. remarks:—

"I have elsewhere stated my belief that the tendency of all diseases is to disorganization and death. But the cause which produces disease may be transient in its influence or in its application, or it is removed by some change of circumstances; it is originally somewhat less than mortal in its intensity, or its force undergoes diminution; the excitability on which it first acted wears out; the predisposition to which it was adapted is altered in some mode; the system becomes habituated, and thus callous to its irritation—as a ball will sometimes remain quietly lodged in a part which it had inflamed severely; owing to some one or more of these conditions, all amounting virtually to a removal of the cause of disease, it comes to an end (*causa sublata tollitur effectus*) before its natural tendencies are manifested in their ultimate result—before either death or disorganization has occurred. It is thus we explain the apparently spontaneous restoration of health every day met with, and so apt to be attributed to the *vis medicatrix*—the restorative energies of the constitution."

In other words, Dr. D. is forced to admit the fact that, under a variety of circumstances, a spontaneous cessation of disease may take place without the destruction of any portion of the organism in which it occurred, and, con-

sequently, that "the tendency of all diseases is *not* essentially and in their own nature to death, death either of a part or of the whole body."

Dr. Dickson very properly cautions the young practitioner against the tendency, in any disease, to a spontaneous solution. The office of the physician is to remove, if possible, the source of evil, to counteract its influences, and to diminish their intensity by every appropriate means, and to save the more important organs, or those most prone to take on morbid action, by constant watchfulness and timely and judicious interference.

Our author declines a discussion of the question as to our ability "to cut short the course of a fever." What physician, he asks, "has not repeatedly seen a febrile attack abruptly arrested, and all its concomitant irritations, congestions, and inflammations, suddenly resolved by bloodletting, by emetics, by cathartics, by sudorifics, by quinine, opium, piperine? Who has not witnessed the distinct and decided substitution of mercurial action for the previous train of febrile symptoms?"

A very interesting and sensible chapter is given on the types of fevers. Dr. D. acknowledges the following distinct types:—

"I. The *Periodical*: Intermittents and remittents generally.

"II. The *Continued*: comprising 1, yellow fever; 2, typhus fever, typhoid and true typhus, the epidemic fever of Great Britain, simple fever or ephemera, relapsing fever of Jenner; 3, catarrhal fever, influenza; 4, pneumonia typhoides, spotted fever?

"III. The *Exanthematous*: 1, variola; 2, measles; 3, scarlatina; 4, erysipelas; 5, dengue, etc.

"I am persuaded," he says, "that the truth will be found, on examination, in the following propositions: (A.) That each type of fever is the result of a definite cause, revellent in its properties, character, and modes of action to the effects produced. (B.) That these causes, varying greatly in nature, must be sometimes similar, and sometimes contrasted, that is, they must vary greatly in the character of their efficiency. (C.) That causes of different kinds or nature may sometimes coexist. (D.) That when they resemble each other, their effects or influences are readily blended, and mingled, and interchanged, as one or the other may predominate. (E.) That when dissimilar causes coexist, they may sometimes act together, but not often; may sometimes blend their influences, but not readily or freely. That they may possibly supersede each other by substitution; but that in no instance can the effect of one cause be the effect of any other cause. This sort of transformation, the only true conversion in the logical sense, is a rational impossibility, whether we regard diseases materially and ontologically as entities, or pathologically and dynamically as mere affections of the organism, arising out of precedent impressions, or from causative agencies.

"The coexistence of maladies of the same and even of different classes, though once pronounced, according to Hunter's unguarded and unfounded maxim, an anomaly in nature, and even now thought by many to be a rare phenomenon, is of quite as frequent occurrence as circumstances admit, that is to say, there seems nothing in the nature of things generally to forbid it. We cannot now venture to affirm that there is anything protective or exclusive resulting from the presence of one disease in the system, that shall secure it against the invasion of others to which it is exposed. That certain conditions of the body or its parts, are incompatible and cannot concur, does indeed seem probable theoretically, but cannot be fairly proved. The records of medicine abound with histories of concurrent disease of very great variety.

"I need offer no proof of the frequent blending of the types connate in cause and analogous in symptoms. As to the periodical fevers, regarding the mass of subjects, we shall find at the same time, in the same locality, and exposed to the same causative agencies, cases of intermittent and remittent, and congestive fevers of every form and grade. In the same individual a simple intermittent shall be followed by a 'malignant,' 'pernicious,' or 'congestive'

paroxysm, which, if not fatal, will subside into an ordinary remittent promiscuously interchanging. Perhaps the strongest and most marked evidences of these interesting phenomena are found in the histories of African fevers, given by Burnet, Pym, Bryson, McWilliam, and others. An absolute confusion of grade and violence seems to reign. In the same settlement, the endemic fever perennially existing will suddenly become epidemic, pestilential, from simply changing to congestive, pernicious, malignant, vehemently destructive. In the same ship, under the same exposure and general conditions, some will be seized with a mild intermittent, and others with the most terrible and promptly fatal remittents.

"The similar interfusion of the continued fevers, speaking first of the cases in mass, is as easily exhibited, nay, unless we are prepared to set aside the authority of the greater number of those who have in all countries and climates, and at all periods treated of these fevers, we shall be forced to admit the difficulty of finding them separate in nature, and keeping them apart in description. I am not prepared to follow a course so harshly critical. I do not object to the nicest diagnosis, the drawing of the most delicate and precise lines of distinction. I applaud rather all attempts in this way, and study with pleasure as well as profit, the essays of Copland, Gerhard, Jackson, Ware, and Jenner. But even in these ingenious and useful efforts at analysis, these vivid delineations of differences, we are perpetually presented with striking analogies, striking resemblances, the intrusion of characteristic symptoms, where they were not looked for, and their absence where they were confidently expected. The synocha and synochus, the typhus and typhoid run together inexplicably. Partisan advocates of special doctrines often find no reply available, but a denial of some alleged fact as a suggestion either of error or carelessness in observation. The impartial judge, the earnest student, is not satisfied, however, with this mode of conducting the investigation; and in summing up for the purpose of arriving at the truth, feels himself under the necessity of regarding with equal eye, and weighing with the same scales the statements on both sides of the question—accepting no 'foregone conclusion.'"

We can afford room for only one other extract from this chapter of the treatise. It is one with which we find no fault, in which a doctrine is inculcated, which is in direct opposition to the supposition that a disease not originally and absolutely contagious can become so under any possible contingency.

"I cannot for a moment," Dr. Dickson remarks, "admit the truth of the modern doctrine, that fever, or, as it is termed, the *febrile element*, is a unit, identical in all the forms of that protean malady. On the contrary I am prepared to maintain that from the great diversity of specific causes of fever, a great diversity of specific effects must necessarily result; and that as the effect must always be precisely relevant to the cause, unless the latter undergoes modification, transformation, or substitution, the former must remain unaltered. Specific difference in causative or generative agency will give absolutely specific difference in results. If, for example, some fevers are contagious, and not all, this single fact must constitute of itself an essentiality of diagnosis. Contagion, speaking of it materially, not dynamically, must, in the present state of our knowledge, be assumed to be an organic germ, vital, reproductive, and self-multiplying. It is a vital organic germ, because reproductive and self-multiplying; these properties are predicable of no form of dead or inorganic matter. It follows, therefore, clearly and logically, that contagious types of fever, if any exist, are divided *toto calo*, from non-contagious types of fever, if there be any such. But the existence of both is universally recognized; it is known that they may co-exist, occurring together in the same locality, the causes of both being present there; they may be blended together, their causes conjointly or simultaneously affecting the same subject. We cannot imagine these causes or effects to be convertible in the precise and logically accurate sense by any possible mode of transformation, under any conceivable force or influence, but we know them in many cases to be superimposed upon, nay, substituted one for

the other; in some instances a total subversion of one by the other seems to take place, the first disappearing in all its characteristics, and the second prevailing with despotic and exclusive sway. Such conversion is not, we repeat, a rare occurrence, and may happen universally, as between any two of the diversified forms of fever."

From the foregoing somewhat obscurely worded sentence, we understand the author as teaching: 1st. That the contagious are divided from non-contagious fevers *toto calo*, essentially, specifically. 2d. That these two forms or types of fever result from generative causes specifically distinct. 3d. That the effects resulting from one or other of these generative causes is always, specifically the same. 4th. That though the two sets of causes may co-exist at the same time and in the same place, and though one of them may acquire such an increase in diffusion or potency as totally to subvert or overpower the other, still neither they nor the effects resulting from them are convertible, by any possible mode of transformation, or under any conceivable force or influence. And these several positions appear to us to be borne out by all the facts and observations that have been accumulated in reference to the etiology of disease.

A very excellent account is given by Dr. Dickson of the several forms of idiopathic fever, comprising a brief but instructive summary of all the well established facts and conclusions illustrative of their causes, character and ordinary progress, with an exposition—judicious in all its leading features—of the treatment adapted to their several stages and to the different accidental circumstances that may occur, calculated to modify their respective characters.

We should, it is true, dissent from some of the pathological views advanced by the author, and call in question the correctness of some few of his practical directions. In general, however—in all their more important features at least—the doctrines advanced by him are sound, while his practical directions are the result evidently of much personal experience.

Intermittent and remittent fever, Dr. Dickson refers, with the great majority of authorities, to malaria, the miasm from low grounds, stagnant pools, and swampy meadows.

Yellow fever he does not believe to be the result of a malaria produced by the continued operation of heat and the effluvia of decomposing animal and vegetable matter. He believes it to be the effect of a specific and peculiar cause.

"In certain localities, this obscure cause is," he supposes, "permanent and always active; in others, it exhibits only an occasional activity, by which alone its presence can be inferred. Its relation to season and temperature is equally well made out, being efficient only during the hot months of summer and autumn.

"Yellow fever," according to Dr. D., "is contagious; in other words, a case of yellow fever having been generated in favourable season and locality, by its unknown and undetected cause, becomes itself a generating centre productive of other cases, or of a morbid agent capable of producing them.

"It is transmissible from one centre to another, or from any one of its generating centres to a healthy locality; and this communication or extension may take place in two modes, either by conveyance of a portion of atmosphere in which is diffused its undefined specific cause, as in the hold of a foul vessel, from any place where it prevails epidemically, or by the introduction of a sick body, or any fomites imbued with its contagion.

"As a general rule, we may add that the contagiousness of yellow fever is limited by certain contingencies." "The same circumstances," remarks Dr. D., "limit the efficiency also of the generating cause, as, indeed, of all the alleged causes of yellow fever. Thus, high temperature is necessary to its

production, existence, and extension. No matter *how* it is generated, the fact is known that it does not exist anywhere in winter. Hence it follows, that if carried into a cold region from a hot one, it will not diffuse itself."

For a very full and able exposition of the facts and arguments by which the contagious character of yellow fever is disproved, we refer our readers to the second volume of the treatise on that disease by Dr. La Roche, which has just appeared.

Typhous fever, under which term the author includes the typhus and typhoid or enteric fevers, he presumes to be caused, "as in the similar conditions of scrofulosis and tuberculosis," by "a morbid diathesis" generated within the system, "under the influence, more or less pronounced, of want of ventilation, or of cleanliness, or of *contagion*—all the agencies that we include under the term *ochlesis*. The nutrition becomes abnormal; the blood is diseased; the excretory organs, the skin and kidneys especially, failing to perform their functions, a poisonous matter accumulates and is deposited, chiefly, about the intestines, upon which the task of depuration is thrown; but also in the mesenteric glands, and often upon the parotids and sub-maxillaries. These symptoms are not peculiar to, or exclusively met with in typhoid, but may occur in all fevers, if sufficiently protracted." He pronounces the typhous fevers to be diseases whose cause and propagation are due to contagion. Now, if such be the case, this typhous contagion—this specific "vital organic germ," must exist diffused throughout the entire atmosphere of every portion of the globe, for in every place where ill-fed masses are accumulated in filthy, ill ventilated localities or apartments, there will typhus fever prevail. Now, from the influence of local causes alone—without the presence of its specific contagion, can any known contagious disease be produced? can any one originating from a non-contagious cause, "by any possible mode of transformation, or in any conceivable force or influence," assume the contagious type?

Catarrhal fever, which Dr. D. very properly treats of in connection with the idiopathic fevers, he very evidently inclines to view, in its epidemic form at least, as capable of being propagated by contagion; while, at the same time, he admits that sporadic or individual attacks may be produced by exposure to night air, to a strong draught, to getting the feet wet, sleeping in damp sheets, being caught in a thunder-storm, or remaining long unprotected in inclement weather; nay, he admits farther, that great numbers of persons have been attacked at once, upon any very sudden and remarkable change of temperature, whether the thermometer rises or falls. Now, to prove that epidemic catarrhal fever is contagious, it will be necessary to show that it differs *toto celo*, that it is a type of fever specially different from catarrhal fever occurring sporadically, or affecting entire communities from sudden transitions in the temperature, according to the law laid down by Dr. D., already referred to.

Spotted fever, or, as the author names it, from a constant though not invariable or essential symptom of the disease, pneumonia typhoides, he acknowledges to originate from causes of which we are in entire ignorance; of its contagiousness he has personally seen no proofs. Spotted fever is evidently a mere form or modification of typhoid fever; in the etiology of the two diseases, so far as we are acquainted with it, there would appear to be a perfect identity.

In connection with remittent fever, Dr. Dickson describes the disease known as the infantile remittent or gastric fever of children, which he ascribes to the presence of intestinal worms, or to any species of gastric or intestinal derange-

ment, any disorder of the chylopoietic viscera, produced by improper food, over-feeding, etc. There can be no doubt that from these causes a fever may be produced in the mobile constitutions of young children, and that such fever will in general resume the remittent type. But this is not the fever described by the best authorities under the name of infantile remittent fever, nor the one so frequently met with by the practitioner in our larger cities. A careful analysis of the symptoms of this latter, and an examination of the lesions discovered after death, have convinced us that it is essentially the same disease as the typhoid fever of the adult, the slight modification in the morbid phenomena and course of the disease being referable to the difference in the age of the patients.

Under the head of symptomatic fever, a very full and excellent account is given of hectic fever and its treatment.

Of the cardiac affections, a short but, in its general outlines, a very faithful sketch follows next in order. Syncope, to which the author has assigned the rank of a disease, we cannot consider in any other light than as a symptom of various morbid conditions of the heart itself, of the blood, or of remote organs; sometimes of mere impressions made upon the nervous system, unconnected with disease. It is often the effect of mental emotions alone.

Angina pectoris Dr. D. describes as a cramp or spasm of the heart—a purely functional disease of the organ, neither dependent upon a disordered state of the stomach, a gouty diathesis, or simple vascular plethora; nor necessarily connected with any organic disease of the heart or of its vessels. Each of the first-named conditions may, he admits, be considered as among its efficient predisposing and occasional causes, and that with the last it may be coincident in certain subjects.

It would occupy too much space to enter into an examination of the pathology of angina pectoris. We believe it to be, in the majority of cases, a symptomatic affection, often of disease of the heart itself, though, in numerous instances, entirely unconnected with any organic affection of that or any other organ.

The author's account of hemorrhage and of dropsy, as well in relation to their pathology as to their therapeutical management, is replete with good sense, and sound and judicious views.

Dr. D. treats of scurvy in connection with the hemorrhages, to which it has certainly very little pathological relationship. His account of the disease, though very concise, is, upon the whole, sufficiently accurate.

The chapter on hydrocephalus—an affection which the author includes among the dropsies—is a particularly confused and unsatisfactory one. The name hydrocephalus has been unfortunately applied to diseased conditions of the brain having little or no actual relationship with each other. In some of these, serous effusion into the ventricles, or between the membranes of the organ, occurs to too slight an extent to constitute the case one of dropsy, in the proper acceptation of the term, and has but a secondary influence upon the phenomena, course, or termination of the disease with which the brain or its meninges are actually affected. This loose and improper use of the term hydrocephalus, has not been sufficiently attended to by the author, and hence the confusion into which he has fallen; some of his remarks, under this head, being applicable to one form, while others of them have reference to other and very dissimilar forms of encephalic disease.

The chapters devoted to the consideration of scrofulosis and tuberculosis—though extremely concise, considering the vast importance of the subjects embraced in them, and the great obscurity in which almost every important

point connected with their etiology and pathology generally is enveloped—present an excellent digest of the leading facts known in relation to these affections. The author has not, it is true, entered into any very profound examination of the true character and causation of the particular morbid condition of the nutritive function by which they are produced. The interesting question of the influence of climate in their production, prevention, and cure, in reference to which, until recently, much ignorance has been exhibited, he has not touched upon.

The diseases of the digestive system, comprising the more prominent affections of the stomach, bowels, liver, and spleen, are treated of, in general, with great ability.

Under this head, the author presents an interesting account of that singular form of disease known by the name of milk sickness, produced in the human subject by eating the flesh, or partaking of the milk of herbivorous animals labouring under a particular malady, endemic and exclusive, so far as Dr. D. is aware, to certain localities throughout the southern and southwestern States of our Union.

As pneumatosis he describes the undue generation of a gaseous fluid within the digestive cavity, which often constitutes a prominent symptom of disease; it is sometimes, Dr. Dickson thinks, a primary affection, not unfrequently of immense annoyance and unconquerable tenacity.

Its presence is ascribed usually to the decomposition of the ingesta, but, with the author, we are satisfied that, in many cases, the gas which accumulates in the stomach and intestines to an extent to cause great uneasiness, and often actual suffering to the patient, is either directly or indirectly exhaled or secreted from the alimentary mucous membrane.

"This source, denied by most, is admitted by Williams and Vogel. It is proved by a large mass of facts. Flatulence comes on often after fasting. Dyspeptics are frequently attacked in this way, and some, indeed, eat to prevent a fit of colic. It is often connected with neuropathic disorders; hysteric colic and the globus hystericus are familiar. It often attends on paroxysms of nephralgia, and is known as nephritic colic. It comes on occasionally in a regularly periodical form. I have twice seen the stomach suddenly distended with gas to such a degree as to press up the diaphragm, and displace the heart, and oppress the breathing, by compressing the lungs."

It is not our purpose to discuss the correctness of the author's assertion that the proofs of the communicability and actual transportation from one locality to another of cholera asphyxia, are abundant. We have had ample and repeated opportunities for a careful study of the disease, and from our personal observations can gather not one single fact that would give the slightest support to the doctrine of its communicability, under any circumstances, from the sick to the well. Of its extension, from place to place, in consequence of some peculiar epidemic constitution of the atmosphere there can be no doubt. And a reference to this mode of extension will explain many of the facts adduced as proofs of the contagiousness of cholera.

Dysentery, in its epidemic form at least, is another of the diseases which the author includes among those communicable by contagion. The fact, however, is far from being actually established, even in reference to that form of the malady occurring "in camps, jails, hospitals, and other crowded and ill-ventilated places, and when the attendant fever takes the typhoid type." We cannot admit that a disease not absolutely contagious in itself can become so under any possible contingency. If it be said that, although simple dysentery is not contagious, yet by its combination with a contagious fever it may

become so, this would be tantamount to admitting that the contagion of one disease may produce a contagious principle in another with which it has become accidentally conjoined. To prove the contagiousness of typhoid dysentery, the disease resulting from exposure to a patient labouring under it, must be not merely typhoid fever, but typhoid fever and dysentery conjoined.

The author has given a very good account of parotitis, or mumps. In those cases in which a sudden cessation of the disease of the parotid gland is followed by inflammation and swelling of the mamma in the female, or of the testis in the male, and in rare cases by phrenitis, we have no evidence that the morbid affections of the one gland has been translated, as Dr. D. expresses it, to another gland, or to the brain. This doctrine of actual metastasis is the remnant of a pathology long since exploded, and it is proper that the employment of modes of expression which have no positive meaning, except upon a recognition of that pathology, should be avoided.

The diseases of the respiratory system—or rather the more prominent ones—are treated of with great brevity. The whole account given by the author of these affections, which, from their frequency, and the mortality by which several of them are liable to be attended, demand, on the part of the practitioner, a close acquaintance with their pathology—their correct and early diagnosis—their usual stages and progress—and, with the treatment which experience has shown to be the best adapted to arrest their fatal tendency—is comprised in about sixty pages, including a preliminary examination of the modes of thoracic exploration, and of the nature and value of the physical signs of disease thus attained.

In the short digest, with which we are presented, of the diseases of the respiratory apparatus, we find, nevertheless, a very good outline of their pathology and treatment—there is a want of fulness and distinctness, however, throughout, which unfits this portion of the treatise, at least, from becoming a satisfactory or useful manual of reference to the advanced student or practitioner of medicine.

In the section devoted to a consideration of the diseases of the sensorial system, Dr. Dickson treats of cephalalgia, phrenitis, under which head he includes all the encephalic inflammations, mania-a-potu, apoplexy, paralysis, chorea, and epilepsy.

Insanity is treated of under the head of chronic phrenitis—being regarded by the author “as uniformly depending upon cerebral inflammation.” The account of its pathology and treatment occupies just three pages. It is evident, therefore, that it must be incomplete and unsatisfactory. It does not present even an allusion to, much less a digest of the mass of important facts that have been collected by the labours of recent observers, in reference to the pathology of insanity in the various phases under which it presents itself, and its proper medical, physical, and moral treatment.

The doctrine that insanity is dependent upon inflammation of the brain in even the majority of cases is one that cannot be received by any one conversant with the disease, while the treatment based upon such doctrine—bleeding, active purgation, blistering, antimonials, mercurials—has been shown to be, not simply inefficient in the cure of insanity, but absolutely mischievous.

Mania-a-potu, or more correctly delirium tremens, the author is, also, “disposed to regard as a peculiar form of phrenitis, modified, 1st, by the causes which produce it, and 2d, by the morbid condition of other organs with which it is universally connected.” Though he does not propose to treat it by bleeding, purging, &c.

In the history, pathognomony, and morbid anatomy of delirium tremens,

taken in conjunction with the treatment which experience has shown to be the most successful in its cure, not a single fact is presented of the disease being dependent upon inflammation, either acute or chronic, of the brain or its meninges. The lesions of the brain, usually slight in extent and unimportant in character, which are occasionally met with in those who die of delirium tremens, have had evidently no influence in its production, and a very slight one, if any, in the production of the fatal event.

The account given of apoplexy and paralysis, considered as a mere outline, is a very good one. In his account of the pathological anatomy of these diseases, without a very full acquaintance with which it will be very difficult to understand their true character, their varied phenomena and course in different cases, and their proper therapeutical management, the author has neglected many important details of vast importance. The usual location of cerebral hemorrhages, their varying extent, and the changes which occur in and around the effused blood, matters of deep interest in the pathological history of apoplexy, are not even alluded to.

The chapter on chorea presents a very fair outline of the leading facts in relation to the pathology and treatment of the disease. The author remarks that the prognosis is, in general, favourable. He has seen no death, and but once a return of the disease; and such is the testimony of several writers. So far as relates to the danger of a fatal termination in cases of chorea, this is certainly not very great. We have never seen an instance in which death could, with propriety, be said to be the direct result of the disease. In general, also, we have found chorea, when early attacked by proper remedial measures, to be, without much difficulty, completely and radically cured. Occasionally, however, we have known it to persist and augment in violence, under every plan of treatment instituted for its arrest. In most of such instances the patient has sunk into a state of fatuity more or less complete.

A very good account is given of epilepsy, of neuralgia, of tetanus, and of hydrophobia.

Under the head of diseases of the motory system, the author treats of gout and rheumatism. The propriety of ranking the first of these among the diseases of the motory system may well be disputed. Although the leading symptoms of an attack of what has been termed regular gout, are manifested most prominently, but by no means exclusively, in the neighbourhood of the joints, still, taking into consideration the entire phenomena and history of the disease, we think there are sufficient reasons for including it among those affections which have their origin in disturbances of the digestive and assimilatory functions.

The account presented by Dr. D. of both gout and rheumatism, comprises an excellent summary of their symptoms, predisposing and exciting causes, progress, and treatment.

The diseases of the excrement system include, agreeably to the classification of Dr. D., the exanthemata or eruptive fevers.

We shall not discuss the question as to the propriety of arranging the exanthematic fevers as diseases of the excrement system in a strictly pathological sense. The account given by the author of these affections is sufficiently correct, and his directions for their treatment accord with those laid down by the leading medical authorities of Europe and America. His account of the dengue is among the fullest and most satisfactory with which we are acquainted.

We fully concur with Dr. D. in considering erysipelas as a constitutional disease, depending upon a morbid condition of the blood. We believe, also,

in the contagiousness of this affection. Although "the force of the malady" is mainly expended upon the skin, still we are to recollect that in many cases its effects are displayed in the production of the most intense inflammation of the mucous and serous tissues.

The other diseases of the excrement system described by the author, are those of the urinary organs. Of these, a very short and unsatisfactory account is presented.

It would be unfair to judge of the work before us by the same criterion that would be proper in testing the merits of a complete systematic treatise on the practice of medicine. It is not offered as such; the author claiming for it no higher title than that of a simple compendium of pathology and therapeutics, adapted for the instruction of those engaged in the study of medicine, and as an aid to those who have recently assumed the responsibilities of practice. As such, all that we had a right to expect in it is a full and fair statement of well established elementary principles, and approved rules of practice. Even viewed in the light of a simple elementary treatise, and in reference to the class of persons for whose instruction it is primarily intended, the work is one which demands our closest scrutiny, from the powerful and lasting influence it is calculated to exert over the opinions and practice of a large and important portion of the medical profession. In no class of medical writings is it so essential that the doctrines and practical precepts inculcated should be in strict accordance with well-established facts and the best accredited series of observations; in short, that it should present a fair transcript of the actual state of our knowledge in pathology and therapeutics.

The work of Dr. Dickson presents no little unevenness in its execution. Many of the diseases are admirably sketched, and the outline of their treatment given with great accuracy; a few are discussed with a degree of fulness even beyond the requirements of a simple elementary treatise; occasionally, however, we regret to say, the author's pathological as well as therapeutical teachings, do not present a clear and satisfactory exposition of the views generally received and approved by the profession, while his portraiture of disease, in more than one instance, is defective and unsatisfactory. But, whatever may be the faults of the present compendium of the elements of medicine, it must be conceded that it has excellencies which far outbalance them. The plain, correct style in which the work is written, is deserving of all praise.

D. F. C.

BIBLIOGRAPHICAL NOTICES.

ART. XIII.—*Transactions of American Medical Societies.*

1. *Transactions of the Medical Association of the State of Alabama, at its Eighth Annual Session, begun and held in the City of Mobile, February 5, 6, 7, 1855.* 8vo. pp. 148.
2. *Transactions of the State Medical Society of the State of New York, held in the City of Albany, February, 1855.* 8vo. pp. 308.
3. *Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session, held at Hollidaysburg, May, 1855.* 8vo. pp. 160.

THE *Transactions* of the Medical Association of Alabama, for 1855, embrace three addresses. The first is by the President, Dr. L. H. Anderson, delivered at the opening of the session. It is principally occupied with a rehearsal of the leading measures adopted by him, since the last meeting, for advancing the interests of the Association, and a few practical suggestions respecting its future action. It contains some excellent suggestions in regard to the proper treatment of quackery by the profession which are well worthy of consideration. Too much prominence has been given, of late years, in the addresses delivered before our professional Associations, to the arts and deceptions of the professed charlatan, and to the encroachments and assumptions of the professors of exclusive dogmas and visionary systems of therapeutics based upon neither sound philosophy nor common sense. The complaints so often made of the success of these mountebanks and irregular pretenders to the character and office of legitimate medical practitioners, and the belligerent attitude assumed towards them in addresses made exclusively to professional audiences, are not calculated to do good. The well educated and intelligent physician needs not to be repeatedly warned of the folly and wickedness of quackery in all the phases it assumes, or behind whatever pretension of doctrine or of system it may attempt to conceal its true features. No high-minded, honourable member of our profession needs to be guarded against entering amid the ranks of such as "traffic with the health and lives of their fellow-citizens for the sake of lucre." This constant recurrence to the subject of quackery on the part of the profession, while it is useless, is at the same time a violation of that dignity which should ever characterize its members. In so far as the public become cognizant of the prominent place we assign to the charlatan and irregular practitioner in our professional addresses, and of our acknowledgment that we are in danger of being supplanted by them in the estimation of the community—that they have already entrenched upon the legitimate emoluments of our practice—we commit a serious wrong upon ourselves; and by giving to them an undue importance and notoriety, we aid in increasing their numbers and augmenting their success. The proper course for us is to pursue quietly our own career as diligent cultivators and practitioners of legitimate medicine, and while we keep aloof from quacks and irregulars, and defend our ranks from their encroachments, to let them pursue their own separate course, well assured that more or less success will be the fruit of their bold assumption, their dishonest pretences and base subservience to popular ignorance and prejudice, until the time shall arrive when enlightened public sentiment will repudiate them.

"Our Association," says Dr. Anderson in the address before us, "has done wisely in having nothing to do with those who soil their garments with quackery; and I am persuaded that to 'let it alone very severely' is the best course to pursue with the foul thing. We but give it the notoriety it craves, and thrives upon, by any direct and public attacks upon it. To reason with the illiterate

is confessedly vain, and experience shows that education and intelligence do not guard men against its delusions. It is among the so-called intelligent classes that the various *'pathies* find their support, while the ignorant are the equally willing victims of less pretentious impositions. Unless men could be taught common sense; led to reflect how stupid it is to reject as worthless the labours of all the bright and pure minds that for over two thousand years have toiled to discover the cause and cure of disease, each taking as a starting point the spot his predecessors had reached—and, in place of their conclusions, to adopt the misty theories of some shallow sciolist, or the crude notions of some illiterate quack, we cannot cure mankind of their proclivity to empiricism. Taking men as we find them, it is the few only who are capable of judging of the merits of any system of practice, or of him who dispenses it. The multitude, with capacious gorge, are ever ready, physically and mentally, to swallow anything, however monstrous, and if they escape alive, to utter loud hosannas to what fails to destroy them. The charlatan is not slow to echo these back, and with bold words, lying certificates, pictorial deceptions, and every device the wit of those who are too lazy to work can suggest, ever draws fresh victims in his net. He is backed by the noisy many, the intelligent physician by the quiet few.

"How worse than useless then to enter the lists with such antagonists, and before such judges. Let the true votary of science go on the even tenor of his way, satisfied to do his duty to himself, his profession, and his fellow-men; assiduously studying out the hidden sources of disease, and ransacking the storehouse of nature for their remedy—assured that 'truth is mighty and will prevail,' and that he owes it to the cause of truth not to falter, or be turned aside in his investigation—to its dignity, not to provoke a contest with mean assailants; and to its public estimation, not to bring it to a trial before a prejudiced and incompetent tribunal.

"Be it ours, therefore, in our treatment of quackery, to let it alone; to touch not, handle not, regard not the unclean thing; to eschew it not only in its vulgar and flagrant forms, but '*very severely*,' also, in its secret manifestations—scorning to increase our emoluments by pandering to the diseased imaginations of those who would be gladly imposed upon by a mixture of its delusions with an authorized system—avoiding religiously all encroachments on the just rights of our brethren; depreciating them never, by word or deed, gesture or grievance; but sustaining them by commendation when due, and avoiding censure for errors we may know they did not willingly commit; in short, following ever and to its utmost extent the infallible precept, so worthy of its Divine author, of 'doing to others as we would have them do unto us.'

"It has been said by high authority that there is as much quackery in the medical profession as out of it, and with so much less excuse, that while the illiterate empiric may be self-deceived, the profession alone cannot be. But the sweeping assertion is not to be credited. I hope that, compared with true merit, quackery is a mere mote in the professional eye—the mote is still an obstacle to clear vision, and ought to be removed. This done, we then may consider whether the beam that is in the eye of empiricism, and in the great public eye, may be best cast out by direct effort, or by striving to render the merits of the profession so clear that the beam may of itself fall out, by making our light so shine before men, that they may see our good works, and gladly receive us as their 'guides, their counsellors and friends.'

The "annual oration" by Dr. W. Taylor is entirely taken up with a laboured exposition of "the eligibility of Mobile as a site for a school of medicine." The address is a well written one. The author, with commendable zeal, advocates the policy and necessity of the establishment of a medical school within the borders of Alabama, and points out the peculiar adaptedness of the city of Mobile for its location.

We see no reason why every State should not have one or more institutions for the teaching of medicine, according to its size and population, and, so far as the State of Alabama is concerned, we have no reason to doubt the eligibility of the locality indicated by Dr. Taylor for such an institution. We are not acquainted, it is true, with the facilities that Mobile furnishes for the practical

study of anatomy, or for a sufficiently extended course of clinical instruction; if such facilities do not exist, or cannot be provided, it must be evident that the location would not be a very eligible one.

We cannot, however, admit the truth of the position assumed by the orator, that it is essential the southern physician should be educated in the South, in order that he may become acquainted with the nature and treatment of its diseases. The leading principles of the theory and practice of medicine are the same everywhere. The doctrines of general pathology and therapeutics are alike applicable to the diseases of all countries and all people. The maladies incident to different climates, and even different localities, may, it is true, differ in type and character, and may demand a very decidedly different mode of treatment. These differences, however, will be soon detected by the well-instructed physician, whether his professional education be acquired in the same climates or localities in which he is called upon to exercise his skill, or in others the very opposite to them. There is but one true system of medicine, and not a different one for the maladies of the north and of the south, of the east, and of the west.

That the well educated physician—the one deeply and thoroughly imbued with the principles of the science—no matter where his education has been obtained, is qualified to detect and combat the type of disease he may encounter in the miasmatic valley, on the wide spread upland or in mountainous districts; in the crowded metropolis, or among the inhabitants of rural districts; that “the same success will attend his practice, whether beneath the scorching rays of a tropical sun, or amid the icebergs of the polar ocean; whether amid the noisome vapours of the pestilential city, or the genial and salubrious atmosphere of the country; whether enveloped in the miasmatic fogs of the alluvial valley, or existing in the light and health promoting atmosphere of the mountains;” is proved by the example of the medical corps of the army and navy of this country and Great Britain, of the army of France, and of the physicians of the East India Company; men who, notwithstanding they are educated exclusively in the leading medical schools of their respective countries, have fully shown their entire competency to contend with the diseases of all climates and all localities, and to present to the medical world the best accounts of their pathology and treatment.

The science of medicine is, in fact, deduced from the observation and experience of the leading physicians of all times, all places, all climes. It recognizes the modifications of diseased action, its character and phenomena, and the consequent modifications in its treatment, resulting from climate, season, locality and its adjuncts, as well as from sex, age, race, habits, and occupation. Were such not the case, it would sink from the rank of a science, and degenerate into a mere set of empirical rules and practices, confined in their application to particular districts, or to the people of a particular clime.

The very neat Valedictory Address by Dr. M. Troy, of Cahaba, contains some judicious remarks on the value of medical association, especially as a means of collecting and diffusing the personal observations and experience of the individual members of the profession, for the benefit of the entire body. Though each may add but little to the general fund of medical truth, yet when all present their respective quota of facts, the accumulated mass becomes invaluable.

We admit all the address says in regard to the value of personal experience in imparting tact and skill in the discernment, discrimination—in the knowledge and diagnosis, as well as in the treatment of disease—a tact and skill that cannot be acquired from books alone. But we do think that Dr. Troy has not fairly stated the true object of, and the end to be obtained from the accumulated wisdom and experience of the master minds in our profession, and has undervalued them as aids in the acquisition of medical knowledge. To the principles, the broad general rules of the healing art, its leading doctrines, its well established facts, books or lectures are to the student of medicine the only reliable guides. Through the medium of books, the physician is enabled to keep up with the constant progress towards ultimate truth—the daily improvements—made in every department of medical knowledge. It is through the medium of books

alone that "the united experience of the whole body of professional men will yearly be placed within the reach of every individual member," as anticipated by Dr. Troy, through the complete organization of "county, State, and national societies."

In one sense it is perfectly true "that every young physician must eventually form a practice for himself;" but if he attempt to accomplish this by his own unaided efforts, let his application to the task be ever so close, it will be "with much toil and with heart-burnings and disappointments, and with his soul sickened" at the fearful responsibility he has assumed. He will very quickly realize the truth of Hippocrates's aphorism, "*Vita brevis, ars longa, occasio celeris, experimentum periculosum, iudicium difficile.*"

Let no one be deceived by the dangerous advice of "trusting solely to his own observations" for the acquisition of medical knowledge and medical skill. To form the accomplished and successful practitioner, books, lectures, clinical instruction and clinical observation are all equally demanded. Nor can he, without falling back in the march of improvement, relinquish throughout life the devotion of "close application, thought, and study," as well to the diseases he is called upon to treat, as to the recorded experience to be found in the works of those whose wisdom and knowledge render them reliable guides and teachers in medical doctrine and practice.

An interesting report on the Indigenous Botany of Sumter County is presented by Dr. S. W. Clayton. To the physician, these reports would be more valuable, were it possible to embody in them some definite estimate of the true value, as remedial agents, of the several plants they describe. The reputed value of these is generally based upon evidence of a very loose and unsatisfactory character.

A report of a similar character, for Dallas County, by Dr. P. H. Cabell, follows. In this report those plants only are dwelt on "whose properties have recently been discovered, or imperfectly detailed."

Among the reports on the diseases of particular localities, that by Dr. Troy, on the diseases of Cahaba and its vicinity, is the fullest, and most interesting.

Towards the middle of April, 1854, dysentery prevailed epidemically in Cahaba. It also prevailed at different places in the surrounding country, but not in the immediate neighbourhood of the town. "The attack," says Dr. T., "was, in nearly every case, sudden, and the discharges bloody from the first; they varied in frequency from three to four a day, to an almost constant disposition to stool. They were nearly always attended with severe griping, and always with great tenesmus. In most cases the pain preceded the operation, but in a few it followed; in one or two cases, the discharge would be effected without much trouble, but the patient would be in intense pain for an hour or two afterwards. The discharges were nearly always small, frequently not more than a few drops would be passed at a time. Where they were more copious, the disease was of a severer character. The pulse was quick and weak, and the skin dry and cool, except over the belly, where it was hot—sometimes very hot. There was always retraction of the abdominal muscles, and this retraction was the best index of the progress of the case towards death or cure. In the advanced stages of bad cases it was extreme; the abdominal parietes almost touched the spine. There was not often much tenderness on pressure upon the abdomen, and what there was, for the most part, was confined to the left iliac region. In very few cases did the disease appear to extend much above the sigmoid flexure of the colon, and in only one instance could I trace the whole tract of this gut by the tenderness on pressure.

"Sickness of the stomach was not a common symptom, but there was complete loss of appetite, with great depression of spirits, and, in some cases, most intense thirst. Severe general fever was very rare, and even in fatal cases, the mind was perfectly clear to the last. The disease, when not checked, ran a protracted course, and was extremely refractory to medicines. The exact proportion of fatal cases cannot be ascertained, but the total mortality from it was greater during the past year than from any other cause. Of the seven fatal cases which occurred in town last year, death took place, in all but one, from the 13th to the 18th day of the disease. This one died on the fifth day, ex-

hausted by the constant and profuse discharge. Unfortunately, this was the only case in which a post-mortem examination was allowed. It was made by Dr. Ulmer, the attending physician, and Dr. Thornton. The colon was found intensely inflamed, all the other organs being, apparently, perfectly healthy. The liver was particularly examined; it was perfectly sound, no sign of engorgement, nor any appearance of obstructed portal circulation, which has been so often considered an essential ingredient in dysentery.

"The cause of the disease was very obscure, or rather altogether unknown. There was nothing in the weather to account for its prevalence, and we were driven to the vague explanation of an epidemic constitution of the atmosphere, or something else equally indefinite. It prevailed in certain localities only, thus leading to the suspicion of some endemic or malarious cause existing at those places. But if so, the cause was different from ordinary malaria, which has often been considered as having something to do in the production of the dysentery of warm climates. In the course of the epidemic I saw five cases complicated with intermittent fever, and in every one of these the fever yielded to quinine, without the course of the dysentery being at all influenced thereby. The remittent fever in one case might have depended upon worms, though it yielded to quinine two or three days before they were expelled, and no very marked amendment of the dysentery followed their expulsion; the patient convalesced very slowly.

"The dysentery did not effect negroes more than whites, nor did they bear the disease any worse. Children were more liable to it than adults, and in them the disease was more obstinate and severe. The youngest patient I saw was nine months old, another was only a year old; both had severe attacks, though both recovered.

"In the beginning of the epidemic the favourite prescription was, of course, blue pill and opium, or calomel, opium, and ipecac. The liver would respond with great readiness to the action of the mercurial, and we would be gratified to find, on our next visit, a chamberful of dark bilious feces, containing hardly a trace of blood; and though our patient would be complaining of increased pain, tenderness, griping, and thirst, so strong was our confidence that such discharges must be followed by amendment in dysentery, that it would not be until our subsequent visit, when we found our patient, instead of being better, with every symptom aggravated—discharging blood mixed with shreds of mucus, looking like portions of the membrane itself; his pulse increased in frequency, his tongue dry, with great thirst and tender belly—that we would be fully undeceived. It did not require many repetitions of this experience to prove that the bile, which was poured out under the stimulus of the mercury, acted as a powerful irritant to the inflamed membrane over which it passed. The same may be said of the discharges produced by other cathartics, though in less degree. Nothing seemed to do so much harm as mercury. I very early abandoned all purgatives, but they, including the treatment by salines, were fairly tried by my professional brethren, but, as they informed me, with very unsatisfactory results.

"The various astringents were tried, and though they were used by others to the end of the epidemic, under the impression of their utility, I soon abandoned them all, as either useless or pernicious. Acetate of lead was given in doses varying from one to ten grs. While it failed in any instance to check the discharges, it appeared to exert an unfavourable influence on the stomach. Sulph. zinci was no more useful, and nit. argent. failed to do any good. The vegetable astringents, kino, catechu, tannin, &c., were also tried, but abandoned as useless.

"The situation of the disease gave us great reason to hope for the best results from enemas; but, to our great disappointment, they appeared to do little or no good. They were tried of acetate of lead, sulph. zinci, sulph. cupri, nit. argent. (20 grs. to 1 oz. fluid), alum, and the vegetable astringents. A very favorite enema in my practice in dysentery, previously, had been a sufficient quantity of fresh melted hog's lard, to which laudanum or sugar of lead might be added or not. It had always appeared to act as a most soothing application to the inflamed membrane, and to supply in some measure the protection of the

natural mucus. But I cannot honestly say that, in this epidemic, it afforded any relief in a single case.

"General bleeding was resorted to in two cases by Drs. Farley and Ulmer, but they did not think the course of the disease was favourably influenced by it. Blisters were used in a few cases, but they appeared to do more harm than good. The treatment upon which I relied with most confidence, was a combination of opium and camphor, given in full doses, and repeated so as to keep the patient constantly under its influence until the disease was cured—which, if the treatment was commenced within twenty-four hours from the attack, and before any purgative medicine had been given, would, in my experience, always be in three days or less." "The griping would be allayed, and the discharges lessened, first in quantity, then in frequency, until they ceased. Sometimes the patient would have several calls to stool after they had entirely stopped. The disposition to stool was generally the last symptom which disappeared. In twenty-four or forty-eight hours after the dysentery ceased, the patient would usually have a healthy evacuation from the bowels, but I was careful not to hasten it, though the delay might be double this length of time.

"The camphor was probably the curative agent in this treatment, and exerted a specific effect upon the disease, similar to that which turpentine, which it so nearly resembles in composition, is known to exercise in some bowel affections. The opium suspended to a great degree the secretions from the bowels, and thus secured the inflamed membrane from irritation, besides allaying pain and mitigating the griping. A large majority of my patients took nothing else, by the mouth or anus, and in reviewing the epidemic now, I am confident that those did best who took nothing else whatever. I did not treat children in this way, as I was afraid to give them sedative doses of opium, and it is to the difference in treatment that I ascribe the greater duration of their cases.

"The disease ceased to be epidemic in about eight weeks after its appearance, though a case of it was now and then seen through the early part of the season.

"About the last of August, dysentery again made its appearance among us, and twenty or thirty cases occurred in quick succession. Its type differed considerably from the disease as it appeared in the spring. It more frequently commenced with diarrhoea; there was less griping, less pain, but rather more tenesmus and straining, and, in every respect it was of a lower and more typhoid aspect. The nervous system and the secretions generally were more gravely implicated. It was more a general and less a local affection than in the spring. As might be expected, it required or would bear a modification of treatment. Opium was not so urgently indicated, as there was often very little pain. Camphor was still useful, and mercury, which had been so very hurtful in the spring, was not only well borne, but in several cases seemed to exert a salutary influence. The liver was not so readily aroused by it, nor did an aggravation always follow the appearance of bilious stools, neither were there any symptoms of amendment. I still preferred to treat my cases with it, merely adding a full dose of capsicum to the camphor and opium, as I had administered it in the spring.

"We are equally in the dark as to the cause of this new outbreak of dysentery. The weather had been intensely hot and dry for a long time, and there was no change or variability in the temperature or winds about the time the first cases occurred, and during its prevalence the great change took place, from summer to fall weather, which this year, as is usual in this climate, was sudden, and attended by storms of wind and rain, and great reduction of temperature, without influencing it in the slightest degree, either for better or for worse. Like the epidemic in the spring, it gradually ceased about six weeks after it commenced; its coming and going being alike unaccountable."

In the succeeding report, on the diseases of Talladega County, by Dr. W. Taylor, we are informed that until the year 1851, the valley of Talladega was exempt from the visitations of epidemic dysentery.

"In September of that year, however, it made its appearance, marching down in a narrow strip of only a mile or two in width, through the centre of the valley. Many of the cases were severe and protracted, but the mortality

was light. In the year following (1852), it made its appearance, in a much more malignant form, on the eastern margin of the valley along the line of demarcation, between the silurian and metamorphic systems. Its influence was felt only a short distance in the silurian district, but its ravages extended several miles into the metamorphic. The greatest force of the epidemic was expended along the line, between the two systems. During this year the other parts of the county were relatively free from the disease; only a few sporadic cases occurring, and those few mild, and yielding readily to treatment. In 1853, epidemic dysentery presented itself in a most malignant form in the northwestern part of the county, where the silurian district of Talladega borders on the coal measures of St. Clair. It was here peculiarly fatal, and its visitation will long be remembered by many whom it caused to mourn the loss of friends and relatives, cut off by its ravages, alike in the bud of infancy, the bloom of youth, and the ripeness of mature age. Dysentery prevailed, however, this year to a considerable extent in other parts of the county, chiefly as a sequel to the measles, which was, also, at that time with us. It was, in many instances, extremely difficult to manage, and several deaths occurred from it, under these circumstances, after the patient seemed to have made a good recovery from the original disease."

Turning back to the report of Dr. Troy, we quote from it the following account of a convulsive disease observed by him in "very young children," and which he fears "is common and not sufficiently understood."

"I have," he remarks, "seen four cases of it this year—1854—all the patients being under a month old. They all had diarrhoea; this seemed, gradually, to become attended by severe griping and pain. The child would, at length, cry nearly all the time; sleeping only a few hours at a time, and waking with a start and a scream. It at length refused to suck, and shortly afterwards one of the starts took a well-marked convulsive form. The child bending itself back, the *respiration was either wholly interrupted or very much impeded*; the surface assumed a purple livid hue, and the extremities became cold, and the pulse almost or quite imperceptible. After a longer or shorter interval, respiration was re-established, the pulse returned, and the surface again assumed a florid hue, and the little patient lay quiet and apparently insensible, until another spasm came on, which was, sometimes, in the course of a quarter or half an hour, and at others, only three or four times a day. In one case which I attended, they came on oftener than every half hour, for about twelve hours; several times the respiration and heart's action were completely suspended for a few moments, and the child was apparently dead. Artificial respiration was resorted to whenever the natural respiration ceased, and the latter was, eventually, permanently re-established. This child, like the others, recovered under the use of large and repeated doses of calomel. In no case was there any amendment until dark green stools had been obtained, though of course a great many other remedies were tried without much if any effect. In the worst case—to which I have already alluded—I thought that bathing the child with hot whiskey perfectly saturated with red pepper, was of service in causing a powerful determination to the surface, and thus aiding, during the intervals between the spasms, in the aeration of the blood through the skin. As a revulsive, it certainly produced no effect upon the child's nervous system.

"All these cases occurred during the hot weather of last summer. Whether they have any connection with malaria or with trismus nascentium, I cannot say. It may have been merely accidental; but four cases out of the few infants born in our town, during that short period, seem to indicate a fearful degree of frequency for a disease which, dangerous or not, is certainly one of the most frightful, both to parents and physician, that we have to encounter. All of these children had taken a cathartic, to expel the meconium, immediately after birth—a practice much more common than wise, I am afraid—and had looseness of the bowels ever since its action. The duration of these cases from the first spasm to the last, varied from one to three days. The surest sign of improvement, is the ability to take the breast; when the child begins to nurse it is safe."

The disease described by Dr. Troy is evidently laryngismus stridulus. The
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symptoms as detailed above correspond very nearly with the second form of laryngismus as given by Dr. James Reid, of London, in his very excellent treatise on the disease. We would refer Dr. T., for the information he requires, to a treatise, by the writer of the present notice, on the diseases of children (4th edition, p. 346).

From the report on the diseases of Mobile for 1854, by Dr. Geo. A. Ketchum, we extract the following remarks, on the yellow fever as it occurred in that city. Every authentic observation bearing upon the question of the etiology and mode of propagation of this fearful malady is of importance to the physician. As one of the conservators of the public health, his advice on the subject of the prophylaxis of yellow fever must be based upon a knowledge of its origin, whether in local causes, and epidemic influences, or in a specific contagion coming from abroad, either in the persons of those labouring under the disease, or in fomites, whether these be found among the cargo of a vessel, or in the baggage and apparel of individuals arriving from places where the fever was prevalent when they left.

"The first death reported from yellow fever, in Mobile, was on the 7th August. The case was an imported one, having occurred on board a vessel from Matanzas; two other deaths from it were reported during the month, both of which were from abroad. In view of the excitement produced in the public mind by the importation of these cases, there was a cry for a quarantine against all vessels coming here from infected ports; therefore, on the 28th, the Mayor, having consulted and advised with the Board of Health, declared the quarantine laws of the city in force; a quarantine station was appointed in the lower bay, and the laws were as strictly enforced as was practicable.

"During September, there was much colic, dengue fever, and some cases of yellow fever. The mortality from the latter disease during the month, was twenty-two, many of these cases were from abroad; the most of them from New Orleans: many persons coming from infected places were taken sick after arriving. A good many cases originated in the city; but the disease showed no disposition to spread or become epidemic, nor do I know of a single case where the advocates of its contagiousness could find an argument to support their theory. The attacks generally were not of a very malignant character, and when they were treated early in the attack, and were so circumstanced that they could receive careful nursing and attention, the large proportion of them recovered."

"During October, dengue appeared but seldom, and yellow fever, originating in the city, was rather more frequently observed. There were eleven deaths from the disease during the month. The first frost occurred on the 5th of November, and after that malarial fevers disappeared. November and December were remarkably healthy months, the total mortality of the former being sixty-eight, and of the latter sixty-two."

"Some peculiarities which marked the epidemic of 1853, led many to suppose the disease strictly contagious. They formed their opinion upon facts observed during that epidemic alone, losing sight of many other epidemics when the disease exhibited no such properties, and losing sight too of the important facts noticeable every year: 1st, that yellow fever very often appears spontaneously where there were no circumstances strong enough to warrant even a suspicion of contagion; and, 2d, that it fails very often to appear in places which have constant communication with infected localities, though no precautions are taken to prevent its communicability. It is true, beyond the question of a doubt, that the last epidemic in Mobile (that of 1853), did present some features differing from the ordinary epidemics that have prevailed here: but for many days before it manifested its decided character, the cases of common fever were assuming a more threatening type, and taking on in their course, more and more, the features of yellow fever, until, at last, it showed itself in a decided unmixed case, in the person of a young lady on Spring Hill road, far removed from any infected locality. This was the signal note of that wide-spread, desolating epidemic. Within forty-eight hours from the appearance of this first case, there were twenty or thirty cases springing up, like mushrooms from a hotbed, in every ward or district in the city. I

saw this first case on Friday night at twelve o'clock—by six o'clock on the following Saturday afternoon, I had visited six decided, well-marked cases, each case originating in a different quarter of the city, and where it was impossible to trace a single one of them to contagious contamination.

"It is also true that the epidemic furnished more arguments in favour of the communicability of the disease than any former one—and it may be true, as my esteemed friend Dr. Fenner, of New Orleans, suggests: 'That a more malignant type of yellow fever than ordinary may now be springing up from local causes that have long been in process of maturation at this point, which may prove highly infectious.' These things may all be true of that epidemic, still the disease was yellow fever, and a multitude of facts, connected with other epidemics, impel me to believe that the fever of '53 was but an exception, in some particulars, to the general rule which governs the mode of progress in this disease, and that its wide spread havoc was the result of an extraordinary constitution or condition of the atmosphere existing at the time; and that no circumstances connected with that single epidemic prove that *contagion* is the *only or even the usual method* by which this disease traverses from point to point. If such were the fact, we would every year or two see a repetition of the sad scenes of '53—for a disease that is contagious one year is so the next, and ever after—and the many towns and villages in constant communication with New Orleans or Mobile, would have been often scourged as they were in '53; but such has not been the history of the past, and reasoning from that, we may safely hope that such will not be the history of the future. The fact that this epidemic scourged severely so great an extent of the South and Southwest, extending its ravages all along the sea-coast of Texas—up the banks of the Mississippi, from its mouth to Napoleon, at the mouth of the Arkansas River, devastating the inland towns and plantations of Louisiana and Mississippi, and many of those of our own State as high as Montgomery, on the Alabama, is another significant fact, for, if this were the work of contagion, why is it that so often, when the communication between New Orleans on the one side, and Mobile on the other—whilst suffering from the visitations of this disease—with the inland towns and villages, was much less interrupted than during this fatal year, these places are not scourged by the fever as they were this season? Does not this warrant us in drawing the inference that the agent of its propagation from place to place was not contagion—but that there existed throughout the district an unknown constitution of the atmosphere, arising, most probably, from malarial exhalations, which fostered the disease and invited its approaches from one point to another? Upon this theory only can we account for those cases which occurred spontaneously in districts where the disease was unknown before, and where no communication with infected points could be traced, and it is only in this way that you can account for the fact, that, during other epidemics of this disease, places in constant communication with New Orleans or Mobile have not suffered at all from the disease."

The concessions and reasoning of Dr. Ketchum in favour of the doctrine of the non-communicability of the yellow fever from the sick to the well, are the more forcible, inasmuch as they were evidently forced upon him by the strong evidence of facts, notwithstanding his apparent bias in favour of the untenable supposition that the disease may occasionally or contingently assume a contagious character.

From this same report, we learn that, during the months of June and July, 1854, attacks of colic became "fearfully frequent" in Mobile.

"They occurred, to a greater or less extent, in all parts of the city—but the western portion seemed to be more afflicted than any other. These attacks of colic were characterized by the following symptoms. After a general feeling of *malaise*, lasting a longer or shorter time, the patient would be seized with pains of an intense and persistent character, at or about the umbilicus; generally there was much nausea, and if vomiting occurred it was, after the usual contents of the stomach had been discharged, of a green watery fluid, or, at other times, of a thick tenacious mucus, tinged with bile; there was some thirst, much restlessness, and obstinate constipation; many patients that I

saw complained of much oppression in breathing, and of intense pains at times shooting through the arms and legs. The cases usually ran through a painful course of from three to seven days in spite of treatment—the free evacuation of the bowels was generally followed by entire relief—though I saw some cases where much pain and uneasiness were felt for many days after the more violent symptoms had been relieved. Patients who had had the disease during former seasons, or who had been once attacked this season seemed more prone to a second or third attack, each yielding with more difficulty than the one preceding it; and after repeated attacks, the patient was generally left in a deplorably decrepid condition, with partial or entire paralysis of the arms and legs, and a blanched and sallow hue of countenance. The first approaches of this paralysis usually manifested itself by a trembling of the hands, a want of proper control over the extensor muscles of the arms, a trifling numbness of sensation, and in some cases a wasting of the muscular fibre. I noticed that those patients who complained most of the pains through the limbs, the arms particularly, were more liable to be attacked with these symptoms. This paralysis has proved very obstinate under treatment—where improvement has occurred, it has been slow and tedious. Strychnine, electricity, and a change of climate and of *water* seem to have acted most effectually in affording relief.

“I have long felt convinced that many of these cases, if not all, resulted from *lead poisoning*; and these convictions have been forced upon me by the following facts. The water furnished the citizens of certain portions of Mobile, is carried through eight miles of main pipes of lead; all the service pipes in the city are of lead. If it be possible for the water to become impregnated with this deleterious article, assuredly there is an abundant opportunity here afforded. The attacks of colic occur most frequently in the western parts of the city, where the water furnished from the city waterworks is carried for the greatest distance through lead pipes. From the inadequate supply of water, the pipes in this most elevated portion of the city, are very frequently, partially, or entirely empty, and are thus left to the action of the atmosphere. The same may be said of the service pipes in the lower part of the city, on the occasion of any considerable use of water for fires, or for the purpose of cleansing the gutters. Again, the symptoms of these attacks are, in the main, identical with the attacks produced by well attested cases of lead poisoning, and the treatment adapted to the management of lead colic, acts more effectually in relieving the attacks than any other.

“The subsequent palsy can only be accounted for, on rational principles, by ascribing it to the action of lead poison; as in other cases of lead poisoning, the palsy attacks most frequently the extensor muscles of the upper extremities. These are facts worthy of attracting the attention of medical men, and I hope that more particular attention and more careful investigation may be given to this subject, by those more competent to the task than your reporter.”

In addition to the reports already noticed, the *Transactions* of the Alabama Medical Association contain others of an interesting character, among which we would include those on surgery, by Drs. Gaines, Nott, and Taylor. There are, also, other papers, as, an account of a carcinomatous tumour of the abdomen, by Dr. W. C. Hicklin; on the treatment of dysentery, by Dr. F. E. Gordon; and on metastasis of disease, by Dr. A. Lopez, all of which will repay a careful perusal.

2. The *Transactions* of the Medical Society of the State of New York open with the annual address of the President, Dr. C. B. Coventry. The subject of the address is the philosophy of medicine as a science and an art. Which is most ably discussed and enforced.

Dr. Coventry defines medicine as “that department of natural science, which treats of the means of preventing diseases and of removing them when actually present.” Medicine, he contends, “is both a science and an art, both having the same great object, the amelioration of human suffering, but still separate and distinct. That the *science* of medicine, depending as it does on the fixed laws of creation, is unchangeable, whilst the *art* of medicine, depending on the skill and tact of the physician or surgeon, must vary with each particular case.

The term science means knowledge, and has been defined as knowledge reduced to principles, whilst art is defined as knowledge reduced to practice."

He endeavours to show that medicine, as a science, is a department of natural science, governed by the same rules, and to be investigated in the same manner. That, although more difficult of investigation, its facts are just as certain and unchangeable as those of chemistry, natural philosophy, or astronomy.

"Whilst," says Dr. C., "it is undoubtedly true that medicine is a department of natural science, consisting in an investigation of laws established by the Creator, to be studied with the same care and caution as other departments of science, we are compelled to admit that it is the most extensive, and at the same time the most obscure and difficult of all the branches of natural science. The chemist can retire to his laboratory, and in silence and alone test the truth of his discoveries. If he hears of discoveries made by others, he has but to submit them to the test of the crucible and blowpipe. The natural philosopher can force nature to the test, and compel her to reveal her secrets. The physician has no such power; he is compelled to wait in patience nature's own time, until the opportunity offers, and then, instead of an inanimate mass, which may be moulded and tested at pleasure, you have a living, moving body, which is constantly changing, never stationary, animated by a spirit, and where all the ordinary laws which govern matter are disregarded or removed."

The science of medicine has not merely to contend with the difficulties incident to the correct appreciation of the complex structure of the human organism, and the subtle and intangible agents by which it is animated, and its various functions are directed and controlled; to the correct analysis of the phenomena of life in their normal and abnormal manifestations, but it embraces by far the widest range of human knowledge of any of the liberal arts or sciences; in its investigation of the agency of external influences, whether salutary or noxious, upon the living body, and in applying or avoiding them, so as to produce the desired result, either of preventing the occurrence of disease, or of converting the state of disease into that of health, all other sciences, almost every other species of human knowledge becomes subsidiary to that of medicine, or, perhaps more correctly, they constitute so many of its essential elements. Etiology and therapeutics are as much based upon a knowledge of modifications produced in the living organism by external agencies in its healthy and diseased conditions, as upon a knowledge of its structure and vital laws, and in the extent and correctness of our knowledge of these agencies will depend our acquaintance with the causes, prevention, and cure of disease.

So much for medicine as a science: as an art it applies the principles of the science—the deductions of knowledge, to the prevention and cure of the abnormal conditions of the vital organism, the alleviation of human suffering, and the prolongation of human life.

Were we intimately acquainted with all the external influences to which the human organization is subjected in all climates, seasons, places and conditions, did we understand fully the effect of their action upon the several organs of the body, our power to prevent disease would be augmented. And could we calculate with the same certainty as do the chemist and natural philosopher the agents with which they operate, the effects of remediate agents upon the diseased conditions of the human system, our success in the removal of those conditions would become more certain; unfortunately, however, there are numerous contingencies interfering to modify the operation of our remedies, which we are not always able to anticipate or correct. Hence, the proper administration of medicine as an art, it must be evident, requires not merely sufficient scientific attainments, but a natural capacity for observation and analysis, as well as much judgment, tact, and experience.

Besides the annual address, just noticed, we have another by Dr. J. McCall, of Utica, on the needs, duties, and privileges of the medical profession. With many good ideas and suggestions, the address is defective in its arrangement, and deficient in point and precision.

The first of the strictly professional papers is a very able and voluminous

report on dislocations, with especial reference to their results, by Frank H. Hamilton, M. D., of Buffalo.

The cases given, embracing examples of nearly all possible dislocations, with their results, in connection with the remarks of the reporter, are both interesting and eminently instructive. They will demand the close attention of the younger surgeon especially, affording to him a valuable mass of statistics from which much may be learned calculated to lead to correct practice and a cautious prognosis.

A useful analysis of the report could not well be given. We shall present to our readers the introductory remarks from which they will acquire an idea of the general character, scope, and design of the paper.

"It is equally true of dislocations, as of fractures, that neither general treatises upon surgery, nor monographs, have informed us sufficiently as to the results of these accidents. There is no lack of excellent instruction in the rules of diagnosis, or in the laws of treatment; but of the frequency of non-reduction and its consequences; of the amount and character of the maiming which is likely to ensue when the reduction has been effected, and the length of time during which such maiming may be reasonably expected to continue, there is, I think, a palpable deficiency of information. The young surgeon experiences, therefore, a constant embarrassment when interrogated as to the character and sum of the injury which the patient is to sustain; and what is more unfortunate still, I fear, if he entertains any opinions on the subject, they are in general quite too favourable.

"It is not difficult to see how the utterance of a favourable prognosis, which the result does not confirm, will lead to a distrust of the skill of the surgeon, if it does not establish in the mind of the sufferer a conviction of malpractice. But still more certainly will the patient accuse us of ignorance and unskillfulness if, in any case of simple dislocation, we fail entirely to reduce the bone. It will be in vain now that the mortified surgeon shall apply to his printed volumes for justification, since he will seldom find any mention of failures or of unfortunate results, and I suspect his neighbours will not have any cases so unlucky as this one, with which to countenance and console him.

"In this view alone the subject of 'results' in dislocations is of sufficient importance to demand our attention. But there is another view in which it may be claimed to possess equal, if not greater interest. If it shall be found that, other things being equal, certain results more or less grave pretty uniformly follow upon certain modes of reduction, we may hereafter choose with better judgment between the different procedures where more than one plan has been recommended, and be led to devise new modes where no choice exists; and, moreover, I need scarcely add, that we shall be better able to declare what of the maiming is intrinsic to the accident, and what is justly attributable to the treatment.

"Willing, as far as possible, to contribute my limited experience to the elucidation of these points, I have collected with care, from my private and hospital records all the cases of dislocations which have passed under my observation, and of which I have notes, during a period of twenty-one years; and I have not only written them out at length, but I have also arranged them in a more abridged and tabular form, for the greater convenience of reference. It is proper, and perhaps only justice to myself, that I should advise you, gentlemen, that the report includes a pretty large proportion of cases in which I have not acted as the operating, but only as consulting surgeon; and that in a few of the cases, also, I have been merely permitted to examine the limbs, with a view to the ascertainment of the results, and without any intention on the part of the patient of asking counsel, or of obtaining surgical services.

"While, therefore, I have made no attempt to hide my own shortcomings, I have taken as little pains to conceal the shortcomings of others. I presume that no surgeon would confess to so large a proportion of unfortunate or imperfect results as these records supply. Such a confession would certainly imply unskillfulness on the part of the surgeon himself, or a degree of imperfection in our art which I am by no means prepared to admit. In short, I have only pro-

posed to give you a faithful transcript of what has, during a period of years, come under my actual notice; so far, at least, as my notes enable me to do so.

"The 'remarks' appended to the several classes of cases are intended as an analysis, and also to convey to you a few practical hints and original suggestions, which arise naturally from the cases themselves."

The remarks referred to are replete with interest, and in connection with the cases to which they have reference, present, in general, an important practical bearing. To the young surgeon the entire report cannot fail to prove eminently instructive.

In a short but sensible paper, Dr. ALDEN MARCH describes an improved forceps for harelip operation, which is designed to combine the semicircular line of the Edinburgh surgeon, and the angular line of Malgaigne. (See FERGUSON, *Principles of Surgery*.)

The construction of and mode of operating with this instrument are illustrated by several drawings, without which neither would be well understood.

The paper concludes with some judicious remarks on some of the various means employed to secure and maintain the fresh cut borders of the divided lip in apposition.

A case of injury of the shoulder-joint, of some interest, is related by W. S. NORTON, which would be more correctly described as a paralysis of the arm occurring after dislocation and continuing upwards of ten months subsequent to complete reduction of the shoulder-joint. The paralysis was entirely relieved by free, daily motion of the limb, continued for some weeks.

The *Transactions* of the New York Society contain three reports on epidemics. On those of the second, third and sixth senatorial districts of the State; respectively by Drs. J. C. Brinsmade, G. C. Monell and A. Willard.

The last of these contains a notice by Dr. Avery of an epidemic of erysipelas which occurred in the northwest portion of Venango County, in the early part of 1854, and an account of the autumnal fevers of Norwich, by Dr. A. Baker, Jr., which had during the past year run into an apparently typhoid type. We copy the remarks of Dr. Avery on epidemic erysipelas, a disease which has of late years prevailed to a considerable extent in different portions of the United States. Of its etiology, pathology and proper treatment, we have yet much to learn.

"In the months of February and March, 1854, there occurred in the Otsetic Valley and on the adjoining hills in the towns of Otsetic and Pitcher, for the first time, an epidemic erysipelas, more fatal in its effects than any previous visitation known to the oldest inhabitants. The number of deaths from this disease, according to the best of my information, was about forty. It commenced in south Otsetic and spread rapidly through the section of country already mentioned. The Otsetic River valley through this section has a rich, loomy and gravelly soil, generally dry and free from marshes. The hills that border it are productive and dry, the soil having a loomy appearance. The water is soft on the hills, but hard in the valleys. We have no limestone—the rocks being generally composed of brown or blue slate. The whole section of country is generally healthy, and remarkably free from fevers or other epidemics, except dysentery, which occasionally appears. This epidemic (erysipelas) in several well marked instances, appeared to spread by contagion, but many cases were not traceable to that source.

"The winter, in the latter part of which the epidemic occurred, was remarkable for the small quantity of snow which fell, and for extremes of wet and cold, following in quick succession, on days that were comparatively warm. Exposure to cold was in very many cases the immediate exciting cause of the disease. Fatigue and debility strongly predisposed to, if it did not immediately induce it. The attack, preceded by sore throat, which presented a scarlet or yellowish red appearance, commenced with cold chills, accompanied by intense pain in the neck, back, and limbs; the pain continuing in some cases two or three days, followed in most cases by high inflammatory fever, in the progress of which various phenomena appeared. All cases had a decided inflammatory diathesis, and the blood, even in the mildest forms of the disease, so far as I observed, was buffed, and cupped on standing a short time. Quite a number

had swelled face and head, which, though enormous, did not suppurate. Many had swelling of the axillary glands, which slowly suppurated. A few cases appeared to be purely phlegmonous, the disease attacking the limbs with which it made sad havoc. But the most remarkable feature of this epidemic was the tendency to development of local inflammation of the erysipelatous kind in internal and vital organs differing from all other forms of inflammation, in the rapid destruction of the parts. Of the forms of local inflammation, peritonitis was most fatal and frequent, suppuration and death following in about forty-eight hours from the first appearance of pain or tenderness; and what is important to be remembered, most of the attacks of peritonitis had been preceded by the domestic administration of some cathartic, such as common salts. That form of local inflammation which occurred next in frequency, was pneumonia, which, though an alarming feature, was more manageable than peritonitis. In a few cases, phrenitis supervened, and was rapidly fatal. Laryngitis was fatal in one case. Even mild forms of the disease, where the departure from health was but slight, were liable to sudden attacks of some of these forms of local inflammation. Those cases in which there was swelled face were certainly not more likely to terminate unfavourably, than those in which there was no external erysipelas. The time required to develop the disease after exposure to contagion, I cannot fix, but think from eight to twelve days.

"The treatment found most successful was such as might be inferred from the febrile state of the blood; heroic bloodletting and mercurialization. None died who were mercurialized, and but few who were thoroughly bled early in the disease. Much time was lost in temporizing with those idiotic empiricisms, so strongly recommended of late in erysipelas; such as tincture of iron, nit. silver: but, after the old fashioned antiphlogistic treatment was adopted, few lives were lost, and many malignant cases were saved."

The report from the second district, contains an account of epidemic dysentery as it prevailed in the town of Vernon, Sussex Co., New Jersey, bordering on Orange Co., N. Y.; the history of two cases of dissecting aneurism; the case of a maniac, with the post-mortem examination, in which death was caused by the patient swallowing an old fashioned pair of silver spectacles, a silver teaspoon, and the large crank or key of a clock, which were found in the cesophagus after death.

The report from the third district is made up entirely of a very full and admirable history of the cholera epidemic of 1854, at Troy, N. Y., by W. P. Seymour, health officer.

Accompanying this history are comprehensive meteorological tables for the months during which the epidemic prevailed, and a map of the city, showing the localities in which the disease existed. The list of cases are given as nearly as possible, in the order in which they occurred, preceded by a very good account of the medical topography of Troy and its environs.

The remarks appended to the cases are ingenious and eminently suggestive.

The reporter remarks that the specific cause of cholera will, probably, never be known, but he thinks himself warranted in saying, that various as are its predisposing causes, such as filth, bad ventilation, insufficient food, bad water, &c. &c., "the most uniform exciting cause is moisture." And he feels as if the facts set forth in his narrative of cases, "would almost justify the assertion that the hygrometer is as good an indicator of its activity as the barometer is of storms, and certainly a far better prophet than the flight of insects, the tests of ozone, and divers other mysterious signs and portents we read of."

"Whether," he remarks, "humidity exerts its power in disengaging and making more active the noxious gases which ordinarily surround us, or by checking perspiration, and the exhalations from the lungs, thereby causing the accumulation of a subtle poison in the system, or by its well-known influence on the *nerve force*, are questions which time, experiment, and patient observation can alone answer. A solution of these questions would go far to determine the nature and laws which govern this mysterious agency."

Although we have not been furnished with an accurate and reliable account of the hygrometric condition of the atmosphere in all the localities where cholera has prevailed, we are in possession of a sufficient number of facts on this

point to show that, although certainly a most potent exciting cause of the disease, moisture cannot be ranked as "the most uniform."

The reporter informs us that, in the commencement of the epidemic, he was strongly disposed to believe in the contagious character of the disease.

"But," he remarks, "a careful consideration of the cases bearing on this question satisfied me that there are none which cannot be explained by the influence which excitement, fatigue, and fear undoubtedly have in bringing the subject of them within reach of the cholera poison already acting on them. The most that can be claimed as proved, is that isolated cases *intensify* the action of the poison in localities already more or less under its influence. I know of no cases which clearly prove the communicability of the disease in an unquestionably healthy locality."

We wish the reporter had taken the trouble to explain to us in what manner he supposes isolated cases intensify the choleric poison in infected localities. Is it in any other manner than by the debilitating influence of the excitement, fear, and, perhaps, fatigue, which the presence of an isolated case of cholera produces in a family or neighbourhood where it makes its appearance, bringing at once under the influence of the existing atmospheric poison the systems of those who had hitherto withstood it?

To the question, Is the disease always preceded by diarrhœa? the reporter makes the following reply, which we believe to be, in the main, correct:—

"There are occasional cases occurring, in which it is indisputable that no diarrhœa or even vomiting exists, not only prior to the full development of the attack, but up to the fatal termination. There can be no doubt, however, that the *vast majority* of what I call *initial* cases are preceded by a diarrhœa which affords sufficient time for arresting the disease; but I very much fear that a careful investigation and analysis of the cases occurring here and elsewhere would show that there is quite a respectable number of cases which may be called secondary, *i. e.* depending on the initial, and which could not have occurred without them, which are not preceded by diarrhœa, or, if preceded by it, the diarrhœa, the partial collapse, and the pulseless stage follow each other so rapidly as to make but little difference in the result, whether treatment is commenced in the one stage or the other."

In respect to treatment, in the early stage of the attack, it was found that small doses of calomel and opium, with or without camphor, and perfect rest, seldom failed to arrest the disease. In the stage of collapse, the reporter states that, from his own experience, he is fully persuaded that camphor, in large doses frequently repeated, is the most powerful agent he has yet seen employed.

"I have," he says, "never seen it relied on alone, but always in conjunction with calomel and opium; but I am persuaded that the number of recoveries from the most profound collapses have been much more frequent when this constituted the only difference and treatment, than in 1849. Dr. Clarke, the one who first called my attention to this mode of treatment, was in the habit of giving one drachm or two drachms of the officinal tincture every fifteen or twenty minutes, according to the severity of the case. In desperate cases, even four drachms would be given. These doses were persisted in until symptoms of reaction appeared, and then gradually lessened, or the interval increased, until all danger of relapse had ceased. The quantity given in this way, in some of his successful cases, has been twelve or sixteen ounces. I have myself given somewhat over half a pint to a patient in the course of twenty-four hours. He was pulseless for six or eight hours, and nearly so from fifteen to twenty-four. He recovered without a single bad symptom, or indeed any unpleasant symptom, which I have not observed in cases variously treated."

The remaining papers in the present volume of *Transactions* are a case of extra-uterine conception, by Dr. Parkhurst, with some interesting remarks by Dr. Armsby. The account of a fatal result from the removal of a tumour from the neck, whilst the patient was under the influence of chloroform, by Dr. Alden March. On the employment of injections into the bronchial tubes, and into tubercular cavities in the lungs, by Dr. Green, and biographical sketches of the late Professor James Webster, M. D., of New York, and of the late Dr. Daniel Ayres, of Amsterdam, Montgomery County, N. Y.

3. The *Transactions* of the Medical Society of Pennsylvania, at its session of 1855, contains reports from eight county societies, a report from a special committee, and the annual address of the president, Dr. Jacob M. Gemmil.

Considering the size and population of Pennsylvania, it is to be regretted that so few of the counties are represented in its State Medical Society, and even of those represented at the last session but two-thirds should be prepared with reports in reference to the medical history and prevailing diseases of their respective localities.

In certainly more than twelve out of the sixty-four counties of the State, a sufficient number of physicians can be found who can appreciate the important results to be obtained by the effectual organization and co-operation of all the members of the medical profession throughout the State, for the cultivation and advancement of medicine as a science and an art—a sufficient number who desire to see the boundaries of our science enlarged, and its direct application to the prevention and cure of disease rendered more certain and effective, and who are fully alive to the fact that this can be effected only by a combined and uniform effort to collect, analyze, and perpetuate the observations and experience of the individual members of the profession, and render their knowledge a part of a common fund, upon which all may draw. There are certainly, too, a sufficient number among the physicians resident within the delinquent counties, who are animated by that love for their profession and that *esprit du corps* which would prompt them to lend their aid and influence in the furtherance of every means calculated to effect the important result above indicated.

We beg of such to peruse with care the sensible, pertinent, and well worded address of Dr. Gemmil, in the volume of *Transactions*. We feel persuaded that his exposition of the importance of the complete and systematic organization of the medical profession throughout the State, and the leading objects to be achieved by such organization, will induce them, if not for the promotion, simply, of their own private interests, at least for the promotion of those of their brethren and the good of society generally, to exert themselves in the cause of medical organization; so that at the session of the State Society, in 1856, at least one-half the counties will be represented, each one of them sending up its quota to enrich the general fund of professional knowledge, and increase the amount of professional skill.

The several reports from county societies, comprised in the volume before us, present very interesting accounts of the prevalent diseases of various sections of country, affording useful hints in reference to the history, character, and treatment of many of the maladies the physician is called upon to treat, and affording, in many instances, the means of testing the influence of locality and local conditions and occupations upon the character and prevalence of particular diseases.

The fullest and most elaborate of these reports is that from the Medical Society of Philadelphia County. The introductory portion of this report is particularly valuable and interesting. That, especially, showing the meteorological conditions of a series of years, and the comparative mortality of Philadelphia with that of London, New York, and Boston.

Appended to the report from Lancaster County is one from Dr. J. L. Atlee, from Lancaster city, on the epidemic of cholera which prevailed there in the months of August and September, 1854. The object of the reporter is "not to give a particular description of the disease, but to refer more especially to its etiology."

From the various facts adduced in this report, Dr. Atlee believes that but one conclusion can be arrived at, and that is that the cholera was introduced into Lancaster as well as into the town of Columbia, Penn., by a specific contagion eliminated by the bodies of persons labouring under the disease, and that its subsequent propagation was the result of a similar contagion.

We feel somewhat surprised that a medical gentleman of the standing and acknowledged professional acquirements of Dr. Atlee, should have been led to such a conclusion from the few imperfectly observed facts, or, at least, imperfectly reported facts, to which he directs attention, and upon which he bases his conversion to the doctrine of contagion in cholera. Strictly analyzed, the

facts presented, even with the aid of "it is said," "may have been," "in my opinion," and other helps used to eke them out, afford no very striking, certainly no conclusive evidence of the origin and propagation of the cholera from contagion in either Lancaster or Columbia.

In Columbia, some weeks before the outbreak of the epidemic, cases of the disease, several of them fatal, occurred. We have no right, in the absence of testimony, and in reference to a question so momentous in all its bearings as that of contagion, to suppose, as Dr. A. has done, that the disease, in these first cases at Columbia, was contracted from emigrants labouring under it. That the spread of the disease in Columbia was not due to a specific contagion, but to strictly local causes, we can hardly imagine any one, who has carefully studied the history of the epidemic there, will attempt to argue.

But in Lancaster, we are told, the disease in the hospital and almshouse broke out after the introduction of cases from abroad. But we are furnished with no facts by which the subsequent cases can be traced to the contagion emanating from the former. Was there a free intercourse allowed between the cholera patients and those of the insane wards, among whom the disease prevailed with so much malignity? Was not the disease, in the latter patients, the result of the moist, warm southerly winds, blowing into their sleeping apartments directly from the river, and the imperfect ventilation of these apartments the result of faulty construction?

From whence originated, two weeks subsequently, the disease in the almshouse? Had the old inmate, who was the first attacked, been in contact with the cholera patients in the hospital? These wanting links in the chain of evidence must be supplied by direct evidence; we will take nothing for granted in a case like this.

Now, we think it will appear very certain from the facts adduced by Dr. A., that the epidemic influence, whatever that may be supposed to consist in, existed as well at Lancaster as at Columbia. Although, from the salubrious location of the former city, and its freedom from any very prominent source of malaria, it felt but lightly its influence.

The isolated cases which occurred in the city were evidently brought on by anxiety, fatigue, grief, and, perhaps, undue exposure, and not from contact with fomites imbued with contagion, or from the contagion adhering to persons who had come from Cleveland, Ohio.

The fact admitted by Dr. A. that the disease was not communicated to others in the same house or family, or neighbourhood, by these isolated cases, is an evidence of the non-contagiousness of the disease. We cannot admit that the contagion was disarmed of its efficiency by the favourable external circumstances in which the patients and those surrounding them were placed. We are acquainted with no other disease of an unquestionably contagious character, the spread of which from the sick to the well can be thus prevented.

A very judicious report on Forms for County Reports is presented by a committee appointed to take the subject into consideration at the last session of the Society. The suggestions as to the character of the subjects of the reports sent up from the county medical societies to the State Society, are well adapted to render those documents more uniform and better adapted to the important object they are intended to fulfil—the contribution of accurate personal observations and facts in relation to the endemics and epidemics of particular districts and localities, with all the concomitant circumstances essential to give force and value to the observations and facts recorded.

We trust that all the county societies now organized, as well as those whose speedy organization we confidently anticipate, will, in their several reports, adhere to the forms recommended.

In relation to the number and the classification of the items of information to be embraced in these reports, the committee remark:—

"These, it is believed, ought to be so comprehensive as to secure a luminous exposition of the hygienic and medical condition of every county in the State, however varied it may be. This object is not to be gained so much by the presentation of the opinions of the county reporters, as by the exhibition of the reasons upon which these opinions are based; not by the discussion of chemical

and medical theories, but by the careful collection of facts. Opinions and doctrines may, with more propriety, be expressed on the pages of our medical journals, where they can be reviewed, advocated, or opposed through the same medium which gave them publicity. Nor is extreme conciseness in the returns to be recommended, as thereby many valuable suggestions may be suppressed. Your committee, in view of the foregoing, have decided to prepare the blanks in the form of topics, rather than in that of questions.

"These topics naturally arrange themselves under three heads:—

- I. Causes which modify the health of the county.
- II. Mortuary tables.
- III. Prevalent diseases.

"I. Causes which modify the health of the county.

"1. Locality.—Boundaries of county, and its situation in the State. Its proximity to large rivers and the lakes; to mountain ranges; their direction and extent.

"2. Hydrography or Drainage.—Size and direction of watercourses, and of the dividing ridges or watersheds. Extent of river bottom. Power and rapidity of streams. Quantity of marshy or springy soil; artificial water-channels and reservoirs; amount of lockage; danger and frequency of inundation.

"3. Topography.—Area of county. Population, and their lineage; chief avocations. Location and size of principal towns. General character of surface. Principal valleys and eminences. Nature and extent of surface destitute of vegetation, tilled, covered with forest, or in grass. Agricultural produce. Kinds of timber; effects of clearing and of drainage on climate. Extent of artificial irrigation.

"4. Geology.—Geological position, actual location, dip, direction, and extent of the different formations. Source and nature of water used for domestic purposes. Character of soil, subsoil, and subjacent rock. Supply of water to towns; kind of pipe used as conduits. Map with geological features named and coloured, agreeably to the order adopted by the State Geologist.

"5. Meteorology.—Latitude, longitude, and altitude of observer. Barometric, thermometric, and hygrometric states of the atmosphere for every day in the year. Rain, snow, indicated in columns, as per blanks furnished by the Franklin Institute (a copy of which is appended to the report). Observers for this Institute, as well as for the Smithsonian Institute, reside in several of the counties, and would furnish all the local meteorological information required.

"II. Mortuary tables. (These, where obtainable, ought, for the sake of uniformity, and from the merit of the classification, to be made to correspond with those published by the College of Physicians, Philadelphia.)

"Mortality from fevers, from measles, smallpox, and varioloid, from diseases of the lungs and air-passages, of the nervous system, of the organs of nutrition, of the genito-urinary organs. Causes assigned for death where the number exceeds 10 per cent. of the whole. Quarterly tables, showing the whole number of deaths of white and of coloured persons, under 1 year, from 1 to 2, from 2 to 5, from 5 to 10, from 10 to 15, from 15 to 20, and for every decennial period over 20.

"III. Prevalent diseases.

"Epidemics and endemics of the year; their origin and march; apparently contagious or not; how affected by race, age, sex, temperament, avocation, circumfusa, ingesta, and density of population.

"Intermittent and remittent fevers; their frequency, as compared with that of former years.

"Typhus and typhoid fevers. Smallpox; benefits of vaccination. Measles, Scarlatina.

"Peculiar features of any of the ordinary diseases.

"Miscellaneous items on the etiology, pathology, and therapeutics of any diseases observed.

"In describing cases, give the age, sex, condition, and location of patient, season of year, treatment, and termination.

"On the geological map, inclose by dotted lines, with proper marginal references, those sections of the country in which epidemics have prevailed."

Added to the present volume of *Transactions* is a very full index to the subjects embraced in the several reports from the county medical societies, presented during the five years from 1850 to 1854, inclusive. Such an index greatly enhances the usefulness of those reports, by facilitating a reference to the materials they embody by any one engaged in the investigation of subjects to the elucidation of which they may be made available. It would be desirable to have the transactions of all our State medical societies furnished with a similar index.

D. F. C.

ART. XIV.—*The Cause and Prevention of Yellow Fever, contained in the Report of the Sanitary Commission of New Orleans.* By E. H. BARTON, A. M., M. D., Chairman of the Sanitary Commission; President of the Louisiana State Medical Society, and of the New Orleans Academy of Sciences, etc. Philadelphia: Lindsay & Blakiston, 1855. 8vo. pp. 282.

WE have had lying upon our table, for some months past, the *Report of the Sanitary Commission of New Orleans*, and were in hopes to have been enabled to present before now a notice of the very interesting matters embraced in it.

The present work is a reprint of that report, with prefatory remarks, and, as a supplement, a paper read before the Academy of Sciences of New Orleans; the object of these additions being to "fortify the positions taken in the report itself, to extend its illustrations, and to give further explanations of portions of it which have not been so fully understood as they might have been."

The entire volume is one replete with matter of deep and absorbing interest. The views advanced in it, in relation to the cause and prevention of one of the most destructive and appalling endemics of certain of the cities of our Union, are deserving of a careful and candid examination. They are far from being visionary and hypothetical, but, on the contrary, are apparently the legitimate deductions from well-attested facts and a series of reliable observations.

That the yellow fever requires for its production the concurrence of particular local morbid causes with certain meteorological conditions, is now, we believe, pretty generally conceded. Though it is still a matter of dispute as to the exact character of the former, and of some, at least, of the latter. In the attempt to settle this controversy, and determine what is the nature of the terrene and atmospheric agencies to which yellow fever owes its origin, Dr. Barton has come forward with the aid of his personal observations and investigations. And certainly no one has had more ample opportunities for investigating the etiology of "the dread pestilence," or has more industriously, and, as we believe, more successfully, cultivated those opportunities. Although he may not always present his facts and arguments and deductions in a style exactly suited to a strictly scientific discussion—although certain of the forms of expression he adopts may be esteemed loose, and liable to be misunderstood—we are content to receive the truths he utters, without stopping to criticise a few defects we may observe in the dress in which those truths are set forth.

It will not be in our power to enter upon a formal analysis of the doctrines advanced in the volume before us, in relation to the cause and prevention of yellow fever, nor a comparison of these with the facts and illustrations adduced in their support. We would merely remark that the leading facts would appear to be sufficiently verified, and the illustrations legitimate and apposite.

The following are the general conclusions of the author, as set forth in the ninth section of the report. From these our readers will be able to form a correct idea of the views of Dr. Barton in respect to the etiology of yellow fever, and the principal data upon which those views are based.

"The duty," he remarks, "of tracing the outbreak of the fever—its origin and its transmissibility—has, in the division of duties, devolved upon my col-

league, Dr. Axson, and most ably and graphically has he performed the task; clearly demonstrating that it was not from foreign importation that it was derived, but, although connected with foul ships from European ports, that it was due to domestic birth and growth, whether at the levee or elsewhere; and that, at its divers origins, there was no necessary connection the one with another. Now it becomes my duty, under the resolution, in exposing the sanitary condition of the city, to show what and where those causes were. They have before been referred to in general, wherever it has been attempted to demonstrate their applicability, the influence of such causes in similar and in different climates, their direct bearing upon former epidemics, and their influence in the rural districts; and I now proceed to show their special influence in the production of the late epidemic.

"The causes assigned were twofold, and these formed the constituents of the epidemic—1. *Meteorological*; and 2. *Terrene*.

"To the first belonged—A. A long-continued range of tropical temperature preceding the outbreak. The average at midday, of the two preceding months of May and June (instead of being a month later), being nearly 83°, and which continued throughout the epidemic. B. An unusually high hygrometer, which continued and increased, exhibiting an almost saturated atmosphere. C. Heavy rains. D. Unusually high and distressing radiation. And E. An unprecedented intensity and continuance of stagnant air. The unusually early establishment of this *tropicoid* condition, in the elevation of winter temperature to that of spring, and of spring to that of summer, thus anticipating by more than a month the usual evils of autumn, with an aggravation of the burdens ordinarily incident to it, with the extraordinary combination of those which preceded them, were the main *atmospheric elements* which composed it. These are stated in detail in the tables, and for the three epidemic months four or five times daily.

2. The *terrene condition* was composed—A. Of the upturning and exposure of the original soil, in the cleaning out of the canals Claiborne, Carondelet, Marigney, &c.; the immense exposure in making a new basin on Bayou St. John; digging on St. Paul Street to Bayou St. John; digging ditches and clearing between Conti and Common Streets; making a new levee and ditch on Lake Pontchartrain; the digging and embankments on the Northern and Jackson Street Railroads, and extending up within half a mile of Carrollton—approaching the river, and extending near twenty miles in the rear of the plantations; in the centre of the city, the exposure of the subsoil for water-pipes in Bourbon Street, near the water-works—where *some of the first cases occurred*—New Levee, and Post 84, and other parts, to the extent of about a mile; and for gas probably as much, and principally in Apollo, and to Nayades and Dryades, in Galvez and Perdido Streets; and repairing Annunciation, Royal, and Chartres Streets.

"B. Extensive digging and embankments of earth at Algiers, opposite the city—being almost eighteen inches high and eighteen feet wide, ascending the coast for about twenty miles, running from half a mile to a mile from the river, in the immediate rear of the plantations.

"C. The exposure of the naked bank of the river for about six miles, many parts made a common receptacle of, and reeking with garbage and filth of all kinds, exposed to the sun and rain, without a single police officer to prevent its being made a common deposit for these nuisances, or covering or throwing them into the river; besides, the fermenting drainage of sugar and molasses hogsheads on the levee.

"D. The filthiness of the streets, privies, and backyards, a matter of common observation by the public and complaint in the newspapers; the gutters, often twelve hours after a rain which had washed them clear, bubbling up with a gas through dirty water.

"E. The large number of unfilled empty lots and unpaved streets in various parts of the city, and particularly in the Fourth District, which was much the most severely scourged with the fever, in proportion to its population; these low lots being a receptacle for, and exposing filth of all kinds, and stagnant putrid water.

"F. The large open drains in and near the city, including the large ones in rear of the First and Second Districts, and Gormley's Basin, half filled with the refuse of its district.

"G. The nuisances of soap and tallow chandleries, and the large collection of manure near the vacheries of the Fourth District.

"H. The interments, *within the city*, of six cemeteries, the receptacle of 7,063 bodies during the past year, to lend their important aid in corrupting the air.

"I. The numerous slaughter-houses in the Fourth District, and the many large vacheries and livery stables, with their offensive and polluting exhalations.

"K. The crowded, filthy, and unventilated dwellings, in low, damp situations, many in half-drained and unpaved lots and courts, with filthy, stagnant water under the floors.

"L. And about sixty thousand of unacclimated population, which has been added to the city since the last severe epidemic of 1847, and we have aggregated together materials to produce an epidemic, and the food to support it, unprecedented in this country."

Dr. Barton having unfortunately made use of the term "original soil," he has rendered himself the subject of severe criticism, and some unmerited ridicule, in relation to his views as to one, and a most important, source of morbid terrene exhalations. In the address appended to the report in the present volume, he has explained the sense in which he employed the term—a sense which, it seems to us, must have been very apparent to every one who has carefully read the report with a disposition to arrive at the true meaning of its author.

"The soil," says Dr. B., "as deposited by the river, *on its banks*, is known to contain very little organic matter; and the simple disturbance of that soil alone, it is not believed, or ever stated, would produce the influence ascribed to the 'terrene.' What is specially denominated and comprehended under this term was distinctly stated to be the rich alluvion of the country, the marsh mud, the detritus and remains of vegetable and animal life, and to be equivalent to putrefiable substances of all kinds—the filth of kitchens, stables, vacheries, privies, and every species of filth and offal, the relics of civilized life from whence proceeds the bad air produced by this disturbance and decomposition. These are found mostly in our backyards, in the gutters, streets, open lots, and are especially conspicuous where our pavements are disturbed—for the pebble-stone pavement is eminently objectionable in being the best filterer and retainer of putrescent organic matter. The 'disturbance' of these is always very offensive in hot, humid weather (the meteorological condition), and injurious to health. So the detritus and filth of our canals and basins, when dug out or cleansed, are composed mainly of the same materials; also the deep cuttings and excavations for our railroads, the first cultivation of the soil for agricultural purposes: all are followed by sickness *when the meteorological condition is present* and of sufficient duration. These results are believed to be uniform, the proofs are positive, the facts are not denied."

Dr. Barton insists that undue dampness of the air is essential to the generation of yellow fever. It is incapable alone, however, of producing the disease, requiring the joint agency of a high and long-continued atmospheric temperature, and local sources of malarial exhalations—among which latter he ranks, as a most efficient one, extensive disturbances of soil replete with organic remains. When these three causes—continued and intense heat, and dampness of the atmosphere, and malaria—are conjoined, he considers that the occurrence of yellow fever is almost inevitable.

We copy from the prefatory remarks the following positions, as expressive of Dr. B.'s views in reference to the agency of dampness in the production of yellow fever:—

"1. The epidemic yellow fever has never occurred here (New Orleans) at its commencement, but during a high dew-point—the minimum being upwards of 74°. In Savannah, last year, it was almost 2° less, and continued for some time.

"2. Yellow fever has always ceased, as an epidemic, before the dew-point descended as low as 58°. In Savannah, last year, it terminated when the dew-point was a fraction less than 65°. In 1848, here, it ceased at a dew-point about

1° higher, although the average of a series of years was when the dew-point reached 62° 12'.

"3. At temperatures of the dew-point below these, sporadic or endemic yellow fever may occur; but it is not known to have existed here, with any certainty, as an epidemic, when the dew-point differed from that above stated.

"4. What is miscalled the *contagion of yellow fever*, or its liability to spread, exists only with the first condition.

"This at once strikes at the root of all contagion in yellow fever, *per se*. No one pretends that either *sporadic* or *endemic* yellow fever is contagious. Do these differ from *epidemic* yellow fever in their nature? No one has the hardihood to make any such pretension. A change of air, which suddenly lowers the dew-point to near 58°, here, if continued, puts an end to epidemic yellow fever; a crowded population may enter the city, occupy the houses, rooms, nay, the very beds, which lately reeked with yellow fever, yet not an instance which can be attributed to contagion occurs. The filth, the miasm, and all the 'terrene' matters, are just as before. *But one change has occurred*—the connecting link, the combination, has been broken—the *meteorological element is wanting*, and the *effects* are no longer present. Can anything be more conclusive? Where is the contagion now? Do a few degrees of temperature less at once disarm the giant that has been mowing down, but a day or two before, his countless victims, with his remorseless scythe? The 'contingency' exists no longer. Such a misnomer is applied to no other disease. 'Sober second thoughts,' and sound judgment, worthy to enlighten and guide this people, will not apply it here, when its unsoundness is thus exposed.

"5. The main controlling influence, in all unhealthy situations, is *moisture*, whether in cities, towns, countries, ships, or dwellings, although filth and heat are to be deemed correlative.

"6. *Malaria* is not any *one specific thing*, but all impurities of the air, and organic matter in decomposition, are liable to influence injuriously the organism; and particularly the worn-out excreta of human beings may be so denominated, and is particularly incompatible with healthy action, and, when in combination with the meteorological condition, may produce yellow fever."

It is not our intention to discuss the question as to how far a correlation exists between yellow fever and bilious remittent fever. That, to a certain extent, these two diseases are related, there are many reasons for supposing. They are both, under the influence of particular atmospherical conditions, the endemics of certain localities, where the two may prevail, at the same season, simultaneously; or, as it often happens, the outbreak of yellow fever may be preceded by an unusual prevalence of bilious fever, of an aggravated grade. Still, the two diseases are so clearly distinguishable from each other by broad and unmistakable points of dissimilarity, that their specific difference can scarcely, we think, be disputed. To refer them both, as Dr. B. has done, to the same causes, differing only in their intensity, would be equivalent to an admission that the bilious remittent and yellow fevers are simply different grades of one and the same disease; which can, we think, be very clearly shown not to be the case.

The views of Dr. Barton, in relation to this question, will be seen by the following propositions laid down by him in the supplement to his report in the volume before us. These propositions he believes to be clearly demonstrated by the facts adduced in the report, by experimental observation, and by every principle of fair analogy.

"1. That yellow and bilious fevers proceed from the same causes, although differing in degree and amount.

"2. That these causes, acting upon individuals of different susceptibilities—as the acclimated, or native, and the unacclimated—produce these different effects: in the first, developing a milder grade of periodic fever; and in the second, the aggravated form, or yellow fever.

"3. That the main pathological cause of the difference in the phenomena exhibited in yellow fever from bilious fever arises from the difference in the rank and importance of the organs attacked in each case respectively: in the first, it is on organs whose integrity is more immediately essential to life, as the

nervous and sanguiferous systems, or those of cerebral life; and in the second, developing its influence on subsidiary organs, or those of rather secondary importance, those of animal life, as the liver, spleen, stomach, etc.

"4. That these causes proceed from all the circumstances that impair the purity of the air, which is essential to healthy existence, proceeding from vegetable and animal decomposition of all kinds, and disturbances of the original soil; that these, in the aggregate, constitute *malaria*, together with certain meteorological conditions, which are indispensable to give it activity.

"5. That all that we know of *contagion* is, that, being a specific virus, the product of secretory action, *it must be*, in its very nature, independent of all these circumstances and conditions; the existence and the spread of these can necessarily have no connection with it. But, as all the conditions productive of vitiated or bad air must tend to extend the above influences within the area of that impure air, and in proportion to that impurity and the meteorological condition, so the susceptibility to the spread of these diseases will exist.

"6. The final proof of all these propositions is, that when the conditions above pointed out are removed, or no longer exist, the effects cease, *causa sublata tollitur effectus*."

We have endeavoured, in the above notice of the treatise of Dr. Barton, to present our readers with an accurate outline of the views entertained by its author in regard to the etiology—the cause of yellow fever. For the very able and skilful manner in which those views are "reasoned out," illustrated, and supported, for the large array of facts adduced in their defence, and for the application of those views to the all-important subject, the means for the prevention of yellow fever, we refer our readers to the work itself. It is one well deserving of an attentive study. No one can rise from its perusal without instruction—without having acquired more definite and philosophical views in respect to the source and origin of a pestilence that has already wrought, on more than one occasion, dismay and death among the populations of several of our cities, and which may again, in the absence of a complete, watchful, and judicious system of hygienic police, visit with destruction the same localities, or even others, the indwellers of which may now feel themselves in entire security. For we fully believe, with Dr. Barton, that wherever there shall become conjoined an intense and long-continued atmospheric temperature, undue moisture, and extensive sources of malarial exhalations, the development of fever may be anticipated with tolerable certainty. And the concurrence of these conditions in several of our cities, where the disease has never, or at least not for many years, made its appearance, is neither impossible nor improbable.

D. F. C.

ART. XV.—*Report of the Board of Trustees (including that of the Resident Physician) of the Insane Asylum of the State of California.* Submitted to the Legislature, January 20th, 1854.

CALIFORNIA contains at present a large population assembled with a rapidity which has had no parallel in the settlement of any portion of the globe. They have congregated under the influence of the faculty of acquisitiveness. They have come from every quarter of the world. They speak a great diversity of languages. They are mostly men, and in the prime of energetic manhood. They present every grade of intellectual and moral cultivation; but, unfortunately, in the majority these characteristics are of the lower orders. All shades of habit, a wonderful heterogeneity of customs, all modes of social thought, habitudes to all modes of civil government, and all religious faith are represented by them. The adhesive power ordinarily found among great bodies of men, are here in a great measure wanting. There is no general bond of sympathy, but simply that of *manhood*, a bond less firmly tied, and more easily severed, than the Gordian knot.

"California ought to produce," says the report before us, "a race of men the

most vigorous and powerful on the face of the earth, on account of the mildness of the climate, the purity of the atmosphere, and the great opportunity for out-door exercise; man being able to live in the open air, without a habitation, nine months in the year." California may yet achieve that which it is here asserted she ought. As for her present inhabitants, she did not produce them. They are, as we have seen, the congregated off-shoots of other countries; and, to continue the language of the report, "it is fearful to contemplate the amount of mental excitement, the violent passions, the ungoverned tempers, and continued turmoil prevailing throughout the entire population of the State."

Under all these circumstances, what could be expected *à priori* by the psychologist? What, but that which has resulted? "The number of insane persons sent to the Station House, in San Francisco, during the year 1850, was *fourteen*; 1851, *twenty-two*; 1852, *thirty-four*; 1853, *sixty-five*." And within six years from the time of the discovery of gold, California has a State Hospital for the Insane, with upwards of a hundred patients actually resident, and numbering, upon its register of admissions, upwards of three hundred!

A General Hospital was established at Stockton, by act of the California legislature, and opened in August, 1851.

A department for the insane "was added, and all insane persons in the State directed to be sent to it, in May, 1852; and agreeably to the instructions of the last legislature, all invalids remaining in the General Hospital on the 1st of July, 1853, were transferred to the State Marine Hospital, in San Francisco. At the same time, all the furniture and other property of the hospital, and all the insane patients, were placed in charge of the officers and trustees of the State Asylum for the Insane."

The asylum is under the medical superintendence of Dr. Robert K. Reid, to whom we are indebted for the report now under review. It is provided with one hundred acres of land. The building already erected forms but one wing of the structure ultimately contemplated. "It is built of brick, is 130 feet in length by 50 feet in width, and two stories in height." It will properly accommodate but 80 patients, although it had 102 at the date of the report.

The unprecedented events and circumstances of which this report is one of the sequences, and the necessarily peculiar and more or less anomalous and unique character of the matter which it includes, are a sufficient justification for a more elaborate review, and more extensive quotations of statistics, than we have been accustomed to give to publications of the kind.

The time embraced by the report is a period of about eighteen and one-half months, from the 14th of May, 1852, to the 31st of December, 1853.

	Men.	Women.	Total.
Patients admitted	264	20	284
Discharged, recovered	153	7	160
Died	19	3	22
Remaining, Dec. 31, 1853	92	10	102

Causes of death.—Maniacal exhaustion 3, epilepsy 3, dysentery 3, acute mania 2, delirium tremens 2, meningitis 2, marasmus 2, typhoid fever 2, disease of spine 1, puerperal fever 1, laryngitis 1, dropsy 1. "In 90 cases of acute mania, only 2 died. No suicide has yet happened in the institution, although several attempts have been made. No acute disease unconnected with the brain has prevailed to any great extent. Some cases of intermittent and remittent fever have occurred during the last four months. They were all of a mild type, and soon recovered."

The great disparity of the numbers of the two sexes admitted, is undoubtedly to be attributed to a similar difference in the general population. The percentage of recoveries (57.95) is large, and finds its cause in the large proportion of recent cases admitted. When institutions in the older States are opened, they are made, almost without exception, the receptacles for large numbers of the incurable, which have been accumulated in other places through a course of many years. This could not occur in California.

The different forms of the mental disorder were as follows:—

Mania, acute	90	Melancholia	30
“ chronic	18	Monomania	24
“ puerperal	34	“ suicidal	10
“ paroxysmal	18	“ homicidal	8
“ epileptic	12	Dementia, acute	36
“ nympho	2	“ chronic	31

The nomenclature of insanity is so imperfect, the standards of arrangement so little understood, the shades of mental disorder so combined and blended, that scarcely two physicians would classify any collection of patients alike. Hence this table is of but comparatively little value. We are particularly struck with the large number set against the form “acute dementia.”

Alleged Causes of Insanity.

Moral.		Physical.	
Disappointment	27	Intemperance	42
Domestic trouble	13	Bad health	25
Loss of property	9	Masturbation	20
Religious anxiety	8	Dissipation and exposure	19
Fear and grief	8	Epilepsy	10
Mental excitement	7	Typhoid fever	6
Desertion of wife	4	Parturition	4
Spiritualism	4	Hereditary	4
Jealousy, &c.	4	Injury of head	3
Intense application	3	Insolation	2
Disappointed affection	3	Effect of poison	2
Seduction and desertion	2	Excessive venery	2
Fanaticism	2	Syphilis	2
Neglect of husband	1	Disease of spine	1
Desertion of mistress	1	Meningitis	1
Ill treatment	1		

Unknown, 40

There are but few cases of mental alienation which can be assigned with full confidence of accuracy, to any cause; and therefore implicit reliance upon statistics of this kind is not expected. Nevertheless, the above table is worthy of study by the reader. A large proportion of the cases are attributed to causes necessarily growing out of the manner in which the State was peopled.

“The investigation of ‘spiritual manifestations’ was the exciting cause in four cases. They were all persons of middle age, in the prime of life, with good intellects, fine education, and of the first standing in the community. One was a lawyer and judge, one an editor, one a professor, and one an artist. They all recovered, and were discharged.”

The subjoined schedule of the *nativity* of the patients will be read with interest.

1. *United States*.—Maine 7, New Hampshire 2, Vermont 2, Massachusetts 12, Rhode Island 4, Connecticut 5, New York 34, New Jersey 2, Pennsylvania 10, Delaware 1, Maryland 6, Virginia 10, North Carolina 9, South Carolina 1, Georgia 5, Mississippi 2, Missouri 2, Louisiana 5, Arkansas 1, Texas 1, Ohio 8, Indiana 8, Illinois 4, Michigan 1, Iowa 1, Kentucky 3, Tennessee 6, Oregon 1, CALIFORNIA 4.

2. *Other parts of North America*.—Canada 1, Mexico 5.

3. *South America*.—Chili 6.

4. *Europe*.—England 14, Ireland 28, Scotland 11, France 29, Germany 18, Spain 1, Portugal 3, Italy 3, Denmark 1, Norway 1, Sweden 2, Russia 1, Prussia 2, Poland 2.

5. *Asia*.—China 1.

6. *Australia* 1.

Occupation.—Miners 78, no occupation 32, labourers 21, sailors 18, farmers 17, carpenters 13, merchants 10, bakers 10, soldiers 8, clerks 7, blacksmiths 6,

traders 6, cooks 6, painters 5, masons 5, machinists 4, tailors 3, peddlers 3, gamblers 3, servants 3, preachers 2, artists 2, druggists 2, saddlers 2, distillers 2, lawyers 2, gardeners 2, butchers 2, arrieros 2, vaqueros 2, shoemakers 2, cabinet-makers 2, teacher, printer, jeweller, engineer, architect, tanner, lithographer, engraver, editor, tinner, silversmith, sail-maker, and wagon-maker, 1 each.

By this it appears that the miners have suffered severely. The proportion of farmers is, as might be expected, small, while that of sailors, carpenters, bakers, blacksmiths, masons, painters, persons of "no occupation," and some others, are uncommonly large. The table contrasts singularly with those of most institutions.

Social Condition.						Men.	Women.	Total.
Unmarried	184	5	189
Married	58	10	68
Widowed	22	5	27

Here are to be remarked the great predominance of the unmarried men, and the large proportion of the widowed of both sexes. From a remark in the report, under this table, we learn that, small as is the number of female patients, it is still greater, in proportion to their sex in the whole population, than that of the males. We subjoin the table of ages at the time of admission.

						Men.	Women.	Total.
Between 10 and 20 years	18	3	21
20 " 30 "	114	11	125
30 " 40 "	74	2	76
40 " 50 "	33	3	36
over 50 "	24	1	25

The number between 20 and 30 years largely predominates. "The average age of the whole number admitted was only 34 years! The extremes were 10 and 70 years."

"We admitted two persons insane who were congenitally deaf and dumb. They both recovered, were discharged, and sent to the mines." Ten of the patients were negroes.

Dr. Reid makes the following remarks upon his manner of medical treatment: "General bleeding, blistering, with violent emetics, or powerful cathartics, are never employed. Mild laxatives, tonics, stimulants, and the whole tribe of narcotics, are the chief reliance. * * * As demulcents, the various gums, elm, flaxseed, sago, tapioca, and arrowroot are in hourly use."

As some of the resources for moral treatment, eleven different newspapers are mentioned, which have been sent to the asylum by their editors or proprietors. "With the labour of the convalescents alone, the yards have been graded and sown with grass-seed, several pavements made, numerous walks laid out, and a large number of trees, shrubs, vines, and seeds planted, so that in a few years, the asylum will become one of the most beautiful and attractive places in the country."

"The climate of Stockton and the San Joaquin Valley is very mild and equable. The variations of the thermometer and barometer are very slight; and the number of cloudy days is very small compared with those of clear and sunshine."

At the close of the report there is a meteorological register, kept at the hospital, from September, 1851, to December, 1853. The following extract in relation to the year last mentioned will give some idea of the climate:—

"In the whole year there were only 48 cloudy and rainy days; the remaining 317 days were clear, with a bright sun.

"The whole quantity of rain during the year, was 12½ inches.

"The coldest morning was that of Dec. 31, thermometer 28° above zero.

" " noon " " 26, " 43° " "

" " night " " 25, " 34° " "

"The warmest morning was that of June 16, " 82° " "

" " noon " " 16, " 100° " "

" " evening " " 15, " 83° " "

"The barometer on June 19th ran down to 29.15, and on Nov. 18th, rose as high as 29.87. These were the extremes."

We observe, also, that only upon *eight* observations in the course of the year, was the mercury down to the freezing point of water. These were all from the 15th to the 31st of December, and all in the morning. Three times it stood at 32°, four times at 30°, and once at 28°.

The prevailing winds are almost always from the northwest during the dry season; from the southwest, southeast, and south, in the wet season. P. E.

ART. XVI.—*Clinical Lectures on the Diseases of Women and Children.* By GUNNING S. BEDFORD, A. M., M. D., Professor of Obstetrics, the Diseases of Women and Children, and Clinical Midwifery, in the University of New York. "Medicus curat morbos, natura sanat."—*Hippocrates.* New York: Samuel S. & W. Wood. 1855. 8vo. pp. 563.

No medical education—however prolonged the course of instruction, however complete the number of professional subjects embraced in it—or however competent and industrious may be the teachers by whom it is conducted—is calculated to prepare the student for the faithful and successful discharge of the important duties of a physician, from which clinical instruction is excluded—of which it does not, indeed, constitute a prominent feature. From the study of books and attendance upon lectures, much useful knowledge is to be gained that can be acquired from no other source. Both are essential to imbue the mind with those principles and facts which constitute the theory of medicine, and form the basis of its rational practice. It is only, however, at the bedside of the sick, that a practical acquaintance with the phenomena, diagnosis, course, and terminations of disease can be acquired, as well as of the application and usual results of the remedial measures instituted for the cure of its several forms, the control of its more prominent symptoms, or the arrest of various tendencies calculated to augment its intensity.

From no descriptions given in books, or delineated in the oral instructions of the most accomplished lecturer, let these descriptions be as accurate and graphic as they are capable of being made, is it possible to acquire that accurate knowledge of disease, which is so readily communicated under the instructions of a competent teacher, in the clinical wards of a large and well arranged hospital.

Clinical instruction must, therefore, be considered as an indispensable branch in every scheme for a complete course of medical education. We cannot say, with Dr. Bedford, that it is what the physician stands most in need of, but we do assert that, without it, he will enter upon the duties of his profession inadequate for their proper fulfilment—obliged to grope his way, at first, amid doubt and uncertainty—and only, after many blunders, and much deep mortification, acquiring, imperfectly, that clinical experience, with the materials for which he should have been put in possession before he entered upon his professional career.

But notwithstanding our high estimate of the value of clinical instruction, we cannot say that we can perceive any great value in a printed course of clinical lectures, similar to the one before us. A treatise on any given disease or class of diseases, or a systematic work on pathology and therapeutics, based upon the results of the clinical observations of its author—provided these observations were of sufficient extent to insure the accuracy of the results deduced from them—would, we admit, possess a very high value; and we regret that the opportunities, qualifications, and inclination necessary for the preparation of works of this character, are not more frequently met with than has heretofore been the case. The value of these works consists in their teachings being based on the personal observations of their authors, and in this respect may be considered as conveying clinical instruction, but not in the sense in which clinical

instruction becomes of importance to the medical student and young practitioner, by enabling him to study—in patients actually labouring under disease—its forms and features, its progress and results. In a course of lectures on various maladies—mere descriptions of cases with running commentaries on the particular disease present in each—without the slightest approach to systematic arrangement—much valuable and instructive matter may be presented, but which, from the very form in which it is presented, is not calculated to induct the student into a knowledge of the theory and practice of medicine, or to aid the young practitioner in the formation of a sound judgment in the diagnosis of disease, and correct views as to its therapeutical management, amid the doubts and emergencies incident to his professional career. A course of lectures, such as we have described, can never become a substitute for, nor serve as a useful guide to a proper course of clinical instruction—the true practical study of the several forms of morbid action, with their effects and phenomena.

In the lectures of Dr. Bedford, amid much that is common-place, though valuable to the students for whose instruction it was given, will be found many judicious remarks in reference to the phenomena, causation, and management of some of the more frequent of the diseases peculiar to the female sex and to the period of childhood, with, occasionally, pathological views and practical directions, the soundness and propriety of which may admit of question—the whole delivered in a peculiar oracular style, as though in every sentence that fell from the lips of the lecturer there was embodied some truth, reserved alone for the ears of such as have the good fortune to constitute his auditory. In the manner in which the prelections of Dr. Bedford are delivered, there is often an overweening pretension, which, though perhaps pardonable when assumed within the walls of the clinical ward, and before a class of admiring pupils, becomes amusing if not even supremely ridiculous when we meet with it in a work recommended by its author to the approbation of his professional brethren.

The lectures before us have, we are assured, been faithfully reported as they were delivered by their author, by competent persons. In some particulars the fidelity of the reporters has been carried to an extreme. Many of the colloquies between the lecturer and his patients on their introduction to the class, and at their dismissal from the clinic, might with great propriety have been omitted. When these are for the purpose of arriving at facts elucidatory of the seat, character, or cause of the malady under which the patient is labouring—those facts, and the manner in which they were arrived at, could have been more clearly indicated by a simple statement, than by detailing the actual conversation in the course of which they were elicited. We do not, indeed, think that the oral examination of the patients, as presented in the lectures before us, was always of a character adapted to elicit the information aimed at, nor exactly adapted as a model to be followed by the student when he shall be called upon to minister to the sick. Some of the conversations that have been gravely reported, have, so far as we can discern, nothing in them in the remotest degree pertinent to clinical instruction. Take for example the following from the 16th lecture.

The patient was labouring under an encysted tumour of the vagina.—The professor remarks:—

“There are two modes of treating encysted tumours of the vagina. One consists in excision, the other in merely evacuating the contents of the sac. The former is sometimes attended with difficulty, and in my opinion is rarely necessary. I shall now with my lancet penetrate the sac, and allow its contents to escape. ‘Oh, sir, you won’t hurt me, will you?’ ‘I will give you a little pain, my good woman, but it will only be momentary; will you consent?’ ‘Anything you say, doctor.’ ‘That is right, madam; I shall not abuse your confidence. There, did I hurt you?’ ‘Oh, dear! is it over, sir?’ ‘Yes, my good woman, it is all done.’ ‘God bless you, doctor.’ ‘Thank you, madam.’”

After some directions as to subsequent treatment, we are informed that the professor said: “‘You may go home, madam. You will have no more trouble from that tumour.’ ‘Oh, sir, I am so much obliged to you.’ ‘You are quite welcome, my good woman. Come to the *Clinique* two weeks from this day, and

report whether or not we have told you the truth.' 'Indeed I will, sir.' 'Good morning, madam.'"

The lectures of Dr. Bedford are not always confined to the dry, precise, severe details of clinical instruction. He occasionally bursts forth from the trammels imposed upon him by his office, in flights of oratory. Treating of the critical period of female life, in his seventh lecture:—

"Woman," he remarks, "at every period of her existence, is liable to disease and suffering; and it would, perhaps, appear to the careless observer that God, for some wise yet mysterious purpose, had imposed on her penalties and afflictions far heavier than those which our sex is called upon to bear. Such may be the belief engendered in the vulgar mind, after contemplating the constant and imminent perils by which the female is more or less surrounded during the various eras of life. But the philosophic eye, glancing as it does at the admirable laws on which all health is based, sees at once that it is the violation of those laws, more than any other circumstance, which produces such disastrous effects on the female frame. The refinements of civilization, and the consequent departure from those salutary influences so essential to that harmony of action, without which a healthy condition of the system cannot be maintained, are making fearful inroads on the females of the present day; so that whilst, on the one hand, the scholar is gladdened by the triumphs of civilization, the philanthropist, on the other, cannot but lament the evils which necessarily follow in its train.

"It was the pride of the ancients to impart to their children robust constitutions, and enduring health; and could a mother of those sensible times again visit earth, look upon the present condition of society, and witness its effects upon the women of the present generation, she would indeed think that human nature had nearly run its course. She would search in vain, in our gay cities, for those who would remind her of her own ruddy and vigorous daughters; and from the fulness of her heart she would drop a tear over poor degenerate humanity. If the diseases incident to women be more frequent at the present time than formerly—and the fact no one will deny—the frequency is to be attributed to changes in modes of life and education, and to the increase of nervous excitement, the immediate effect of those changes. Whilst I would not desire to see the females of the present day subjected to the severe training imposed upon the young girls among the ancient Greeks, yet I would suggest that a useful lesson might be learned from reference to the discipline then exercised. History informs us that the Lacedæmonian father required of his daughter to support the weight of arms, and encounter the labours of war, until the time of her marriage; and Hippocrates observes, that the girls of Scythia were not permitted to marry until they had killed three men! In those days it is asserted that hysteria and other nervous derangements were not of frequent occurrence!

"There is, however, even in our times, a remarkable difference in the aptitude of females to disease, and this arises from the differences of habit, education, etc. Compare, for example, if you desire fully to appreciate the influence of habit, education, and mode of life on the health of the female, the buxom lass of the country with the tender and frail belle of the metropolis. And in order to obtain the just benefit of the comparison, let it be instituted at the period of puberty, a most trying and critical period—so critical, indeed, that it is often the index of future health, or of premature and painful decline. The function of menstruation, which exercises such a controlling influence over the economy, appears, generally speaking, in the former case with marked regularity, and in entire accordance with the appointments of nature; whilst, in the latter, in consequence of influences which have subjected the nervous system to continued excitement, thus prematurely developing the vital forces, and, as it were, forcing nature, menstruation is characterized by evident aberrations, and more or less derangement in the various functions of the body. This departure from the exactions of nature, is too frequently followed by the penalty of severe suffering and disease. The young and thoughtless girl, who, in her wayward career, so far contravenes the laws of the system as to interfere with the menstrual function, imposes on herself a life of sorrow, if not of irremediable ill

health. Between this function and the thoracic viscera, as also other portions of the economy, there is a close alliance, if, indeed, there be not a mutual dependence, which, unhappily, too often escapes the observation of the practitioner.

"Palpitation of the heart, asthma, hæmoptysis, are not uncommon consequences of functional disturbance of the uterine organs; and instead of being regarded as the effects of this form of derangement, should they be treated without any reference to their legitimate cause, serious, if not fatal results will oftentimes ensue. Look, too, at the condition of the nervous system in cases either of suppression or retention of the menses; it is thrown frequently into extraordinary excitement, producing convulsions, hysteria, catalepsy, epilepsy, chorea, and even mania. Do not these facts declare in silent, yet eloquent language, the complete subjection in which the uterine organs hold the general system, and at the same time point out to the physician the absolute necessity, when nature is incompetent to act for herself, of preserving, by judicious interference, the integrity of function appertaining to these most important organs?

"Woman, from her infancy to old age, is an object of constant interest; and it is not strange that a being so tender, and yet so full of endearments, should have called forth the admiration of the philosopher, and the fervid praises of the poet. Her history is but the narrative of good deeds. In health she is our pride; in disease, our solace; and in the faithful discharge of her duties to society, she is the idol of all hearts. Like a ministering angel, she soothes us in affliction, and under the depressing influences of adversity, she inspires hope, and incites to renewed effort. Who has not felt the cheering influence of her smiles, and the encouragements of her eloquence in the dark hour of despondency! Abandoned by friends, and left to the cold charities of a selfish and heartless world, the husband of her bosom then knows how to appreciate the depths of her love, and the sincerity of her vows.

' There, drink my tears while yet they fall,
Would that my bosom's blood were balm,
And well thou knowest I'd shed it all
To give thy brow one minute's calm.
Nay, turn not from me that dear face—
Am I not thine—thy own loved bride—
The one, the chosen one, whose place,
In life or death, is by thy side?"

"As wife, mother, sister—in a word, in every situation of life, virtuous woman is the kind and fast friend of man. Is it, therefore, not due to this self-sacrificing being that we, who know so well how to value her excellence, should labour assiduously to diminish the sufferings and assuage the sorrows incident to her sex?"

We shall not stop to inquire whether it be really a fact that the female of the present day is inferior in health and vigour to what she was in former ages, though we doubt very much that any satisfactory evidence of the fact can be shown; nor shall we discuss the question as to the real pathological relationship between "functional disturbance of the uterine organs," and the various affections of the lungs and nervous system, enumerated by the lecturer as common consequences of such disturbance; but, passing on to the 20th lecture, quote from it the author's rhetorical rebuke of "meddlesome midwifery." It was elicited by a case, presented at the clinic, of a female, twenty-two years old, with complete occlusion of the meatus urinarius; adhesion of the walls of the upper fourth of the vagina, and a vesico-vaginal fistula, produced by instrumental delivery.

"The case before you, gentlemen," remarks the lecturer, "exhibits another of the many instances of professional brutality constantly occurring in this populous city; and it is time that something should be done to arrest the reckless temerity of men calling themselves physicians, who, if we are to judge them by their acts, place a very insignificant estimate on human life. But the melancholy feature of this whole business is, that these assaults on health and life are made under the protection of a diploma, and therefore are perfectly

within the record! No; a diploma, though it may serve the purposes of the holder, is insufficient to justify the moral wrong of the sufferings entailed on this unhappy woman! They are sufferings, as I shall show you, of gross ignorance, or a wanton disregard of life. A diploma without knowledge is a curse to its possessor, and a fearful instrument of destruction to the community. With knowledge, too, must be conjoined a refined morality, based upon that Christian principle, '*Do unto others as you would wish others to do unto you.*'

"You have before you a poor woman, whose health is her only capital; whose daily bread is the product of her daily labour, and who has had entailed upon her, either through ignorance or unpardonable carelessness, a complication of maladies, which, even if they be measurably relieved, will cause her more or less distress during her entire existence! The first question which naturally presents itself to the mind in viewing the serious afflictions of this patient is this: What has produced this state of things, and could it, by a proper exercise of judgment, have been avoided? She was delivered with instruments, and to their unskilful and unnecessary employment is to be referred all her present difficulties. There is no evidence before us that the use of instruments was at all indicated. The patient tells us that 'her labour was not severe,' it was 'only lingering!' She then has fallen a victim to that 'hot haste,' which too often prevails in the lying-in chamber; or to that undying fondness which some men cherish for operative midwifery. Let this case be a lesson to you—think of it in your hours of meditation, and let it act as a shield for those who confide their lives to your custody! In the eye of Heaven murder loses nothing of its atrocity, because concealed from the ken of human observation; so is it with the dark deeds of our profession. The diploma may afford a mantle, so far as earthly jurisprudence is concerned—but the time of reckoning will come with appalling retribution."

It will be impossible for us to enter upon a detailed examination of the diversified subjects treated of in the volume before us. To take them up as they are presented in the several lectures would render our notice of these, like the book itself, a thing without order or arrangement; and after a careful perusal of the work, we have discovered no leading points in the pathological views and practical rules inculcated by the author, which, by their originality or the particularly skilful manner in which they are presented, seemed to demand especial attention.

In the teachings of Dr. Bedford in reference to the class of cases which present themselves at the clinic, there is far more to praise than to condemn. His prelections are in general sound; though occasionally his pathological expositions are superficial and defective, and his indications of cure not always very clearly indicated.

Among other portions of these lectures that may be read with profit by the student at least, we would indicate particularly the remarks on chlorosis in the 2d lecture, on ulceration of the neck of the uterus in the 8th, on the uterus in health and disease in the 9th, on the mortality of infancy in the 13th and 17th, on physometra in the 18th, on asphyxia of the new-born infant in the 25th, on the management of the new-born infant in the 26th, on nursing, weaning, and vaccination in the 27th, on uræmia in the 29th, and on sterility in the 30th lecture.

Judging from the work before us, which we are assured presents "a faithful representation of what occurs in the *clinique*," the latter is not adapted to convey all the instruction that it is necessary for the student to acquire at the bedside, inasmuch as it does not appear that he has the opportunity afforded him of watching the daily progress of the cases which constitute the subject of the lectures delivered. This is a radical defect in all courses of clinical teachings that are undertaken out of the wards of a properly arranged hospital, though partially obviated by what are termed ambulant clinics, as instituted in some portions of Germany.

D. F. C.

ART. XVII.—*A Practical Treatise on the Diseases of the Eye.* By WILLIAM MACKENZIE, M. D., Surgeon Oculist in Scotland in Ordinary to Her Majesty, &c. &c. To which is prefixed an *Anatomical Introduction explanatory of a Horizontal Section of the Human Eyeball.* By THOMAS WHARTON JONES, F. R. S., Professor of Ophthalmic Medicine and Surgery, &c. &c. *With one hundred and seventy-five Illustrations. From the fourth and enlarged London edition, with Notes and Additions* by ADDINELL HEWSON, A. M., M. D., one of the Surgeons to Wills' Hospital, &c. &c. Philadelphia: Blanchard & Lea, 1855. 8vo. pp. 1027, including index.

THE appearance of an American edition of Mackenzie's elaborate treatise so soon after the new issue of Hays's *Lawrence*, speaks well for the increased attention to the scientific study of ophthalmic surgery in our country. The long-established and deservedly high reputation of this great work, and of its author—world-wide, as it has been fairly called—renders any special commendation needless. The mere announcement of its publication is sufficient to interest professional readers; and we sincerely congratulate them on such a valuable accession to their means of reference upon a branch of practice so important, and often so embarrassing, as the treatment of diseases of the eye.

In the London advertisement to the present edition, we are informed that "a large amount of new matter has been added, and an attempt made, as far as the author's opportunities have served, to notice every material advance in the pathology and treatment of the diseases of the eye which has been made during the last fourteen years." He has also taken care "to introduce, under each head, the most remarkable synonyms, and to give references to the works where the best figures of each disease may be found;" and he appends a list of twenty-five authors, which he chiefly refers to for such illustrations.

These references, and the vast number of citations accumulated throughout the book, together with the abundant array of cases (over four hundred in all), combine fully to justify the assertion quoted by the American editor, that the treatise "forms, in respect of learning and research, an encyclopedia unequalled in extent by any other work of the kind, either English or foreign." That this is the general estimate, is shown by the fact, to which the author himself alludes with very natural gratification, that his book has not only passed through three large editions in his own country, and been reprinted by a transatlantic press, but "has been deemed worthy of being translated and published in the three best known languages of modern Europe—German, French, and Italian."

We have looked through the different chapters with a good deal of interest, and need hardly say that, even in the unavoidably hasty glance to which we have been limited, we have been struck with evidence, from page to page, of the learned author's unremitting care and industry in maintaining the position of his work in keeping with the progress of the day. In some few instances, perhaps, an experienced reader, who happens to be familiar with individual topics which have been more thoroughly discussed elsewhere, may feel a little disappointment; but, on the whole, we are satisfied that no student, teacher, or practitioner need fear to rely upon the book as a library in itself, and one adapted to every want in relation to the subjects of which it treats. The illustrations have been very materially increased in number and excellence, and have been admirably copied by the American artist. Several additional wood-cuts, in the same superior style, have been introduced by the American editor, and materially aid in illustrating the original text, as well as his own annotations.

Dr. Hewson's contributions relate, as he tells us, "chiefly to matters of a practical character," and are, many of them, as a matter of course, of especial interest to American readers. They include many new cases, as well as recent observations, which are scattered throughout the volume, and are generally brief, appropriate, and clearly written. By affording essentially new matter (as in the "Short Account of the Ophthalmoscope," entirely overlooked in the original), they may be regarded as decidedly enhancing the value of the edition.

In the getting-up of the latter, as to paper, printing, and engraving, the pub-

lishers have shown their usual taste and liberality. They are entitled to the thanks of the profession of this country for having at last brought within our reach a handsome reprint of one of the best productions, of its kind, that has issued from the British press.

E. H.

ART. XVIII.—*A Dictionary of Terms used in Medicine and the Collateral Sciences.* By RICHARD D. HOBLYN, A. M. Oxon. A new American, from the last London edition. Revised, with numerous additions, by ISAAC HAYS, M. D., Editor of the *American Journal of the Medical Sciences*. Philadelphia: Blanchard & Lea, 1855. 12mo. pp. 522.

THE Dictionary of Hoblyn is one well adapted to the use of the student of medicine. Presenting, as it does, in the edition before us, the etymology, with a concise explanation of the meaning of the terms which most frequently occur in those works to which the attention of the student will necessarily be directed during the period of his pupilage, and to which he will continue to recur for instruction during the early period of his professional career, it will be found by him a valuable and trustworthy *vade mecum* for the purpose of inducting him into the true signification of the received terminology of every department of the healing art.

Works of this kind afford great assistance to the student, and are indispensable aids to those who have not the advantages of a classical education, and to whom the etymology of a technical term affords no clue to its meaning. To render these works of easy reference, and, at the same time, faithful guides to the information for which they are resorted to, it is important that, while they embrace all the terms in common use in medical works and those devoted to the collateral sciences, the definition given of each term should be concise, clear, and explicit. Both of these requisites will be found in the Dictionary before us. With the additions of the editor it is sufficiently copious, while the explanations of the terms embraced in it, though condensed, convey a just idea of the sense in which each term is employed by medical writers. It merits, we believe, in even a higher degree, the encomiums that have been bestowed upon the previous editions by the professional journals of Europe and America.

D. F. C.

ART. XIX.—*The Mineral and Thermal Springs of the United States and Canada.* By JOHN BELL, M. D., author of "Baths and Mineral Waters;" "Baths and the Watery Regimen;" "Letters on the Practice of Physic;" "Regimen and Longevity;" "Dictionary of Materia Medica," etc. etc. Philadelphia: Parry & McMillan, 1855. 12mo. pp. 394.

THIS very excellent manual of the mineral and thermal springs of North America will be found interesting as well to the general reader as to the physician. Independently of the rational curiosity which every inquiring mind must experience in reference to the location, composition, temperature, and modes of issue of the numerous warm, hot, and mineral springs which occur in so many portions of the United States, with the character and geological formation of their respective vicinities, the traveller in pursuit of pleasure and the invalid in search of health are desirous of learning "where to go, how to go, and what to find"—where "waters, gushing forth from the rock's hidden channels or the bosom of the earth, form natural pictures which delight the eye, or by their temperature or mineral impregnations serve as medicines for the ills of flesh."

The physician, also, in order that he may be able to direct aright his patients to the spot where, from the use of the water and the influence of its associated scenery, purity of air, and temperature of climate, according to the nature of

their respective ailments, they may most certainly expect relief, requires a knowledge of the location and physical and chemical properties and remedial virtues of our several mineral and thermal springs.

Dr. Bell was the first, we believe, who made the attempt to collect and arrange methodically the numerous separate and scattered histories and descriptions of the different medicinal springs of this country. Some twenty-five years ago his first account was published, since which time he has been industriously engaged in collecting from every reliable source additional information on the subject. The work before us contains notices, more or less full, of one hundred and thirty springs and groups of springs belonging to the United States. If account were taken of each separate spring of the several groups, which is marked by distinctive properties, the number would exceed two hundred.

The present volume is presented by the author as the forerunner of a much larger work, in which will be embraced the natural history of mineral and thermal springs generally, with a history and chemical account of these springs in all parts of the world.

While we would recommend the manual before us to the patronage of the profession, and to the public at large, as one replete with valuable information, and with much to interest and instruct the general reader, we shall look with some anxiety for the appearance of the promised general treatise, which, if completed after the plan sketched out in the preface to the present work, cannot fail to prove a valuable addition to the library of the physician, and a pleasant and instructive table book for those out of the profession.

D. F. C.

ART. XX.—*A Manual of Clinical Medicine and Physical Diagnosis.* By T. H. TANNER, M. D., Licentiate of the Royal College of Physicians; Physician to the Hospital for Women, etc. etc. To which is added the *Code of Ethics of the American Medical Association*. Philadelphia: Blanchard & Lea, 1855. 12mo. pp. 252.

THE present belongs to a class of works that we have never held in very high favour. We believe that, from the use generally made of them by students, they are calculated to impede, rather than to facilitate, the acquisition of a correct and thorough acquaintance with the subjects of which they profess to treat. The manual of Dr. Tanner, nevertheless, certainly presents much of an interesting and valuable character, and which, though concisely expressed, cannot fail to aid the student in the study of disease at the bedside, provided it be employed by him solely as a manual, and his investigation of the subjects embraced in it be not confined to its pages.

Restricting himself almost exclusively to the means employed in the exploration of the various maladies of the human organism, and their correct location and diagnosis, Dr. Tanner has, in a very happy and successful manner, indicated the leading particulars to which, in the clinical study of a case of disease, the attention of the physician is to be directed, the value and import of the various abnormal phenomena detected, and the several instrumental and accessory means that may be called into requisition to facilitate diagnosis and increase its certainty.

As a manual simply, we can very confidently recommend it to the student, as well as to such of the younger members of the profession who may feel the necessity of a work to which they may refer at any moment, to revive or test the correctness of their knowledge of the proper means for the investigation and discrimination of disease.

D. F. C.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Remarkable Case of Congenital Absence of Sternum; the Movements of the Heart Visible.*—An instance of congenital fissure, or the absence of the sternum, in a young man, a foreigner, excited very considerable attention this week at Guy's Hospital. We have seldom, if ever, seen a case of such intense interest, in a physiological point of view, as connected with the very vexed question of the sounds of the heart. The region of the sternum, in this individual, is, in fact, quite open, and displays under the skin the particular movements of the pericardium and heart. This fault of conformation interferes rather singularly with the actions usually ascribed to the sterno-hyoid and sterno-thyroid muscles, which are entirely absent at the right side, and very imperfectly developed at the left. The sterno-cleido-mastoid muscles, curiously enough, in their movements, extensively influence the neck, without a trace of sternal attachment; and in fact, when forced inspiration is tried, it is quite remarkable how the chest is expanded by these muscles, and the trapezius at each side, the diaphragm and the heart not seeming to be much affected, while the lungs, or what may be conceived to be a mass of pleura, mediastinum, and lung, being forced out, fills up the front of the neck. The man is now twenty-five years of age, and enjoys apparently good health. He has been examined by Skoda, Rokitansky, Bouillaud, Bernard, Piorry, Malgaigne, and some others on the continent, and has excited no ordinary speculation as to the sounds of the heart. He has been at Guy's Hospital, under the observation of Dr. Wilks, Dr. Parry, Dr. Addison; at St. Thomas's Hospital under that of Mr. Simon, Dr. Peacock, &c.; at King's College Hospital under that of Dr. Todd, &c. We have been particularly struck, at a hasty glance of the case, by the beautiful vermicular movement of the heart, by which the right auricle is filled with blood; for it does not appear to be the aorta which is under the skin, as at first suspected, but an oblong tumour covered with pericardium, the pulsations of which are not exactly synchronous with the pulse at the wrist, though it requires considerable care to detect the difference—more especially if the young man, the subject of the malformation, is tired by too much examination of the parts. This oblong tumour is most probably the right auricle, dilating and contracting, in a peculiar vermicular way, sixty or seventy times in a minute, and being about the size of two large thumbs of an adult, but made once and a half as large by the young man "holding his breath," and thus allowing the auricle to be engorged. The diastole and systole are alternate, but not of that steam-engine piston character, up and down, as is usually thought; the period of the dilatation is longer than that of the contraction of the tumour or auricle; the dilatation is, however, rapid, with an

apparent tilt of the heart forward, immediately followed by a peculiar vermicular collapse, or emptying of the tumour, impeded only by "holding the breath," or offering an obstacle to the blood going to the lungs. These various points are clear, and what, theoretically, might be expected. Not so, however, the relation of the *sounds* of the heart to the movements of systole and diastole; on which it is curious to perceive (looking through the note-book the young man takes with him) that all the first observers in Europe seem to have had peculiar and not very harmonizing opinions, from those offered by Rokitsansky, Skoda, Bouillaud, Hamernik, Beclard, &c., down to the plessimetric crudities of Piorry, and the fancies of some American friends. The *second* sound is, perhaps, more marked than the first. It is difficult to arrive at any certainty on this and other points, however, as at Guy's Hospital, on Tuesday last, every pupil in the theatre seemed anxious about some one other particular point, no little confusion necessarily ensuing. The two sounds are about equal in duration and force, but passing obliquely towards the base of the heart. The second sound (shall we call it valvular?) is more marked over the pulmonary artery, and to the ear even more superficial than that of the aorta. There did not seem to us any doubt that the impulse of the heart or tumour coincided exactly, not with the pulse at the wrist, as a ventricular phenomenon, but rather with the emptying of the subcutaneous tumour or auricle. We thought we could detect the systole of the right ventricle, agreeing with the phenomena of the opposite ventricle and pulse; a very appreciable interval, it must be remembered, existing between the latter and the exposed right *auricle*.

The movement of the tumour is very marked, as already stated, especially on forced expiration, or "holding the breath." It (the tumour) sometimes appears fluttering or incomplete in its contractions or movements, pointing still further to its auricular character. The shock or impulse, so much a matter of controversy at the British Association and elsewhere, would appear to arise from the diastole of the ventricle, and not the systole, as usually believed. The second sound of the heart is, however, not so well cleared up by the case as the first sound. The second sound is now universally considered to be caused by the *click* of the valves, as suggested to us by Dr. Sieveking, who also examined the case this week. This fact is arrived at, of course, from pathological indications. It will be found interesting to confine the observations made on this young man (E. A. Graux) as much as possible to the first sound, due so much to the impulse or shock of the heart, and synchronous contraction of both ventricles.—*Lancet*, June 23, 1855.

[We subjoin some further particulars of this very interesting and remarkable case from the *Gazette des Hôpitaux*, the patient having visited Paris, and been examined by the physiologists of that capital. The man proposes, we learn, to shortly come to this country to exhibit himself here.]

Alexander Graux, 25 years of age, presented himself before M. ARAN, exhibiting a malformation of the sternum, consisting in an almost entire division of that bone along its mesial line.

In the whole of the region naturally occupied by the sternum there existed a depression or gutter-like cleft, its walls formed by the integument only. This depression was about three and a half inches in length, measuring it vertically from a line drawn from one clavicle to the other, and assumed a triangular shape, the base above in the space between the two clavicles, and the apex below. At its base it measured about two-thirds of an inch, and at its apex about an eighth. The dimensions of this furrow could be considerably increased at the pleasure of the patient by putting his pectoral muscles into strong action, the integument covering it being then stretched so as to occupy the same level as the rest of the wall of the thorax, the depression being entirely effaced.

When in a state of repose, an osseous rim, evidently formed by the two halves of the sternum, was distinctly seen on either side of the cleft, and to this the ribs were connected in the usual manner. This malformation entailed some considerable modification in the relations of the osseous and muscular structures of the neck and chest. The clavicles, natural in their curve and structure, terminate in a large extremity above either half of the sternum, without any trace of a sterno-clavicular articulation. The sterno-cleido mastoid mus-

cles arise from the clavicle only; properly speaking they are cleido-mastoid, the right sterno-hyoid and thyroid are absent, but on the left side they can be seen, during a deep inspiration, resembling two prominent cords. The respiratory functions are no less considerably modified by this malformation. An ordinary inspiration is at once diaphragmatic and superior costal, with this peculiarity, that the diaphragm acts first, the upper ribs rising after the commencement of the recoil of the abdominal walls. During a deep inspiration the chest is raised and enlarged by the trapezius and sterno-mastoid on either side, while there is no proportionate increase in the action of the diaphragm and upper ribs. There is nothing remarkable in an ordinary inspiration, but during a forced one the chest and the abdomen retract, the veins of the neck become conspicuous, some of the intercostal spaces become convex on the outer surface, and then the fissure commences to rise from its middle upwards until the integument becomes level with, or even projects beyond the level of the neighbouring structures.

Owing to this malformation, the mode of performing some of the functions of the heart can be studied through the thin covering of integument. About the middle of the fissure an oblong tumour alternately dilates and contracts from sixty to sixty-four times in the minute. When most dilated, the tumour is about two inches in its greatest or vertical diameter, and it diminishes from one-half to two-thirds when contracting from above downwards; and from the right to the left side, the dilating and the contracting alternate, and the former occupies rather more time than the latter. This pulsating tumour is relatively less resonant on percussion than the neighbouring parts, and this dullness is continuous with that of the ventricles, or of the heart properly so called, measuring three inches from side to side, reckoning from the mesial line, and three and a half from above downwards. At the level of the tumour the two sounds of the heart are very loud and clear, especially the second. At the boundary of the ventricular dullness, which extends beyond the level of the impulse, the two are equally loud, clear, and nearly of the same duration; but ascending obliquely towards the base, the second sound becomes stronger and clearer, and near the sternum the first acquires a blowing character. The second sound derived from the pulmonary artery is more distinct and superficial than that of the aorta. When the hand is placed upon the spot at which the impulse is most distinctly felt, viz: in the fourth intercostal space close to the nipple, when the patient is lying down, or a little below the fifth rib if he is standing, this impulse is found exactly to coincide with the commencement of the contraction of the tumour. If the fingers are placed on the radial or the carotid while the eye attentively watches the movements of the subcutaneous tumour, an appreciable interval is noticed between its contraction and the dilating of the artery, the latter being always consecutive.

Such are the principal peculiarities presented by this malformation. The young man enjoys excellent health, though of a feeble appearance. For the present we record the phenomenon without seeking to deduce from it any argument in reference to the many different theories respecting the movements of the heart, and we abstain from bringing forward the diverse explanations which have been already offered.

A commission composed of MM. Aran, Beau, Béheir, Bouvier, Hérard, and Monneret, has been requested to report upon the subject to the Medical Society of the Hospitals.

2. *Production of Sugar illustrated by Pathological Facts.*—In a memoir recently read at the Imperial Academy of Sciences, M. ANDRAL furnishes an account of some observations he has had occasion to make on diabetic patients. First, as to the effect of a deprivation of food. When a diabetic patient has ceased, from any cause, to take food, M. Andral has found the sugar of the urine diminish or disappear. Thus, a woman whose urine furnished from 40 to 70 grammes of sugar per litre, lost her appetite, and at last took no food at all. The sugar kept diminishing under a diminished diet, from 54 to 28 grammes per litre, and 48 hours after abstinence not an atom of sugar could be detected.

Only three days after a gradual return to food did the sugar again begin to show itself (20 grammes per litre), but after then it rapidly regained its old proportion.

Next, in the absence of these aliments which chemistry can transform into saccharine matter, can sugar be produced in the organism from exclusively albuminous aliment? M. Bernard declares that he has found sugar in the liver and hepatic veins of dogs kept exclusively on meat, and clinical observations confirm his statement. Although by the abstraction of all saccharine and amylaceous food we may temporarily diminish the quantity of sugar in the urine, we cannot, in the immense majority of cases, reduce it to zero, or, at all events, can only do so temporarily. Nay, even under an exclusively animal regimen we may see it continually increase. A woman rigorously kept to an exclusively meat regimen for two months, without ever missing a day, taking for drink water, to which a little alcohol was added; at the end of this period, being no better, and disgusted with her food, she abandoned the trial. When she commenced this regimen, the urine furnished 27 grammes of sugar per litre, and at first the proportion diminished to 20, 15, 12, and 10 litres. Then, quite suddenly, without any change in the regimen, it mounted up from 10 to 20, 30, 44, and 49 grammes. When the animal regimen was combined with eggs, milk, and a little bread and vegetables, and some wine and water, it was found, contrary to all expectation, that the sugar again descended to 30, 26, and 15 grammes; but, in three weeks, had reached 54 grammes. Thus, the important conclusion is deducible from this patient's case, that all sudden changes of diet, even of an opposite kind, are attended by a temporary diminution of the sugar, to be followed by an increase. M. Andral has met with several similar cases. Not long since, he had a patient whose urine, while he was on exclusive animal diet, furnished 82 grammes per litre; and as he passed 8 litres of urine in the twenty-four hours, he excreted in that time 656 grammes.

If, as is deducible from Bernard's experiments, the blood which comes from the liver charged with sugar contains this no longer when it has traversed the lungs, does the sugar found in the diabetic arise from the formation of an excessive quantity by a diseased liver, which escapes the action of the lungs, or has this latter organ undergone such alteration as allows the sugar reaching it in the hepatic blood passing intact? We do not, however, observe in the lungs of the diabetic any special alteration. The tubercles so commonly found in them cannot be so regarded, as sugar is not usually found in the urine in phthisis; while the statement made by Reynoso, that sugar is found in the urine whenever the respiration is embarrassed, requires confirmation. Nor do we habitually find sugar in the urine of individuals attacked with any of the diseases of the liver we are acquainted with. But while the lungs of the diabetic do not present anything special, this is not the case with regard to the liver. Since the publication of Bernard's researches, M. Andral has made five autopsies in diabetic patients, and the liver in all has presented similar alterations, viz: a deep brownish-red colour, so that the entire organ, in place of presenting the yellow and red substances of the normal state, exhibited a perfectly uniform red colour. There were all the anatomical characteristics of a very intense hyperæmia, presenting a very different appearance to the hyperæmiæ that, under the influence of very different causes, are so easily produced in this organ. The constancy of this complete engorgement of the liver is a proof of its importance; and if the organ secretes sugar, it is logical to admit that this hepatic hyperæmia is the anatomical sign of the increased activity in the glucogenic function. It must not be said that the azotized and substantial food given to these patients is the cause of the hyperæmia, for in two of the cases alimentation was nearly as in ordinary. But as every hepatic congestion is not attended by the increased production of sugar, the most common effect of such being to diffuse the elements of the bile through all parts of the organism, we may, perhaps, find the explanation of this fact in the difference of the seat of the congestion. Is it not, in fact, possible that, accordingly as this or that anatomical element, or order of capillary vessels, is more specially congested, sometimes a change in the secretion of bile, sometimes in that of sugar, some-

times a modification of some other organic action of which the liver may also be the instrument, may take place?—*Medical Times and Gazette*, Sept. 1, from *L'Union Médicale*, 1855, No. 89.

3. *Experiments on the Existence of Sugar in the Urine of the Fœtus*.—From the experiments of Dr. Wm. D. MOORE, which are, however, of limited extent, it appears (*Dublin Quarterly Journal of Medical Science*, Aug. 1855) that fetal urine is an albuminous fluid of feeble reaction, free from sugar, containing some of the usual salts of the urine, abounding in a highly nitrogenized principle, probably allantoin, but affording no urea, and depositing a most remarkably large amount of nucleated basement epithelium.

4. *On the extensive Diffusion and Frequency of Starch-corpuscles in the Tissues of the Human Body*.—Two years since, Virchow announced his discovery of the presence of cellulose in the brain and spinal cord of man. (See this Journal for April, 1855, p. 466.) He afterwards also found it in the olfactory and optic nerves, and in a waxy spleen. (*Ibid.*) Subsequently, Rokitansky observed it in the optic nerve, and Kölliker in the retina. M. Busk, who later examined the subject, showed that the so-called cellulose corpuscles of Virchow were absolutely identical, in every property, with starch. (*Ibid.*, for July, 1854, p. 210.) Mr. Stratford, of Toronto, has more recently found this substance in the blood of an epileptic patient. Mr. T. A. CARTER has still more recently investigated this subject, and detected these amyloid corpuscles in the cerebrum, cerebellum, liver, spleen, lung, kidney, striped muscular and areolar tissues of the human subject and of the sheep, and in some of the organs of the ox; and also in various secretions and products of the body, in their normal as well as abnormal conditions. Thus, he has “seen starch in healthy blood drawn from the finger; in pus, cancer juice, chronic lymph, bronchitic sputum, ichthyosis scales, tubercular matter from the lung, and in the urine (both alkaline and acid) of patients labouring under divers maladies—Bright’s disease, dyspepsia, acute articular rheumatism, pneumonia, etc. In some instances, it was associated with oxalates and phosphates; in others, with uric acid and urates; and, again, as in the case of Bright’s disease, and of a convalescent from rheumatism, with no deposit, save an almost inappreciable amount of epithelial debris.”

“It may appear astonishing, or perhaps incredible to many,” he says, “that a substance so universally distributed should for such a long period have remained almost unnoticed by microscopical observers. The solution of this apparent enigma will, however, appear sufficiently simple when fully explained. It is well known to those who have applied themselves to the histological study of the various organs and tissues, that fatty matters, frequently in the form of oil-globules, are almost invariably interspersed among the morphological elements of which they are composed. Now, to these globules the animal starch-corpuscle bears a very marked resemblance, not merely in general form, but likewise in refractile properties. Can it, then, be looked upon as a matter of surprise that the two have been confounded by those who have neither by chemical nor other means endeavoured to discover their nature? In some instances, when recognized, as they often have been in the urine, they were considered either as fraudulent additions to, or accidental admixtures with, this excretion; possibly with correctness in some cases, though by no means in all, as I have satisfied myself by experiments the most rigid.

“Professor Bennett has pointed out to me the observations made by Schrant on Colloid-corpuscles, and, from the figures published by that observer, it would appear very probable that these also must occasionally have been mistaken for those of starch.”

“If, then, the facts here recorded be taken for granted, the question immediately suggests itself—are starch-granules, when met with in the localities

¹ “The great majority of the above cases have occurred in the clinical wards of Prof. Bennett, to whom I have shown these bodies in the tissues and fluids referred to, and who has confirmed their presence.”

² “*De Colloid-metamorphose der cel.*”

enumerated, the offspring of pathological causes, or the results of the healthy action of the economy? The majority of cases cited clearly show that they must generally be esteemed as physiological products, although, as with the other constituents of the healthy body, an excess or diminution of them, or an *error loci*, may induce or constitute disease, and will probably hereafter be found to do so.

"Regarding their functions, little can in the present state of our knowledge be said, except, perhaps, that they may be looked upon as thermogenic magazines, analogous to, but not identical in action with, the fat cell. We may, indeed, suppose, from their easy convertibility into grape sugar, and of this, by oxidation, into carbonic acid and water, that from them caloric is generated when a sudden demand is made for it by the system. Another, and a totally different, view may be taken of the purpose they subserve in the economy, and one which does not carry us beyond the limits of possibility, viz: that instead of being converted into the compounds previously mentioned, they are simply metamorphosed into the lactic acid of the gastric secretion."—*Edinburgh Medical Journal*, Aug. 1855.

MATERIA MEDICA AND PHARMACY.

5. *On the Physiological Operation of Delphinia, Veratria, Nicotia, and Aconitia*.—Dr. VAN PRAAG has been occupied in a series of experiments upon the lower animals, with a view to the elucidation of the physiological action of the above-named alkaloids. The following is a summary of his results:—

1. *Delphinia*.—Administered to dogs, cats, and rabbits, it paralyzed the nerves of motion and sensation, and, at a later period, those of secretion; and, lastly, produced death from paralysis of the heart or asphyxia. When injected into a vein, the animals gasped for breath, in consequence of the arrest of the respiratory movements; and, in a very short time, death ensued from cessation of the heart's action. When an alcoholic solution was injected into the anus, staggering walk, adynamia, and anæsthesia were produced. The respiration was first increased and panting; then difficult, from accumulation of mucus in the larynx; and, later, its frequency decreased, until it completely ceased. During the action of the poison, both dogs and cats exhibited signs of tickling sensations in the mouth. After death, the membranes of the brain were found filled with blood, as also the heart, larger venous trunks, and the liver. The gall-bladder was full, and there was a collection of mucus in the air-passages.

2. *Veratria*.—When a minute portion was placed upon the tongue, it was observed to be bitter, and it occasioned a persistent scraping sensation in the throat. It produced tickling of the nose and sneezing for a whole hour after the application of a similar quantity to the Schneiderian membrane. When applied to the integument, it produced a peculiar pricking sensation, with a feeling of cold in the part. A dilute watery solution of the acetate, applied to the umbilical region, produced almost immediately intolerable pain, as if the part were pierced with red-hot needles. Administered to the lower animals, Dr. Van Praag believes it operates much like delphinia. The respiration and circulation are lowered; the muscles lose their tone; and the irritability of many of the nerves, especially the cutaneous nerves, is much reduced. On the other hand, even very small doses induce vomiting, and often diarrhoea; though the latter more frequently arises only after larger doses. The secretion of urine is not remarkably affected, but that of saliva is greatly increased. The stage, of irritation is marked by accelerated respiration and pulse, tonic and clonic muscular spasm, and increased nervous irritability. He believes the tetanic stiffening of the limbs, passing into a dancing movement of them, to be quite peculiar to poisoning by this alkaloid. Death arises from paralysis of the spinal cord. The operation of veratria on the human subject appears, from Van

Praag's observations, to be similar. The pulse is early and remarkably reduced in frequency. He found its efficacy in the relief of pain in prosopalgia, spasm of the stomach, and rheumatic lumbago, to be but temporary; but suggests its use in febrile diseases with augmented muscular tonicity, *e. g.* pneumonia, pleurisy, and inflammatory cardiac diseases.

3. *Nicotia*.—The respiratory movements are first increased, but subsequently, though sometimes at a late period, retarded; and the respiratory acts are accompanied by a sibilus, which Van Praag refers to a spasmodic contraction of the muscles of the larynx. The pulse is first increased, and subsequently reduced in frequency. In those instances of poisoning which do not proceed too rapidly, severe and frequently alternating tonic and clonic spasms attack different parts of the body, either simultaneously or consecutively—a state which is followed by great debility and tremor. In rapid cases, adynamia, with tremor, may set in at once. The occurrence of anæsthesia was not constant. In all the cases, the pupils were first dilated; and in some, at a later period, contracted. The urine was unaffected, but salivation frequently occurred. Van Praag sums up its physiological action as being at first stimulant, but finally depressing, to the circulating, respiratory, and nervous systems.

4. *Aconitia*.—As administered to the lower animals, Van Praag concludes that it exercises a retarding influence upon the respiration, a paralyzing operation upon the voluntary muscles, and a depressing influence upon the brain; and, as respects the circulation, he concludes that it varies greatly in the frequency with which it induces retardation of the pulse. It causes dilatation of the pupil. He regards salivation and increased secretion of urine as among the less constant symptoms of its operation. The animals died at last of exhaustion; if suddenly, it was by asphyxia.

It is to be observed that Van Praag's observations do not in all points correspond with those of Schroff (*Prüger Vierteljahrsschrift für die praktische Heilkunde*, Bd. xi.), who concludes, from experiments made on healthy men and on rabbits, that, in adequate doses, aconitia increases the secretion of the urine; and that the action of the heart is either immediately depressed, or after a brief period of exalted action. When large doses are given, he regards these effects as invariable.—*Association Med. Journ.*, Aug. 17, from *Virchow's Archiv*.

6. *Therapeutic Properties of Carbazotic Acid*.—The carbazotic acid was discovered by Welter, and is sometimes called Welter's bitters. It has been chiefly studied by Gay Lussac, Chevreul, Piria, and Dumas. It is formed by the action of nitric acid on indigo, aloes, silk, and other organic substances. It forms yellow shining scales, soluble in water, to which it gives an intense yellow colour and bitter taste. The process recommended by Liebig for preparing it consists in boiling ten parts of diluted nitric acid on one of indigo, and adding to the liquid, when cold, a quantity of potassa. The potassa combines with the carbazotic acid and forms carbazotate of potassa; which, in its turn, is decomposed by the addition of another acid, by which the carbazotic is set at liberty, and is deposited in brilliant yellow crystals. It is soluble in alcohol and ether. It unites with salifiable bases, and forms compound salts. The name is derived from its composition, which, according to Liebig, consists of—

Carbon	15 atoms.
Azote	3 "
Oxygen	15 "

In a therapeutic sense, carbazotic acid and the carbazotates may be ranked, according to Dr. T. MORFAT, with the tonics and astringents. Dr. M. has prescribed them in the following cases:—

"The first case in which I use the carbazotic acid was that of a woman suffering from an attack of continued fever, complicated with subacute peritonitis and tympanitis. When the case had progressed to that stage in which the use of tonics and stimulants is indicated, I prescribed grain doses of the remedy three times a day. The woman got well just as if quinine had been administered.

"The next case was that of a woman suffering from anæmia. She was;

recovering under the use of quinine and iron. I suspended the quinine and chalybeate treatment, and gave carbazotate of ammonia, in grain doses, three times a day. She lost ground while taking the carbazotate, and again improved on resuming the quinine and chalybeate remedies.

"The next was that of a child ill of scarlatina maligna. Carbazotic acid and wine were administered. The child died.

"I prescribed it next in a case of chronic eczema; and the patient got well.

"Believing that carbazotic acid, in combination with ammonia and potassa, possessed tonic qualities, I felt inclined to try it in combination with metallic bases; and, at my suggestion, Professor Calvert prepared some carbazotate of iron and carbazotate of zinc. At the time I received them, I had two cases of cephalalgia under my care. One was of long standing. In one, I gave grain doses of carbazotate of zinc; and in the other (that of long standing), I prescribed the same quantity of carbazotate of iron three times a day, in combination with extract of conium. The carbazotate of zinc produced nausea and other unpleasant feelings; and the woman said I had poisoned her. I stopped the carbazotate of zinc, and gave carbazotate of iron, as in the other case. This did not produce any unpleasant effects. Both cases improved: indeed, one case got quite well; and that of long standing got so much better that I have not been called upon to prescribe for it since, and a month or more has elapsed since the remedy was administered. The quantity taken by each patient was thirty-two grains, in a period of fourteen days. In both cases, quinine and iron were tried, in combination with the same extract; and, in the case of long standing, sulphate of zinc and strychnine were taken without relief.

"I shall now read notes of two cases in which the carbazotate of ammonia was eminently successful.

"CASE I.—S. E., aged 17 years, had lately suffered from an attack of continued fever. Diarrhoea continued during his convalescence, and for some time after he had so far recovered as to be able to walk about. On the 21st of May last, I prescribed grain doses of carbazotate of ammonia and gallic acid, with one-sixth of a grain of opium, three times a day. On the 26th, *i. e.*, five days afterwards, I saw him again; and he had improved. On the 2d of June, the remedies were repeated, and continued daily. On the 7th, I reported him cured.

"This young man had not been free from diarrhoea for three months previous to his taking the above medicines; and, during that period, gallic acid, acetate of lead, and opium, were administered without effect. He is now quite well.

"CASE II.—The next case was one of diarrhoea, of eighteen months' standing. The patient was suffering from mentagra or sycosis, of two years' standing. On the 4th of June, he began the same remedies, in similar quantities as in the preceding case. I lost sight of this patient for a time. I saw him, however, five days ago; and he said that he did not return, as the "bowel complaint was quite gone." Acetate of lead and opium had been administered in this case also without any good result. The sycosis is no better.

"There is a circumstance in connection with this subject of much physiological interest; and I feel that I shall not have performed my part if I do not bring it under your notice.

"I have prescribed carbazotic acid and the carbazotates of potassa, ammonia, iron, and zinc, in eight cases; and in four of these, the skin and conjunctiva became yellow during the administration of the remedies. They were completely jaundiced; and I believe that the yellowness was owing to the colouring matter of the remedies having tinged the serum of the blood. The coloration may have been owing to some change produced in the biliary system by the remedies; but I am inclined to the former opinion. The tinge of the skin, and conjunctiva so perfectly resembles jaundice, that the keenest observer would be deceived; and I believe that, if the fact were generally known, there would be jaundiced impostors and maligners without end."—*Assoc. Med. Journ.*, Aug. 10, 1855.

7. *Asparagus as a Diuretic.*—By S. J. JEAFFRESON, M. D.—I have used a tincture of asparagus as a diuretic for sixteen or seventeen years, with increas-

ing confidence in its usefulness and efficacy. I have thought it worth the notice of our Association at this particular period, because the time is now approaching at which any gentleman may easily make the experiment of its use. I use the following formula: Take of dried tops of asparagus, five ounces; proof spirit, two pints. Take of fresh tops of asparagus, five pounds. Bruise and press out the juice; evaporate at a low temperature till reduced to one pint, and strain. Lastly, add a pint of rectified spirit. Mr. Baly, chemist, of Warwick, can furnish any person desirous of trying the tincture with a limited supply, and will be happy to make any quantity that may be ordered of him.

The peculiar odour communicated by this substance to the urine, in a remarkably short time, is perhaps as familiar to the laity as the profession. It was this fact that first led me to think that asparagus might constitute a valuable adjunct to our list of diuretics; if not indeed by virtue of any specific diuretic quality it possessed, at least by its power of directing other agents of acknowledged diuretic power to the kidneys.

On referring to such authorities as fell in my way, I found that, whilst some mentioned asparagus as a diuretic in general terms, without any specific reference to its medicinal administration, others omitted entirely to notice this plant, and some others denied its diuretic properties entirely. It appeared to me evident that any deductions drawn on this subject had been founded entirely on its effects as an article of food, and not upon any direct experiments of its medicinal administration. The fallacy and uselessness of such deductions is sufficiently apparent; upon generalizations so vague, we might have discarded numerous of our best remedies as deleterious, innocuous, or useless.

Suffice it to say that, after some sixteen years' experience, I have found the tincture of asparagus a useful adjunct to our diuretic remedies. In many cases, I have found it possessing direct diuretic properties when taken alone in water; but, in still more instances, I have found it most useful in promoting the diuretic properties of other drugs, as I conceive, by directing them at once to the kidneys. I have repeatedly in my own practice, as also in consultation, simply added from half a drachm to two drachms of tincture of asparagus to each dose of an unsuccessful diuretic, and found that copious diuresis was the result.

Mr. Baly informs me that the exact loss by weight in drying the plant is eleven parts out of twelve; in other words, that twelve parts by weight of the fresh shoots are only equal to one part of dried. I have not tried the infusion of the dry shoots, but should think them worthy of trial.

The tincture of asparagus presents the advantage of being capable of combination, so far as I know by experience, with every diuretic substance in use, be it from the animal, the vegetable, or the mineral kingdom.—*Assoc. Med. Journ.*, May 11, 1855.

8. *Iodized or otherwise Medicated Collodion as an External Application.*—Dr. CHRISTOPHER FLEMING observes (*Dublin Hospital Gazette*, June 15), that "the great value derivable from the external application of iodine and its different preparations is generally admitted. In many cases, unnecessary here to specify, a fluid form is to be preferred, and the more circumscribed the specific action of the remedy, and the more rapid the effect produced, the better. Repeated applications are often as troublesome as they are annoying, and hence the solution of a concentrated preparation, or one sufficiently so, is desirable. It occurred to Dr. F. that the combination of collodion with iodine would answer all practical purposes, and he states that as far as he has yet been enabled to ascertain, the most satisfactory results have arisen from such rapid action of the remedy, even from one application, is superinduced, its requisite limits are accurately circumscribed, and a slow, but decided and persistent cutaneous irritation is secured, accompanied with compression, which has appeared to me to be very beneficial.

"This application is in some cases painful for a time, and care is requisite as regards the relative proportions of the ingredients. Dr. Aldridge has been kind enough to make some experiments, on the quantity of iodine soluble in

collodion, and finds that, although a drachm of solid iodine will readily dissolve in an ounce of that fluid, yet that the crystallization of the iodine interferes with the drying of the collodion, and in consequence of the iodine in contact with the skin being prevented from evaporating by the overlying pellicle of collodion, it is likely that a much smaller quantity of iodine will answer all purposes. My impression at present is, that the proportion of ten or twenty grains to the ounce will suffice as an external application, and that even of this strength it should not be indiscriminately applied. A much less proportion, say of five grains to the ounce of collodion, forms a highly stimulating application when the skin is very delicate, and in particular stages of chronic suppuration, &c., will be found a most efficient remedy, and to the 'vesicant collodion' the addition of even a less quantity of iodine imparts considerable activity, as is well known to veterinary surgeons it does, when mixed with their blistering applications. Dr. Aldridge considers the paper collodion the better menstruum, because it forms a tougher skin. When the skin, however is very soft and yielding, this toughness may be productive of a traction not free from annoyance. This use of 'iodized collodion' will be found extremely manageable in very many forms of tumours, and with or without iodine other agents can be combined with collodion of equal efficacy in reproducing their specific action. Thus, several of the alkaloids are soluble in ether, and, therefore, in collodion, and can be applied locally through its agency. Whilst this paper was passing through the press, Dr. Aldridge was kind enough to prepare for me a solution of atropia, in the chloroform solution of gutta serena; this alkaloid formed almost instantaneously a transparent and almost colourless solution in the menstruum, without the aid of any acid. I painted his upper eyelid with some of this solution, and in a very few minutes the pupil was manifestly dilated. Through Dr. Aldridge I contemplate solutions of conea and other alkaloids in collodion, and in chloroform, with similar objects.

"At the same time I may remark, that considerable, and perhaps, for aught I know, equal benefit is derived from the combination of concentrated solutions of extracts of those agents.

"The best mode of applying these remedies is with a camel's-hair brush. It should be rapidly passed over the part, as a thick varnish very quickly forms, and it is curious enough to watch the effects of the rapidly evaporating iodine."

9. *Cod-liver Oil Oleine*.—The utility of cod-liver oil, in a variety of diseases, is well established, but there are some patients in whom it produces dyspeptic symptoms, which forbid its administration. Mr. ARTHUR LEARED, believing that the only constituent of the oil which is available for nutrition is the oleine, determined to try the effects of the latter pure. For this purpose, he had some prepared by submitting cod-liver oil to a very low temperature, and separating the oleine by pressure from the semi-solid mass thus produced. Thus obtained, it is a very liquid, and, at ordinary temperature, a very transparent fluid, agreeing in colour with the oil from which it is prepared.

Its freedom from margarine may be tested by submitting it to as great a degree of cold as possible, when there should be no actual precipitate, but a certain amount of cloudiness usually occurs. Of its exact analysis Mr. L. can only speak conjecturally; that of the oil itself is, however, well known. If, therefore, the good effects of cod-liver oil are largely ascribable to certain substances in combination with it, as iodine and bromine, there is a strong presumption that oleine is superior, as containing a larger proportion of them. Gaduine, a peculiar principle of cod-liver oil to which its efficacy has been also ascribed, according to De Jongh, who discovered it, adheres to the oleine when the margarine is separated, and is isolated only with great difficulty. Gaduine, therefore, will be administered in larger proportion in the oleine than in the oil itself. If, then, in the latter case, oleine is assimilated to the exclusion of margarine, no doubt can exist that oleine is the most active remedy in all cases in which the oil is indicated. Mr. L. maintains that, in certain cases, oleine is alone available, and that its efficacy is at least not inferior to the oil. The question, in its more extended shape, remains to be tested by further clinical experience.—*Med. Times and Gaz.*, July 21, 1855.

10. *The Saoria and Tatzé, two new Remedies for Tænia.*—The *Saoria* is the ripe and dried fruit of the *Maesa Picta*, a native of Abyssinia. The fruit is a drupe, and, when dried and administered in powder, is stated, by M. Schimper, to purge, to kill and expel the worm, and not to affect the health of the patient. M. HEPP (*Bulletin Générale de Thérap.*, July, 1854) records thirteen observations made upon Europeans. In eight of these cases, those in which a worm was certainly known to be present, the parasite was expelled. Nausea, colicky pains, and purging, and once vomiting, were observed amongst its effects, together with a violet colour imparted to the urine. The dose of seven *grammes* (seven and a half drachms) is to be given in the morning fasting, after a moderate diet on the previous day. It may be given suspended in a liquid. In two or three hours, liquid stools will ordinarily occur, in which the *tænia* will be found dead.

The *Tatzé* is the fruit of the *Myrsina Africana*, a native of Abyssinia, etc. It is a more disagreeable remedy than the *saoria*. Its purgative operation is not constant. It renders the urine inky. It expelled the *tænia* in all of six cases in which it was given. The dose of the powder is *fifteen grammes* (one drachm and three quarters), followed, if requisite, by a dose of castor oil.—*Association Med. Journ.*, Aug. 17, 1855.

11. *The Chaulmoogra.*—This drug, to which attention has been drawn by Dr. MOUT (*Indian Annals*, April, 1854), is the fruit of the *Chaulmoogra*, or *Gynocardia odorata*, and is indigenous to the Sylhet district of India. The seed, inclosed in a succulent and indehiscent pericarp, is dried and subjected to expression, and a bland fixed oil is thus obtained, which has been long known and prized by the natives and travelling faquirs as a remedy for leprosy. Dr. Mout relates some cases in which he has used it. One is a case of leprosy. Six grains of the seed were administered three times a day, and the ulcers were dressed with the oil; rapid improvement followed this treatment. He has used it also in scrofulous glandular enlargements, constitutional syphilis, in a mild case of ichthyosis, in elephantiasis of the face, etc., with benefit.

Dr. HOBSON (*Medical Times and Gazette*, March, 1855), who practised among the Chinese in Canton, has also found the *chaulmoogra* of real service in leprosy. He has seen two cases cured and several much benefited by a steady employment of the drug. He gives forty grains twice a day, and rubs the eruption occasionally with the oil.—*Association Med. Journ.*, Aug. 17, 1855.

12. *Preparation of the Caustic of Landolfi.*—M. QUEVENNE recommends the preparation of the caustic of Landolfi with—

Chloride of zinc (deliquesced),
Chloride of antimony (deliquesced),
Chloride of gold,
Chloride of bromine, of each 5 *grammes* (1½ drachm),
Flour, 20 *grammes* (5 drachms),
Water, 18 *grammes* (4½ drachms).

The chloride of gold is to be triturated in a porcelain mortar, with the chlorides of zinc and antimony; the water and half the flour are then to be added, so as to make a somewhat liquid paste; the chloride of bromine is then added, and the whole mixed as quickly as possible with the rest of the flour. The operation should be performed in the open air, on account of the vapours of bromine disengaged. The chloride of gold may be omitted without injury to the caustic.—*Association Med. Journ.*, Aug. 17, from *Rev. Méd.-Chir.*, April, 1855.

13. *Impurity of Commercial Oxide of Zinc.*—The pure oxide of zinc, prepared according to the directions of the London College, is a yellowish white powder; but the prejudice in favour of a perfectly white preparation has led to its sophistication in various ways. Mr. REDWOOD states, as the result of his inquiries (*Pharmaceutical Journal*, January, 1855), that the greater part of what is sold as oxide of zinc at the present time is nothing but *basic sulphate of zinc*, and is prepared by adding caustic ammonia to a solution of sulphate of zinc;

but, as oxide of zinc is soluble in ammonia, it is necessary to avoid adding excess of ammonia; under such circumstances, the oxide of zinc retains a considerable portion of sulphuric acid and water of hydration. Some years ago, the so-called oxide of commerce was nothing else than *carbonate of zinc*, and this is still sometimes met with. It arises out of omitting the calcination, which would have converted it into oxide. He mentions, also, *basic chloride of zinc*. He asserts that oxide of zinc, prepared by combustion, is not, at the present time, a commercial article among druggists.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

14. *On the Epidemic Smallpox which occurred in the Island of Jamaica in 1851-52.*—Dr. SEATON read before the Epidemiological Society, July 2d last, an interesting account of this epidemic. He stated that he had drawn up this account from the returns which had been made by various medical practitioners in the island to questions circulated by the Central Board of Health there in 1852. Copies of these returns were forwarded to the Smallpox and Vaccination Committee of the Society, and were found to contain much interesting matter on the general subject of vaccination in hot climates, as well as a pretty complete history of this particular epidemic of smallpox up to the autumn of 1852. It was of this history only that Dr. Seaton treated in the present communication. The epidemic commenced in July, 1851, in the northern part of the island, on the Hyde Estate, in the county of Trelawney, where it appeared to have been imported by some African emigrants. There had been no epidemic of smallpox in Jamaica since the year 1831. The circumstances attending its importation were fully detailed. From the Hyde Estate it was communicated to other parts of the county of Trelawney, to St. Ann's adjoining Trelawney on the east, and to St. James' adjoining it on the west. It was traced successively to Portland, Kingston, Spanish Town, Vere, Clarendon, Hanover, Westmoreland. It still existed in Westmoreland, and indeed was just commencing at Savanna-la-Mer, when the returns were made to the Central Board. The author then entered into a variety of details, showing the remarkable extent to which vaccination had been neglected in Jamaica, there being the greatest possible difficulty in getting the people to be vaccinated, excepting under the terror of an impending epidemic of smallpox, when they anxiously sought the protection. One practitioner, for example, who, in a residence of sixteen years, had not vaccinated more than 200 persons, operated on upward of 3,000 in a short time after the outbreak, and the testimony of all the others was to the like effect. Under these circumstances, it was to be expected that the epidemic would be wide-spread and fatal; and as if the natural mode of diffusion was not sufficient, in some parts of the island the disease was propagated by inoculation, till this practice was made penal by an order of the Council. Dr. Seaton then proceeded to notice the principal features of the epidemic under the various heads of the period of incubation, duration of the precursory fever, course of the eruption, period at which death took place in fatal cases, complications, treatment, &c. &c. The principal complication appears to have been diarrhoea or dysentery. Measles were seen along with smallpox by Dr. Bowerbank, of Spanish Town, and Dr. Chamberlaine, of Kingston; and many cases were seen by various practitioners, in which the diseases succeeded each other. With regard to the susceptibility to smallpox manifested by the various races, the immense majority of cases occurred in the negro race; but the reasons assigned for this are, that this race is in overwhelming preponderance, and that the white race almost universally, and the coloured population very generally, are protected by vaccination. So far as observation of this epidemic and the opinions of the present reporters go, they would not support the view that the negro

race is liable to any malignancy of type, except from extraneous circumstances, as their extraordinary filth, crowding, and the like. On the protective value of vaccination, the returns give important information. They show that in a large number of cases, reputed to be vaccinated, there was a total absence of evidence that the vaccination had been effective, and that the same difficulty is experienced in Jamaica which has been felt in this country in securing the proper inspection of the vaccinated, and even to a much greater extent. Dr. Clark, of Armotto Bay, gives an account of the results of his inquiries respecting 459 persons who had been subjected to the vaccine puncture, and who resided in localities which for months were the seat of epidemic smallpox. Eight only of these were reputed to have had smallpox. In six of them it was satisfactorily ascertained that no vesicle had followed the operation; the other two had left the neighborhood before the inquiry was instituted. Instances were given of localities well vaccinated enjoying entire immunity from the disease. Abundant evidence was also furnished, in the returns, of the careless manner in which vaccination was often performed, and of the extent to which improper lymph was used, giving rise frequently to spurious results; yet when smallpox afterwards occurred, it was put down as "after vaccination." On the subject of the modifying power of vaccination, the most valuable information was given by Dr. Bowerbank. He had kept a record of 301 cases of smallpox attended by himself. Of these, 241 had never been vaccinated, and of them forty-five died, or more than eighteen per cent.; while of fifty-eight who had been vaccinated, only two died; and of two who had been inoculated, one died. In one of the two deaths said to be after vaccination, there was no evidence of the vaccination; in the other, the smallpox succeeded to measles, and was complicated with dysentery, which was, in fact, the fatal disease. Of the fifty-eight vaccinated, in thirty-four there were good cicatrices, in twelve imperfect cicatrices, and in twelve none at all. Of the 241 unvaccinated, the disease was confluent in fifty-one, discrete in 183, modified in seven. Of the thirty-four vaccinated, with good cicatrices, it was confluent in two, discrete in fourteen, modified in eighteen. Of the twelve with imperfect cicatrices, it was confluent in none, discrete in eleven, modified in one. Of the twelve without cicatrices, it was confluent in three, discrete in nine, modified in none. With regard to race, of the 241 unvaccinated, there were five whites, with one death, ninety-eight coloured, with eighteen deaths, and 138 negroes, with twenty-six deaths. The ratio of mortality, therefore, in all the races, would appear to have been the same. The results recorded by Dr. Turner, of Spanish Town, were generally in accordance with those obtained by Dr. Bowerbank. He also saw cases of smallpox in people who had been inoculated, and in others who had had the natural disease. As the general result of their experience, the medical practitioners of Jamaica expressed, without exception, their unabated confidence in the protective value of efficient vaccination, and, with one exception, their opinion, that inoculation is both inexpedient and unjustifiable. Even the exception is only apparent, as the practitioner who thinks the practice sometimes advisable would restrict it to cases in which a large number of persons susceptible of smallpox collected together, without there being a supply of reliable vaccine lymph. These conclusions, he (Dr. Seaton) observed, were entirely at variance with the extraordinary statement promulgated a few years ago by Dr. Copland, that in hot climates and dark varieties of the human species vaccination had been demonstrated to be inefficacious—a statement most mischievous if not true, and for which he had never been able to find any foundation, either in published books or in documents which he had been able to consult.

Dr. STEWART, Superintendent-general of Vaccination in Calcutta, entirely concurred in the views and opinions enunciated by the author of the paper, and said that nothing he had listened to had given him greater satisfaction.

Dr. STEWART did not attribute the occasional failure of the protective power of vaccination to deterioration of the lymph, but to a neglect in the proper performance of the operation, the indiscriminate selection of subjects, and the want of care with regard to seasons, and due watchfulness over the cases that were operated on.

15. *On Cholera in various parts of Germany.*—Dr. VARRENHAPP, of Frankfort, in a paper sent to the Epidemiological Society (read Aug. 6, 1855), states that in the various epidemics of cholera by which Germany has been invaded since 1831, the southwestern part of that territory has invariably escaped. The northern part of Germany (particularly the kingdom of Prussia) has repeatedly suffered; the southeastern part likewise—Bohemia, for instance; and Vienna itself. There was only one irruption in the southwestern part—i. e., Bavaria, Wurtemberg, Baden, Hesse Darmstadt, Nassau, Frankfort, and the southern portion of the Saxon duchies. Northern Germany, especially Prussia, Mecklenburg, and Holstein, were visited by cholera in the summer of 1849. The disease, on this occasion, passed through this part of Germany from east to west, and when it had reached the Rhine it turned southward. It took a very particular course, and presented a peculiarity in its progress which, so far as the author is aware, has not as yet been noticed by any one. The cholera passed gradually up the Rhine, and there is nothing very remarkable in its spreading in that direction, the Rhine being, as everybody knows, one of the most frequented lines of communication that can exist. There are large and wealthy commercial towns, and a rich agricultural population, on both sides of this river, with good public roads, besides the traffic by steamers on the Rhine itself. But the cholera, according to the author, did not take this path, because there was a constant intercourse and communication of people, but followed merely the valley of the Rhine. Hence, concludes the author, there must have been, at the period in question, very decisive influences, either of a local telluric nature or some other agency, which induced the disease to take this course, and prevented it from deviating either to the right or left, in spite of there being in one of these directions a communication quite as great as in any part of the Rhine itself. Cholera prevailed at Coblenz, but it did not pass up the Lahn to Ems, although these places are distant only five miles, and in summer there is considerable intercourse between them. There was cholera at Bingen, but it did not reach Creusnach, about nine miles distant, and there is active communication between the two places. Cholera was also at Mannheim, but it did not go up the Neckar to Heidelberg, about ten miles distance, although Heidelberg has with no other place so much communication as with Mannheim. These data, says the author, are very important. The cholera evidently followed the valley of the Rhine, but it did not propagate itself to the country on either side. It did not follow merely the course of the water—at least it did not go up any of the secondary rivers just mentioned, although on their banks there is a constantly moving population. The cholera, then, in its propagation from Cologne, did not follow the great line of communication, as far as this was a communication by land, but it followed the line of communication by water, merely as related to the principal valley of the Rhine, and did not enter any lateral valley. The author, besides giving a series of cases illustrative of the fact that a place may have the most active intercourse with another place infected with cholera, and yet remain free from that disease, adduces strong evidence, based upon the first cases of cholera at Frankfort, in favour of the transmissibility of the cholera poison through the medium of soiled linen. The continuous line of propagation by this means appeared so complete; and he adds: “These cases, and others of a similar nature, have convinced the physicians of this town—even those who formerly had been absolutely anticontagionists—that the contagion is not, perhaps, the principal mode of propagating this disease, yet there exist undeniable cases of contagion, and that it seems the excrements on the linen and clothes, particularly when polluted, are capable of propagating the disease.”

16. *On the Premonitory Diarrhœa of Cholera.*—GEORGE TODD, Esq., in a paper read before the Epidemiological Society (Aug. 6, 1855), states that it has been a generally observed fact, ever since epidemic cholera became known to the medical profession, that the greater proportion of cases are preceded by a distinct premonitory stage, varying, however, in duration and intensity. Cases, however, do occur in which the premonitory stage is of such short duration as not to attract notice; and such cases have frequently been brought before the

medical profession as proofs of cholera without any preceding diarrhoea. Careful inquiry of all such cases Mr. Todd considers will establish the existence of a well-marked premonition. In favour of this proposition the author adduced the investigation, by Dr. M'Loughlin, of twenty-one cases of cholera without premonitory diarrhoea contained in the Returns of the Registrar-General between July, 1853, and July, 1854, the results of which, according to Dr. M'Loughlin, are that fifteen of the twenty-one cases had had a diarrhoea of some hours' duration before the accession of cholera; and that the remaining six cases were either not cases of cholera, or cases where nothing was known of their previous history. The author calls in to the support of his views, in addition to his own experience, that of various writers on cholera in this country, in various parts of Europe, and India.

17. *On the Nature of the Change known as Fatty Liver.*—The following are M. A. LEREBoullet's conclusion in regard to this:—

"1. The fatty degeneration of the liver is due to the accumulation of fat in the biliary cells themselves. 2. Special fatty cells are not formed, as biliary cells would then be found amidst the fatty ones, which is not the case. 3. Nothing authorizes us to admit that fat becomes developed in the interstices external to the cells. 4. The biliary cells may, by the accumulation of fat, acquire double or triple their normal volume, this development of the cells explaining the increased size of the fatty liver. 5. These cells entirely lose their secretory character, and no longer contain biliary granules; the biliary secretion is obstructed, and the contracted gall-bladder contains but little bile. 6. The fatty degeneration induces a decolorized state of the liver, which progresses from the periphery towards the centre of a lobule, giving the organ a spotted and reticulated appearance. 7. The decoloration arises from the development of the fatty cells compressing the portal vesicles, and impeding the circulation in them. 8. In the artificial fattening of geese, the liver only becomes loaded with fat after the other organs of the body, and especially the abdominal viscera, have become saturated with it. 9. The cells of the liver of fattened geese differ from pathological fat cells, inasmuch as the fat that fills the former always retains the form of distinct droplets, accumulated in the cell, to which they give an irregular appearance on distension; while in the pathological cells the fat becomes united into larger and larger drops, until the cell is at last distended by a single one like a balloon. 10. The fatty cells in the goose resemble, as regards the disposition of the fat in the interior, the physiological fatty cells of the foetus and those of the lower animals. 11. The nuclei of the normal cells, as well as the biliary granules, disappear when the fatty degeneration commences. 12. The degeneration takes place simultaneously throughout the organ, but all the fatty cells do not present the same degree of development. 13. This change of biliary into fatty cells is observed in tuberculosis, cancer, cirrhosis of the liver, &c. 14. The deposition of fat in the cells appears to be closely connected with a diminution of the nutritive process, and consequently of organic combustion, which is the primary condition of that process. When the quantity of oxygen absorbed is less than in the normal state (as in tuberculosis, cancer, and probably all diseases of nutrition); or, when the respiratory elements (fecula, &c.) are taken in too large proportions, the combustion of these substances is incomplete, and the chemical elements which enter into their composition combine so as to form fat, which is deposited in the biliary cells." (Tom. xvii. p. 496.)—*B. and F. Med. Chirurg. Rev.*, July, 1855, from *Mémoires de l'Acad. Impériale de Méd.*

18. *On certain Conditions in which Sounds originating in the Roots of the Bronchi are transmitted to distant parts of the Chest.*—M. E. BARTHEZ, physician to l'Hôpital Ste. Eugénie, read to the Academy of Medicine a communication on this subject, May 29th last. After adverting to the importance of an acquaintance with such transmitted sounds as being deceptive in their tendency, and apt to lead to serious errors in diagnosis and treatment, he remarks, in conclusion, that—

The normal and abnormal sounds produced in the large aerial trunks can be

transmitted across the chest, and perceived at a point remote from their place of emission.

The means of transmission are substances, solid or liquid. In the first case, the anatomical conditions necessary are—

1. Solidity and compactness of the substance.
2. Its intimate adherence to the larger bronchi or trachea.
3. Its contact with a part of the chest accessible to the ear.

In the second case these conditions appear to be—

1. The coexistence of a solid body adhering to the bronchi, and establishing a continuity of substance, vibrating between them and the liquid.
2. A particular position of this solid body, due either to costal adhesions or to the considerable quantity of the effusion.

The sounds perceived under these circumstances are frequently augmented in their intensity, so as to simulate bronchial respiration, gurgling, or amphoric respiration, in the absence of all pulmonary excavation, or any communication between the pleura and bronchi.

These pseudo-bronchial, cavernous, and amphoric sounds have a peculiar character. This, as well as their exaggeration, is owing to the passage of the sonorous vibrations across a sort of stethoscope, half solid and half liquid, which modifies the character and intensity of vibrations, usually transmitted by solid bodies alone.

If, says M. Barthez, we add that the transmission of the sounds is more easy when the thoracic cavity is small and narrow, when its walls are thin and dry, and when the respiratory movements are strong and rapid, we shall have indicated the principal circumstances favourable for this transmission of sounds across the chest.—*Edinburgh Medical Journal*, Aug. 1855, from *Gazette Médicale*, June 2, 1855.

19. *Pathological Anatomy of Chronic Hydrocephalus*.—M. BLACHE, in a communication read before the Academy of Medicine, describes the details of the pathological alterations resulting from this disease.

The following are his conclusions:—

1. In internal hydrocephalus, the serum accumulates in the cerebral ventricles, but not in the cerebellar or fourth ventricle; it does not communicate with the cephalo-rachidean liquid.

2. Contrary to the opinion of some authors, the white and the gray substances are recognizable in the nervous laminæ, into which the fluid pressure transforms the ventricular walls; when the distension has not been excessive, the circumvolutions are also to be found, and, in all cases, thickenings, which are the vestiges of them.

3. The corpus callosum, fornix, and septum lucidum, are almost entirely destroyed and converted into fibrous laminæ.

4. The tuberculum annulare, the cerebellum, and the origin of the cranial nerves, preserve their integrity, with the exception of more or less alteration always existing in the optic and olfactory nerves.

5. The ventricular membrane is thickened so much as to be capable of dissection throughout its whole extent, by which it has been established to be continuous with the choroid plexus, and prolonged across the aqueduct of Sylvius and the foramen of Monro.

6. In the interval between the two laminæ, which, by facing each other, form the septum lucidum, it is easy to demonstrate the existence of the fifth ventricle, and its communication with the third.

7. The pituitary body is canaliculated.

8. The anterior orifice of the aqueduct of Sylvius was found obliterated in the two cases where this was examined; the cerebral cavities were thus completely closed.

9. Regarding the nature of the malady, the absence of all softening of the cerebral substance, and the very slightly plastic nature of the fluid effused, discountenance the idea of chronic hydrocephalus being the result of inflammation. In our opinion, says M. Blache, it is a pure and simple hydropsy.—*Edinburgh Medical Journal*, Aug. 1855, from *L'Union Médicale*, May 31, 1855.

20. *Hemorrhagic Hypertrophy of the Spleen.*—M. BARTH recently brought under the notice of the Anatomical Society of Paris a remarkable example of lesion of the spleen. It was taken from the body of a woman aged 61, who had never left Paris, or suffered from intermittent fever. Her complaints were traced back at least two years, but their origin was obscure, and of course gradual. She was emaciated, but did not exhibit any cachectic decoloration, nor had there taken place any hemorrhage. The immediate cause of death was peritonitis, induced by a fall.

At the autopsy, there were found signs of recent and old peritonitis. The spleen was truly enormous, weighing 3,050 grammes, while the normal weight of the organ is about 250 grammes. The splenic vein was found entirely obliterated, being almost entirely converted into a fibrous cord. The short vessels of the organs were atrophied, scarcely a vestige of these being discernible amidst the fibrous adhesions connecting the organ with the stomach, from which organ it was only one centimetre distant. The alteration of the spleen might be termed a hemorrhagic hypertrophy, but its appearance varied at different points. At one part yellowish fibrinous matter was observed; at another, it was the colour of muscular substance. Some parts were observed in a state of mash by the side of hemorrhagic centres, resembling pulmonary apoplexy, or the colourless remains of old apoplexies. In some places, the hemorrhage was found in well-defined centres, in others as diffused apoplexy. The form of the organ was entirely changed, while the consistency was hard, friable, soft, or almost fluid, according to the points touched. There was no hypertrophy of the abdominal glands.

M. Cruveilhier agreed with M. Barth in his belief that the obliteration of the splenic vein had preceded the production of his hemorrhagic hypertrophy. Obliteration of the veins is the cause of hemorrhage in a great number of cases, as he has observed in his experiments upon animals. He has endeavoured to produce obliteration of the sinuses of the dura mater, feeling very certain that such lesion would lead to cerebral hemorrhage; but he has not, as yet, succeeded in effecting this. M. Tonnelé has, however, presented a case in which, with inflammation of the sinuses, there was a large number of apoplectic centres; and in pulmonary apoplexy M. Cruveilhier has found the veins of the lungs the subjects of obliterating phlebitis. These facts go, to a certain extent, to explain the condition of the spleen, especially when it is remembered that it has but one vein. —*Medical Times and Gazette*, August 4, 1855, from *Bulletin de la Société Anatomique*, An. xxix. p. 366.

21. *Coma in Scarlatina.*—Dr. HENRY KENNEDY directed the attention of the College of Physicians in Ireland to the coma which was known to be a frequent attendant on scarlatina. The period of its occurrence was one of the points he would notice, and for this purpose he would speak of it as being observed under two very different aspects, which it was well to keep in mind. In the first class of cases, the coma existed at the commencement of the disease; and here it might exist *per se*, and continue even without intermission till death. Of this he had known instances. In other cases, again, while the coma was a very early symptom, it was attended with convulsions, or, rather, the two occurred alternately. Of this, also, he had seen examples. But the most frequent occurrence was the presence of coma at the onset of the attack, and its gradual subsidence as the disease progressed. This, then, was the first way in which the coma was to be observed; that is, as ushering in the attack. In the second, however, matters were very different, for here the disease begins with the ordinary febrile symptoms, and it is only as it progresses that coma shows itself, and usually goes on increasing till the period of death. He thought it well to draw these distinctions as to the period at which coma comes on; for though ever a serious symptom, and occurring only in the worst forms of the disease, still there was this difference between the two classes of cases—that the first was a more favourable case to have to treat than the last. He had seen several instances where the disease had begun with coma, from which the child had recovered in a few hours. One such he had lately seen with his friend, Dr. Denham, and they were not uncommon. But where the coma appeared at a

later stage, and then only for the first time, matters went on very differently, and a case of recovery was a very exceptional one indeed. The remark applied not only to cases of scarlatina, but to all acute fevers, and more particularly to the ordinary fever of this city; in which latter it was well known that the approach of coma, and still more its actual occurrence and tendency to increase, were amongst the very worst signs which could appear. From profound coma, under the circumstances detailed, he had seen no recovery.

What could be done for the class of cases he had been speaking of? Unfortunately, but too little. Still, there was one remedy from which he had seen benefit when the coma was an early symptom. Though well known, it possibly might be used more generally than it is. He spoke of the cold affusion applied to the head, and under the inspection of the medical attendant. This, used with the ordinary care, and modified, as it could be, by taking the chill off the water, he had found very useful. Even when the coma appeared late, he had seen benefit from its use, but not at all equal to what occurred in the other cases. Many of the cases marked by early coma, on recovering from this state, exhibited symptoms of great depression, and which at once called for some modification of the antiphlogistic plan. Besides wine, he had seen marked benefit arise from the use of barm—a remedy which, though formerly much used, had latterly been nearly forgotten. In the Cork Street Hospital it was, however, still employed by Dr. George Kennedy, and often with benefit.—*Dublin Quarterly Journal of Medical Science*, Aug. 1855.

22. *On Pneumonic Catarrh of Infants.* By M. TROUSSEAU.—Diseases of the lungs present very different characters in infants, according as the children happen to be newly born, at the breast, or growing up. The lung of the newly-born infant becomes hard and impervious to air, hepatized, as in the third stage of pneumonia in adults. This state of impermeability of the lung has been described as a *return to the foetal state*. In a physiological point of view, however, I must declare this to be quite erroneous.

From the first month after birth to the fourteenth, infants are subject to a pulmonary inflammation, called lobular or infantile pneumonia. In this disease the pulmonary catarrh is the chief complaint; the pneumonia is secondary; so in place of calling it catarrhal pneumonia, a name given to it by several practitioners, I prefer the more logical and expressive name of pneumonic catarrh.

Pneumonia is an acute affection, which either resolves quickly or kills rapidly. There is, as it were, a great battle, the issue of which is soon decided on one side or the other. Catarrh (bronchitis), on the contrary, is a series of skirmishes; the attack diminishes one day to be augmented on the morrow, and the child whose lungs are repeatedly attacked, either in the same or different parts, generally succumbs.

Pneumonic catarrh is most frequently caused by a simple cold, a slight febrile attack, or coryza. The mother of the child, although the most affectionate and anxious, sees no cause of uneasiness. Suddenly the fever is lighted up, great oppression comes on, and the child presents soon most alarming symptoms. The face becomes congested in a remarkable manner, the cheeks and lips are livid, the skin is hot and very dry, the dilatation of the *alæ* of the nose very considerable and almost constant; the epigastrium is retracted towards the vertebral column, the diaphragmatic belt becomes more marked, forming what is called the *peripneumonic fissure*. Orthopnoea increases; the pulse gains an extreme frequency, rising to 120, 140, or 160 in the minute. Percussion is a mode of exploration both difficult and uncertain in the diseases of infancy. If, however, the hand be applied flat upon the thorax of the child while crying, it will feel the vibration of the voice much more distinctly upon the diseased than upon the healthy side. When about to auscultate the chest, care must be taken not to alarm the infant; it should be placed upon its nurse's knees, and then the physician can apply his ear to the posterior and lateral regions. A subcrepitant rale is most frequently heard, sometimes preceded by a mucous or sibilant rale; it is not rare to hear crepitation as fine as in the adult. Bronchial breathing is heard during inspiration, but chiefly during expiration; but

it has neither that dryness nor clearness which we observe in the adult; it is heard most frequently not altogether at the base, but at the superior part of the inferior lobe of the lung. The presence of the souffle can perhaps be determined in four or five different parts of the chest. The signs furnished by auscultation are very changeable; thus you may not find to-morrow, in a certain part, the souffle which you heard this evening. This arises from the principal bronchial tube leading to this part of the lung being blocked up with mucus, and thus the air is directed to other parts. In many cases we know but too well that the disease is both of long duration and liable to frequent relapses. A pneumonic catarrh remains fifteen days, a month, six months, or even more. During the course of hooping-cough, you may have acute attacks of it five or six times; never entirely ceasing, the malady becomes chronic, an important differential character from pneumonia of adults.

The anatomical lesions are well marked; besides the inflammation and tumefaction of the bronchial mucous membrane, we see a number of small indurated lobules, between which the tissue of the lung is healthy. The surface of the organ presents a marbled appearance. Independent of these indurated lobules which invade sometimes a portion of a lobe, sometimes the whole lobe, we remark other lobules of a violet red colour, indicating a pneumonic inflammation of the first degree, whilst here and there these masses are harder, and form projections, when the lung is collapsed. The microscope discerns globules of pus in these red lobules. In fine, we find lobules of a yellowish colour in the third stage of inflammation.

Pneumonic catarrh is followed by a special lesion, viz., pulmonary abscess, little cavities filled with pus, which in a single lung sometimes amount to the number of 100, 200, or even 500. It has been supposed that these pulmonary abscesses are nothing more than the dilated vesicles of exaggerated emphysema; but I think that the anfractuositities which are seen in these cavities arise from a number of vesicles much broken up. It is a difficult point often to determine between pulmonary phthisis when the vesicles are filled with pus, and the third stage of pneumonia.—*Dublin Hospital Gazette*, August 1, 1855; *Gazette des Hôpitaux*, July 5, 1855.

23. *Various kinds of Treatment of Pneumonia.*—Dr. ROUTH read before the Medical Society of London (May 5, 1855), an interesting paper "on the experience of various kinds of treatment in pneumonia." The author, after recapitulating the leading points of his former paper, considered, 1. The question as to the influence of bloodletting in the treatment of pneumonia in regard to mortality. He denied the normal mortality from that disease could be accurately given, showing from a table he had collected, that it varied from 3 up to 31 per cent. out of some seven thousand cases. He particularly alluded to age, sex, and complication as affecting mortality. At the extremes of life it was very fatal, but benignant at intermediate periods. It was more fatal among females; and complications of other diseases, chiefly phthisis, and Bright's disease, greatly increased it. Thus a selection of favourable ages only, a diminution of the number of females, in the number of complicated cases would generally diminish the mortality.

2. Dr. Routh then considered the treatment by bloodletting singly, instancing first two series of cases from Bouillaud, which he showed were not fairly selected according to age, sex, and complication. Also two series of cases from Grisolle, in one-third of which only had bloodletting succeeded in curing the disease; in the remaining it had failed, necessitating the conjunction of antimonials; lastly, he alluded to cases similarly treated by Dietel of Vienna; the mean mortality from the bloodletting treatment was 16.5 per cent. Dr. Routh then considered the treatment by bloodletting, combined with tartar-emetic, instancing the cases recorded by Dr. Hughes, of Guy's Hospital, and others occurring in the practice of Drs. Walsh, Peacock, and Taylor. These cases appeared to be in no way selected; indeed, as a rule, very unfavourable, the complicated cases amounting in those of Dr. Hughes to 51 per cent., in the others 53 per cent. He also alluded to some cases similarly treated by Grisolle. The mortality obtained by these gentlemen was—

	Simple Pneumonia.	All cases.
Dr. Hughes	2.2 per cent.	24 per cent.
Drs. Taylor, Peacock, and Walsh	3.2 "	30 "
Grisolle		25.4 "
Mean		26 "

3. He then alluded to the treatment by tartar emetic singly, instancing cases from Louis, and Grisolle, and Dietel, giving a mortality out of 170 cases of 18 per cent. These cases were remarkable as generally recovering with very little loss of strength; and in comparing this kind of treatment with that by tartar emetic and bloodletting, conjoined by bloodletting singly, the result proved that that by bloodletting and tartar emetic conjoined was the most fatal, because the most depressing.

4. Dr. Routh then dwelt on the treatment by chloroform, selecting Varentrapp's cases as the best recorded; but even these were not fairly selected, because containing too small a number of females. The mortality he obtained was 4 per cent., or, including some other cases, which he ought not to have omitted, $11\frac{1}{2}$ per cent. A larger number of cases collected by Vacherer, Baumgartner, and Helbing (193), gave a mortality only of $4\frac{1}{2}$ per cent., but he could not speak as to their assertion, not having been able to find the original documents.

5. Dr. Routh then spoke of the results obtained by dietetic treatment only. These were of two classes, those obtained by homœopaths (*i. e.* in those cases where they had been also diagnosed and investigated by legitimate practitioners) and those obtained by experiments directly made by legitimate practitioners themselves. From Jessier's cases the mortality was 14 per cent.; from Dietel's experiments out of 189 cases so treated, the mortality was 7.4 per cent.; Dr. Todd's treatment was also much less energetic. He discouraged bloodletting and tartar emetic, trusting chiefly to the liquor ammoniæ acetatis, and giving the patient support.

6. Dr. Routh then proceeded to speak of the treatment which he recommended. The indications were, first, to diminish the general fever, especially the increased cutaneous and pulmonary respiration. The former was affected by the tincture of the root of the *aconitum napellus*, on the action of which, in small and poisonous doses, he dwelt at length, and especially in reference to its certainty of action and utility as compared with the ordinary tincture of the Pharmacopœia; the latter indication was effected by oleaceous inunctions which cooled the skin very rapidly.

The second class of indications was to relieve the local symptoms, which was best effected by the employment of Junot's exhausting apparatus, which did all that bloodletting could do, but saved the patient's blood, and by dry cupping, or counter-irritation largely, by turpentine according to Dr. Todd's plan, or blisters followed by repeated dressings of cotton, so as to deprive the system of a large quantity of fluid ingredient. The last class of indications to be fulfilled was that which had reference to the support of the patient. He objected altogether to the *diète absolue* of the French, recommending the ordinary middle diet of hospitals, or beef-tea from the first, to obviate the tendency to death by depression. He occasionally gave small doses of tartar emetic during the first days of the disease, to promote expectoration, and perhaps an alterative mercurial. Under this treatment he had been generally very successful in pneumonia.

Dr. Webster said his experience was in accordance with the registrar's returns, that the mortality from pneumonia was greater amongst males than females. Prior to the period of puberty the disease was very fatal. In the case of insane patients, especially those recently become insane, pneumonia often terminated in gangrene of the lungs, though such a termination was very rare with ordinary patients. The method of treatment pursued in France, Germany, and Italy, he thought ill adapted to Englishmen, owing to the diversity of constitution and diet. He would occasionally bleed for pneumonia, but not to such an extent as formerly. In the case of children he preferred real to dry

cupping. Tartar emetics he considered a valuable remedy in addition to bleeding. One of the best remedies was that of blisters applied repeatedly. He would give mercury after bleeding and blistering to produce moderate salivation.

Dr. Richardson said that he had referred some years ago to the fact that chloroform would almost of necessity cure rheumatism, pneumonia, or any other disease attended with hyperinosis of the blood, since the time he had treated a child suffering from acute inflammation of the lungs by keeping it for three days under the influence of chloroform without the use of medicine. On the fourth day the child expectorated freely, and on the sixth was well, having recovered without any of the depressing symptoms usually following the ordinary method of treatment. Like bleeding, chloroform should be administered early.—*Med. Times and Gaz.*, May 12, 1855.

24. *Clinical Observations on the Peculiarities of Empyema.*—Dr. FIRM exhibited to the County and City of Cork Medical and Surgical Society, March 28, 1855, some pathological specimens illustrative of empyema, and made the following clinical observations on the peculiarities of the disease.

Influence of Sex.—In 30 cases, 25 were males—a result exhibiting a remarkable discrepancy between the sexes, in respect of liability to this disease; but which result, however, harmonizes with the general experience. In Dr. Hamilton Roe's table of 24 cases, 21 were males; in that of Dr. Hughes, of Guy's Hospital, out of 25 cases only two were females.¹

Age.—The average age varied from 18 to 35, 4 only having exceeded 40, 3 not having reached the tenth year, the youngest having been about 5. According to Dr. Hamilton Roe, 12, or one-half, were between 18 and 45; under 18, 6; above 45, 6; the oldest having been 62. According to Dr. Hughes, 18 in 25 cases occurred between 18 and 45; under 18, 4; and above 45, 3; the oldest having been 48.

Side affected.—The left was the side affected in 19 out of 30 cases; in the several cases of effusion into the left pleura, displacement of heart towards the right side was observed; in one case of very considerable effusion into the right pleura, the heart was observed to pulsate in the left axilla. In no case that came under notice were both sides affected.

Decubitus.—The cases observed were only seen at an advanced period of the disease, when the decubitus was almost uniformly on the affected side. In one case of circumscribed empyema of the left side, overlying the diaphragm² (which formed its floor, adhesive inflammation having united the opposed surfaces of the lung and diaphragm around it), the decubitus was indifferently on either side, but more conveniently dorsal. In another case, the decubitus was on the unaffected side, notwithstanding the existence of dextrocardia.

Pain.—In 9 only of 30 cases was pain complained of; and this varied much in its character, having been in some instances acute, whilst in others it amounted to a mere sense of uneasiness in the affected side, and sometimes in the opposite one. This result would suggest a conclusion quite antagonistic to the preconceived notions on this subject, pleuritis being *par excellence* associated, in the popular mind, with pain. A very slight acquaintance, however, with thoracic pathology suffices to demonstrate that absence of pain is not incompatible with pleuritis of a very decided character, as exemplified in those rigid adhesions which almost uniformly unite the opposed pleuræ in the vicinity of tuberculous deposits in the apices of the lungs. In tuberculous pleuritis, the

¹ It is not a little remarkable that, during the recent prevalence of ague in this city (which immediately succeeded the epidemic pleuritis forming the subject of these observations), the influence of sex was also remarkably manifested, a comparatively small number of females having suffered from this disease.

² The inflammation in this case involved a portion of the diaphragm, which occasionally, under such circumstances, manifests a very high degree of sensibility, as referred to in the interesting article on "Pleuritis," in the *Cyclopædia of Practical Medicine*; and yet the subject of this case complained of no pain, but suffered much from constant irritability of stomach during the whole course of his illness.

inflammation takes the initiative in the pulmonary pleura; and a question naturally suggests itself—whether the absence of pain in such cases generally is to be attributed to its concentric origin? The author of the article “Pleuritis,” in the *Cyclopaedia of Practical Medicine*, observes, however, that the pain present in such cases indicates the existence of tubercles long before they may be recognized by auscultation. This observation is not consistent with Dr. Finn’s experience, as he has rarely observed the subjects of tuberculous disease refer pain to the part of the lung affected; and in the majority of cases, pain, if present, was referred to the præcordial region, or in some instances to the unaffected lung, the increased requirements of respiration having imposed upon it a compensating duty. In pleuro-pneumonia, or pneumo-pleuritis, according to the more correct nomenclature of Dr. Watson, acute pain is referred to the region of the chest corresponding to the affected portion of the lung; but in this instance, in consequence of the sudden increase in the volume and consistence of the lung (the result of rapid engorgement), the pressure on the costal pleura probably occasions the pain in question.

The susceptibility to pain of the costal appears to contrast remarkably with that of the pulmonary pleura, the nervous sensibility of the latter being modified by the laws which preside over organic life. Should any viscus, whether above or below the diaphragm, become the seat of organic change, the investing serous membrane is necessarily more or less involved, due allowance being made for the difference of the phenomena of serous inflammation in the localities referred to; and yet such deviations from the normal state, in which inflammation, in some form, plays its part, are not in general manifested by any consciousness of pain on the part of the individual.

In pleuritis terminating in empyema, the almost uniform coincidence of pulmonary disturbance suggests the probability of the concentric origin of that disease also; and this fact, if established, may offer, if not a solution, at least an approximation to one, of the absence of pain, and of the insidious progress which this affection so frequently presents.

Relation between Pleuritis and Bronchitis.—In proceeding to canvass the subject of the relation between pleuritis and bronchitis, Dr. Finn recalled the attention of the society to the epidemic constitution which characterized the close of the year 1848 in this city. During the autumn of that year, on the occasion of the subsidence of the epidemic fever and dysentery of the years 1846–47, influenza supervened, and prevailed with unusual severity to the summer of the year 1848. During that period and the two subsequent years, the writer exhibited, at almost every meeting of the Medical Society, pathological specimens illustrative of every variety of pleuritis, and specially called the attention of the Society to the remarkable frequency of this disease at the period referred to, as observed in the several hospitals in this city.

On inquiring into the history of each case, it was ascertained that this affection was preceded in almost every instance by bronchitis, or the prevailing influenza.

This result would appear to establish the relation of cause and effect between bronchitis and pleuritis, in the cases referred to in this paper, the congestion of the lungs, in the asthenic type of the former disease, determining more or less of structural change in the contiguous pleura; which change subsequently involves a corresponding extent of the opposed costal pleura; and this view is rendered further probable by the analogy offered in the order of succession of the pathological phenomena in phthisis and pneumonia.¹

Tuberculous Disease of the Lung.—Of 30 cases, only 2 died of tuberculous disease of the lung; in Dr. Roe’s table, 3 out of 25; in that of Dr. Hughes, 6 out of 25 (a much larger proportion) presented that complication. The comparative infrequency of pulmonary tubercle, in connection with this affection, would imply that there existed a slight relation, if any,² between these diseases;

¹ It may be important to observe that, contemporaneously with the influenza and pleuritis, the so-called cattle disease prevailed.

² In sixteen cases of empyema observed by Dr. Walshe, as referred to in the last edition of his work on *Diseases of the Lungs*, &c., no single case of hæmoptysis occurred.

and this view is further confirmed by the fact that the tuberculous complication in one of the cases was confined to the opposite lung; as if the serous inflammation, however much it may have interfered with the functions and altered the form and consistence of the lung, yet exerted a conservative influence in preventing tuberculous deposit. This exemption from tubercle should excite surprise, when it is taken into account that, in almost all the cases that came under notice, there existed a physical condition eminently calculated to call into activity the tuberculous diathesis.

Bulging of the Intercostal Spaces.—In one case only was protrusion of the intercostal muscles observed. The subject of this case was a child, aged eight years, whose illness, previous to admission to the hospital, had been referred to phthisis; an opinion not confirmed by the physical examination of the chest, which immediately revealed the real nature of his illness, the intercostal spaces at the left side occupying a plane considerably anterior to the ribs,¹ whilst, at the same time, the heart pulsated under the right nipple. This case, after a very protracted illness, terminated fatally; and, on making a *post-mortem* examination, the affected pleura was found to contain an enormous quantity of fluid, entirely purulent. The result of the autopsy in this instance favours the views of those who connect protrusion of the intercostals with the purulent character of the contained fluid; but in two out of four other cases, which also terminated fatally, the products of inflammation were equally purulent in the absence of any change in the muscles referred to. The inference, then, appears to be, that the phenomenon in question is irrespective of the purulent character of the effused fluid merely, and that other conditions are necessary to its production.

Paracentesis Thoracis.—In one case of great urgency,² this operation was resorted to, other means having failed to afford relief; and, owing to a recurrence of the urgent symptoms, it was twice repeated with great benefit.

In this case, the expectoration, previously profuse and purulent, immediately presented a marked diminution in quantity, and alteration in quality, which both continued for several days.

This result coincides with the observations of the late Dr. Greene³ on this subject, and should perhaps excite surprise, as *à priori* reasoning would suggest the reverse, a large area of secreting surface being released by the operation from the pressure of the superincumbent fluid. The explanation of this interesting fact may probably be referred to the physical phenomena of endosmose and exosmose, the sphere of their activity being, in this instance, the portion of the pleura interposed between the effused fluid and the mucous membrane of the air-passages. Under such circumstances, the exosmose to the mucous membrane may be supposed to cease, or at least to be considerably diminished, on the occasion of the removal of the fluid contents of the cavity of the pleura.

Were fistulous communications between the lung and cavity of the pleura of frequent occurrence under such circumstances, a satisfactory explanation may thus be afforded; but such complications are rarely observed, and the pathological changes that ensue are admirably calculated to provide against this contingency; the investments of the lung being generally much increased in width, whilst the lung itself is diminished in volume.—*Dublin Quarterly Journal of Medical Science*, Aug. 1855.

25. *Pneumothorax—Puncture of the Chest.*—Prof. LAW detailed to the College of Physicians in Ireland (Feb. 7, 1855) a case of pneumothorax, occurring in a sailor aged forty years. The individual's suffering was very great, from his distressed

¹ The comparatively rare occurrence of protrusion of the intercostal muscles may further imply that the inflammation had its inception in the pulmonary, not the costal pleura; but, in the more advanced period of the disease, the internal aspect of the cavity of the pleura, when presented to the notice of the pathologist, is, in general, so assimilated by the structural changes observed, that it is impossible to assign to either pleura a priority in morbid action.

² In this case only, a loud splash was heard on succussing the patient.

³ *Dublin Journal*, vol. xvii.

breathing, caused by the accumulation of air in the affected side; he was, in fact, almost dying when Prof. Law had the side punctured to allow the air to escape. The operation was attended with immediate and great relief. The man survived the operation three days, and had no suffering up to the time of his death. Prof. Law's object in detailing the case was, to recommend the operation of tapping the side in certain cases of pneumothorax, and its early performance, where, from the nature of the opening through which the air escapes into the cavity of the side, it speedily accumulates there, and, continually increasing, soon disables the affected lung from taking any share in the respiratory action. He observed that the unaffected lung, being suddenly called upon to do double duty, in most cases becomes, from this exaggerated action, the subject of disease, and so death quickly ensues. He, therefore, directed special attention to the condition of the sound lung, and recommended that the operation of puncturing the side should be performed as soon at least as it exhibited any sign of disease. He felt that even the temporary relief obtained justified the operation, although he was satisfied that, with attention to its timely performance, it would be attended with a much larger measure of success.—*Dublin Quarterly Journal of Medical Science*, Aug. 1855.

26. *Treatment of Chorea by Gymnastics*.—M. BLACHE has submitted a memoir on this subject to the Academy of Sciences. His remarks are founded upon the experience of 108 cases, 84 girls and 24 boys, treated by gymnastics alone, or by these in combination with other measures, such as sulphureous baths, etc. In 102 of these cases the cure was completed, on an average, in 39 days; and in the 6 remaining, which he considers as unsuccessful instances, recovery took place in 122 days. The committee appointed by the Academy to examine M. Blache's paper, have reported on it in the most favourable terms, and considers the method of treatment advanced by him as deserving a place among the most approved curative means known in connection with this disease. The author has not only brought forward practical evidence of the successful employment of gymnastic exercises in chorea, but investigates their probable mode of action in that affection; and has fully established that the efficacy of this method of treatment equals, in most cases, that of any other mode with which we are acquainted, while the remedy is free from those objections to which many others are liable.—*Edinburgh Med. Journ.*, July, 1855, from *Gazette Méd. de Paris*.

27. *On Albumen as a Cholagogue*. By Dr. R. GIESELER, of Göttingen.—I am anxious to call the attention of the profession, briefly, to the employment of the albumen of hen's eggs in certain forms of jaundice.

Bernard's experiments, showing that this substance is assimilable only through the intervention of the hepatic function, immediately suggested to me the idea that in albumen we might find an adequate excitant of the liver. I inferred, first, that fatty nutriment, and in a higher degree albuminous articles of diet, must be avoided in inflammation of the liver; and, secondly, that in torpid conditions of that organ we might possess in albumen a remedy capable of stimulating it to increased activity. If to the liver be assigned the task of rendering albumen adapted to assimilation, this substance must be a stimulant of it, which will, *mutatis mutandis*, set its function to work, in the same manner as the administration of saline medicines does that of the kidneys. It is scarcely necessary to add, that the establishment of these results by experience must secure to albumen not merely the character of an adequate stimulant, but also pre-eminence over all other so-called cholagogues, since the action of the latter is very uncertain.

I think it unnecessary to demonstrate the remarkable efficacy of albumen in this respect by the recital of cases, since it was, as I soon learned, already known to our predecessors. It, however, appears to me not unimportant to point out the source whence it would appear the recommendation to employ albumen as a remedy in jaundice was originally derived. Charles White, in his work on *The Treatment of Pregnant and Puerperal Women*, states that he once suffered for several weeks from jaundice, and was very much reduced. Soap, aloes, iron,

and rhubarb had been taken without the least benefit. A navy officer, happening to visit him, assured him he would cure him in a short time. He told him, in fact, that, while on a voyage some time before, he was attacked with the same disease, and had in vain used the remedies prescribed by the surgeon of the vessel. A Spanish physician of the island of Minorca then advised him to take every morning, while fasting, two raw eggs, both yolk and white, in a glass of water, and to repeat the dose with one egg every four hours during the day. He followed this advice, and in three days his motions were again coloured with bile. White tried the plan suggested, and found the effect attributed to the albumen to be confirmed; in three days the feces were coloured, which they had not been for six weeks before. He continued the use of the eggs for some months. He subsequently recommended the remedy to several patients, and always with good effect, except in cases in which the jaundice proceeded from the presence of gall-stones. So far for the testimony of Mr. White. In the more modern treatises on therapeutics, I have not been able to find any allusion to this application of albumen; the present communication cannot, therefore, be considered superfluous. A few of the older works recommend, not white of egg, but the yolk, probably on account of its yellow colour. It is, indeed, possible that the action of the liver may be excited, not by the vitellin of the yolk, but merely by the albumen of the egg, with which Bernard experimented, and which White recommended in jaundice. Should this supposition prove correct, it would explain why the remedy lapsed into oblivion, and would furnish an important proof, in our day, for the often misunderstood truth that practical results do not become the property of science or art until they are referred to correct principles.—*Dublin Quarterly Journal of Medical Science*, Aug. 1855, from *Zeitschrift für Rationelle Medicin*, Bd. v. p. 253.

28. *On the Anthelmintic Action of Sulphate of Quina.*—By Dr. P. DELVAUX, of Brussels. During the last three years I have proved the efficacy of sulphate of quina as an anthelmintic. Some days after the administration of this salt in the intermittent fevers of children, the parents came to announce to me the cure of the fever; but in a great number of cases, the little patients had had motions, followed by the expulsion of intestinal worms.

Thenceforth I asked myself, if sulphate of quina had not anthelmintic properties? I then gave it to children affected with worms, and my trials were crowned with success. Every time that I made use of this salt as an anthelmintic it produced this effect, which I have verified in a great number of cases.

So early as 1764, Professor Van Doeveren of Groningen, reported two very interesting observations on the anthelmintic property of Peruvian bark.

The first case was that of a child, aged 12, affected with tænia. Purgatives, calomel, assafoetida, &c., had been tried, but in vain. At last, an ounce and a half of Peruvian bark was given in four days. After having taken the powder, the patient passed an entire tænia with the head.

In the second case, the patient was a young girl labouring under fever. She took an ounce of powdered Peruvian bark, made into an electuary with simple syrup. Three round worms were expelled.

In a great many cases, adds Van Doeveren, physicians have given this febrifuge with the single idea of subduing fever, and without having the least suspicion of worms, and nevertheless it has brought them away. Heister combined bark with mercury in his anthelmintic electuary, probably because he suspected the vermifuge power of the former.

I have collected upwards of forty cases of children affected with lumbricoid ascarides who have been radically cured of this affection with sulphate of quina. The salt usually produces, at the end of twenty-four or thirty-six hours, several liquid motions containing these entozoa. I shall quote the two following observations in support of what I have advanced:—

Case 1.—In the month of May, 1854, R—, aged two years and a half, was affected with worms. During three months, he from time to time passed a lumbricoid ascaris. There was general emaciation; the face was pale; the pupils dilated; the sleep disturbed; the tongue was white; the appetite irre-

gular; the abdomen enlarged and doughy; the bowels irregular; the pulse was small and frequent; the patient appeared to be almost moribund. I ordered an electuary containing five grains of sulphate of quina in an ounce and a half of honey, to be taken in teaspoonfuls in twenty-four hours.

At the end of this time the patient had several stools, in which were found five lumbricoid ascarides. I continued the treatment during the following four days; giving, however, but three grains of the sulphate in the twenty-four hours. Several additional worms were expelled. From this time there was a decided improvement in the state of the little patient, who shortly after appeared to be radically cured.

Case 2.—A boy, aged eight, was attacked with delirium and convulsions with grinding of the teeth. On the day preceding, two lumbricoid ascarides were discharged per anum, and one by the mouth. His other symptoms were, paleness of face; dilation of the pupils; emaciation; enlargement of the abdomen; frequent stools; a small and rapid pulse. Sulphate of quina was given. At the end of twenty-four hours he passed an immense quantity of worms.

The treatment was continued during the following eight days, but with a diminished dose of the sulphate, and was attended with the evacuation of a great number of worms. In proportion as the worms were expelled, the delirium and convulsions diminished, and finally completely ceased. The appetite was restored, the alvine discharges returned to their normal condition, and the enlargement of the abdomen subsided.

A tonic regimen was continued for some time; and to the present day, a period of two years, the child, who has become robust, has had no return of his malady.

The sulphate of quina is also most effectual in removing the ordinary ascarides (*oxyures vermiculares*). As is well known, these parasites are lodged in the fecal matters in the rectum, sometimes in the colon. They appear to imbibe a remnant of the chyle which serves to nourish them. They are expelled in a ball (*peloton*) with the feces, or escape by themselves, causing intolerable itching, tenesmus, and other annoyances.

Injections of sulphate of quina, repeated every evening for a certain time, are capable of completely destroying these entozoa.

I had twice an opportunity of administering sulphate of quina for *tænia*, and in both cases the worm was expelled and was not reproduced. The first case occurred in October, 1854, and was that of a widow, aged 28, who had suffered for many years from a *tænia* of which she was constantly passing one or more segments. Every known anthelmintic had been administered without completely freeing her from her malady. After having taken about forty-six grains of sulphate of quina, she voided several yards of a *tænia*, the characters of which corresponded with those of the *bothriocephalus latus*. The medicine was continued for some time, and she has ever since enjoyed good health, and has had no return of the worm attacks.

The subject of the second case, dating from the month of March, 1854, was a little boy, aged four, who, according to the report of the parents, had been from the time he was one year old, in the constant habit of passing entire ells of a large flat worm, which I recognized to be the *tænia lata*. Sulphate of quina was exhibited, a worm twenty-nine and a half feet (nine metres) in length was expelled, since which there has been no return of the affection.

Sulphate of quina is, therefore, truly an anthelmintic. The physician often meets debilitated sickly young children, whose constitution bears the stamp of the most profound asthenia. He generally shrinks, when these children are at the same time attacked with worm affections, from the long list of anthelmintics, which, most frequently, only act on the digestive tube by producing violent effects, which are often felt injuriously throughout the entire system.

It is in such cases especially that the sulphate of quina is advantageous, and I have never seen its employment followed by unfavourable consequences. Sulphate of quina produces its vermifuge effects in virtue of its bitter properties; for bitters, as is well known, act more energetically as poisons on animals, in proportion as the latter are lower in the scale of creation.

Is it not on account of their bitter properties that Celsus and Cælius Aureli-

anus extol wormwood and centaury as anthelmintics, and that Rivière (*Praxis Medica*, Book v. chap. 9) praises the same and other plants as vermifuges, and as especially efficacious in removing lumbricoid ascarides? Kluyskens, in his treatise on "*Materia Medica and Therapeutics*," says that "bitters are very detrimental to worms." "It is a very curious fact," observes this writer, "that vegetable bitters should in general be so destructive to inferior animals; flies perish almost immediately on being wet with an infusion of quassia."

It is, therefore, not impossible that sulphate of quina should be capable of killing intestinal worms. Moreover, by its tonic action, it restores the power of the digestive organs, debility of which strongly predisposes to the production of entozoa.

Doses and Mode of Administration.—The dose of the sulphate must vary according to the age of the patient; from two to ten years it will range between three and six grains; in older persons, so much as nine grains may be given in the twenty-four hours. When the medicine has produced the desired effect, the dose ought to be gradually diminished. During its administration, the diet should be light. It is seldom necessary to have recourse to aperients; it may, however, occasionally be advisable to administer castor oil or syrup of rhubarb. The following formulæ for its exhibition have been employed: *Powders.*—Sulphate of quina, half a scruple; sugar, as much as may be sufficient; divide into six powders. *Pills.*—Sulphate of quina, half a scruple; honey, marshmallow powder, of each as much as sufficient; make into six pills. *Electuary.*—Sulphate of quina, from three to six grains; purified honey, two ounces; mix; a teaspoonful to be taken frequently. *Syrup.*—Sulphate of quina, from three to five grains; syrup of orange-peel, ten drachms; a teaspoonful to be taken at a time, and frequently repeated. *Enema.*—Sulphate of quina, five or six grains; dilute sulphuric acid, as much as may be sufficient; distilled water, eight ounces.—*Dublin Med. Press*, July 4, from *Presse Médicale Belge*, April 15, 1855.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

29. *On Cicatrices.* By M. JOBERT.—A cicatrix is a kind of protective texture, intended to close in solutions of continuity, and to cover over organic tissues in the absence of the natural integument. When the membrane that covers the granulations has been formed, the remarkable retractile power exerted by cicatricial tissue is brought into operation, which, by its centripetal action, constantly diminishes the extent of the wound and the vascularity of the newly-formed product. This retraction, which is greater in proportion as suppuration has been prolonged, persists long after the closure of the wound.

If we examine a cicatrix that is completely formed, we find that it consists in: 1. A thin superficial membrane which, though sometimes rugous, is usually smooth, shining, and, from the absence of secretion, dry. This protects the cicatricial or inodular tissue, just as the epidermis protects the parts beneath it. 2. Below this is a peculiar tissue, consisting of a mass of irregularly interlaced and very resistant fibres. It supplies the place of the dermis, is the fundamental portion of the cicatrix, and may be compared to ligamentous tissue. 3. Arteries and veins, small in number and in size, are found in the centre and midst of this substance. 4. Although the existence of lymphatics has not been demonstrated, it is highly probable that they accompany the bloodvessels. 5. The careful dissection of a great number of cicatrices has never shown M. Jobert the slightest trace of nerves; but he has always found the nerves of the surrounding parts terminating at the periphery of the cicatrix in little gangliform swellings, exactly analogous to those found at the end of stumps after amputation. This enables us to explain the complete insensibility of the cicatricial tissue itself, and the keen sensibility of the integuments

at its periphery. Moreover, if we detach a flap from the vicinity of the cicatrix, attach this to its centre, and, when union is effected, divide the pedicle, we find that this islet of integument retains its sensibility in the midst of a tissue quite insensible to external impressions; the nervous influence seeming, as it were, to traverse the cicatricial tissue as a conductor to reach the cerebrospinal axis.

This inodular tissue is liable to undergo alterations. We frequently find, during changes of temperature, etc., cicatrices liable to pruritus; the patients scratching them until redness or excoriation is produced, without obtaining the slightest relief. Such irritation may lead to the belief that the cicatrix is very sensitive; but on examination we find that it has its seat in the nervous tumefactions at the periphery. The integuments may also have been at first incompletely destroyed, and any portion that has so escaped may retain its sensibility under the cicatrix. The cicatrix may appear red and swollen at the menstrual period or under certain atmospheric conditions; but such changes really take place in the congested cellular tissue and hypertrophied fatty tissue beneath the cicatrix. If the texture of the cicatrix be not firm enough to resist the traction exerted upon it, it may become torn or ulcerated. Ulceration, too, may spontaneously arise, and the reparation is effected by means of new inodular tissue, which renders the cicatrix firmer than before. Sometimes a cicatrix is invaded by diphtheritis, this being frequently the case in the vagina. Occasionally, but much more rarely, mortification of the tissue takes place.

It is to the retractability of this tissue that the production of the deformities commonly called "bridles" is due; and the surgeon should, by his prophylactic treatment, earnestly endeavour to prevent the formation of vicious cicatrices. To this end he should endeavour to diminish the suppurative process, repress too luxuriant granulations by compression or cauterization, moderate the inflammatory process by refrigeration, and place the part in a position opposed to the force exerted by the inodular tissue, so that the lips of the wound may diverge as far as possible. To these points he should direct all his attention, for after a vicious cicatrix has been once formed no mechanical appliance can ever elongate it; and to restore to the different regions their freedom of action we must have recourse to an operation. But we must take care not to operate in every case of deformed cicatrix, the prudent surgeon abstaining when the deformity is too considerable to be effaced. We must not operate either when the cicatrix is too recent; for its vitality and vascularity being but little developed, it may ulcerate or mortify, while the inodular retractility it still preserves, prevents the success of the operation, by incessantly drawing upon, and destroying the adhesions that form. There are two modes of remedying these deformities. The old one, which, indeed, is not yet abandoned, consists in completely dividing the cicatricial tissue, retaining the lips of the wound separated as far as possible, and healing by a new granulation. But if the cicatrix has contracted adhesions, the two lips can hardly be separated, and the operation proves useless. Yet, by this means, M. Velpeau has recently removed a very considerable bridle with success. Delpech was the inventor of the new procedure. He circumscribed the entire cicatrix within two semi-elliptical incisions, dissected it out, and united the healthy tissues by means of sutures. This, very well suited for a cicatrix of small extent, cannot be applied to a very large one, even when aided by the lateral *debridements* and detachment of the integuments recommended by Delpech, for the purpose of preventing traction, and for repairing the loss of substance produced by the bistoury.

M. Jobert operates by what he terms a mixed procedure, as follows: He makes a transverse incision through the most prominent part of the bridle, avoiding the subjacent vessels and nerves. Next, forming from the vicinity a flap sufficiently large to restore to the part the elasticity of its movements, and thick enough to be sufficiently vascular, he twists it on its pedicle, and applies it by means of the interrupted suture between the separated lips of the incision. When it has firmly united there, the pedicle is divided. In this way the motions of the part become quite free, and the deformity cannot be reproduced.—*Med. Times and Gaz.*, Aug. 11, 1855, from *Gaz. des Hôpitaux*, No. 63.

30. *On the Occurrence of Cancer in Children.* By M. GUERSANT.—The subject of cancer in children is nowhere treated of, probably on account of the rarity of this affection during the early periods of life; however, there is not a year in which the author does not see some cases of it.

For a very long time, cancer was met in children only in the orbital cavity, whether it took its origin from the globe of the eye or from the base of the orbit. It has since been proved to occur in the testicles. Dupuytren operated on several such cases; M. Guersant has also removed six or seven testicles affected with encephaloid cancer. Cancer has also been found in other parts, but less frequently, unless in the vulva. Ought we now to make a distinction between fibro-plastic and cancerous tumours? M. Guersant is strongly disposed to class them together, on account, he says, of the tendency the former possess, in common with the latter, to relapse. He admits, however, that their relapse occurs less frequently; and in this point of view they have a character of relative benignity which should be borne in mind, but they are nevertheless, according to this surgeon, closely allied to cancer.

The progress of a cancerous affection in a child is much more rapid than at a more advanced age. At the Salpêtrière and Bicêtre, cancer is seen in old patients to remain stationary for from fifteen to twenty years. In children, on the contrary, the progress of the disease is terrific. In a young boy, under M. Guersant's care for a cancerous tumour in the orbital cavity, the eye was driven out of the orbit in less than six weeks. The sufferings of this little patient were fearful; the globe of the eye was extirpated, but the disease returned with extreme rapidity, and at the end of a month the ethmoid was destroyed.

The prognosis in cancer in children is, therefore, very unfavourable; and because there is no chance of the disease remaining stationary, the indication is to operate with more rapidity and energy than in adults. But, on the other hand, relapse is so general, and occurs so quickly, that we ought to resolve on operating only with the view of procuring temporary relief from intolerable sufferings; nor should we fail to warn the friends of the patient of the likelihood of the disease returning.

M. Guersant quotes, in support of these remarks, a case which terminated fatally; although, on *post-mortem* examination, nothing but fibro-plastic tissue was found in the tumour, the destruction of which had been attempted. The patient had been a little girl, aged thirteen months, who had a tumour in the vulva. This tumour was tied by a surgeon on the 6th of November, and came away. The disease relapsed, and on the 18th of December a second ligature was applied; relapse occurred still more speedily than on the first occasion. In January, the second tumour was pretty large, and its presence gave rise to sympathetic vomiting. The child was presented to the Surgical Society, the members of which body verified the existence of a multilobular tumour, with prolongation into the vagina. Although it was difficult to trace its limits, M. Huguier advised removal with the bistoury, and cauterization of its roots. MM. Lenoir and Guersant were of opinion that the results of such an operation would be incomplete. However, the tumour mortified, and the child fell into a state of marasmus. M. Guersant thought it advisable once more to have recourse to ligature; but the little patient was already exhausted, and sank in a few days from adynamia.

Should excision have been more successful? It is not probable. Nevertheless, in another case, M. Guersant, considering such an affection as a sort of polypus, would excise it early, and cauterize its base with liquid caustics; and if the vagina was large enough to admit of these means, he would not despair of obtaining a more or less permanent cure.—*Dublin Medical Press*, July 25, 1855, from *Journ. de Méd. et de Chirur. Pratiques*, July, 1855, p. 299.

31. *On Dr. Landolfi's Treatment of Cancer.* By M. LASEGUE.—Dr. Landolfi, surgeon-in-chief of the Sicilian army, and lecturer on cancerous diseases at the Trinity Hospital, Naples, is now visiting different parts of the Continent, for the purpose of propagating his method of curing cancer. Having secured many adherents at Vienna, he has repaired to Paris, where a certain number of patients, selected from Salpêtrière, have been placed under his care, a

medical commission watching the results. As this method, though exciting much attention in Italy and Germany, is scarcely known in France, and we may add in England, M. Lasègue, while awaiting the report of the commission, proceeds to give some account of it; and although naturally prejudiced against any specific method of treating the disease, the above-board course of procedure adopted by Landolfi, and the large amount of testimony of success he adduces, have evidently made a considerable impression upon him.

The specific employed by Landolfi is the chloride of bromine, applied externally as a caustic, and administered internally, the latter being of quite secondary importance. This caustic, mixed into a thick paste with liquorice powder, may be employed alone, or it may be combined with other caustics, as in the following formula: R.—Chlor. brom. three parts, chlor. zinc. two parts, chlor. antimon., chlor. auri. ana one part. To be mixed in the air, on account of the fumes disengaged. In open cancer, Landolfi regards the chlor. zinc. as indispensable as a hæmostatic; and the chlor. auri. seems to exert a special action in encephaloid. Cutaneous cancer, epithelioma, lupus, and small cysto-sarcomas may be treated by an ointment formed of one part of chloride of bromine to eight of basilicon. The healthy parts around the tumour are to be protected by bands of linen one and a half to two inches broad, covered with ointment (four parts of chloroform to thirty of lard), and the patient is placed near a window, so that the fumes may escape. Small compresses, upon which the paste has been spread, are gently applied to the part, in an imbricated manner, so as to secure exact juxtaposition, keeping two lines clear of the sound parts. The whole is then covered with charpie and diachylon. A sharp burning sensation is soon followed by severe pain, which may last for several hours, and is combated by repeated doses of anodynes. The paste is usually kept on for twenty-four hours, and on its removal a line of demarcation is generally perceived. The tumour is in part white, and in part reddish, or marbled with yellow and blue. Bread or lettuce-leave poultices, or basilicon ointment, are now applied every three hours. As the gangrene proceeds, the pain diminishes, and about the fourth or fifth day the eschar loosens, being removable by the forceps from the eighth to the fifteenth. A healthy granulating surface is then exposed, and if any vestige of the disease is observable, a little paste is reapplied to that spot. The wound is now to be treated as a simple ulcer, and if there is a deficiency of suppuration a lotion is to be applied, containing from twenty to thirty drops of the chloride of bromine in five hundred grammes of Goulard water. The ulcer usually heals rapidly from the circumference, the cicatrix resembling that resulting from incised wounds.

In spite of the severe pain, there is rarely febrile reaction, and no change in the patient's regimen is required. Although Landolfi believes the paste acts by absorption, as well as locally, he regards internal treatment as only adjunctory, and only so employs it, in the hopes of preventing relapse. The treatment, even in very bad cases, usually at least affords very notable relief, and in such as are quite hopeless or inaccessible to caustic, a lotion, containing ten to twenty drops in five hundred grammes of water, may exercise some useful modifying power.—*B. and F. Med.-Chirurg. Rev.*, July, 1855, from *Archives Générales*, May, 1855.

32. *Treatment of Frostbite.*—Mr. E. J. COMPLIN read before the Smyrna Medico-Chirurgical Society (June 9th) a paper on this subject.

After some general observations on the various stages of frostbite, and the great frequency with which its later stages had fallen under observation in the Smyrna Hospital, the author proceeded as follows: In the greater number of cases of frostbite that we have had under treatment in this hospital, the disease had not extended further than to destroy, in some instances, merely the soft parts of the extremities of the toes and fingers in some parts, and, in others, the whole of the phalanges of the toes and fingers. I believe, also, there were one or two cases in which the disease extended to the metatarsal bones. The condition in which we found them was simply this: the soft parts had become dead, had separated from the living, and left the ends of the bones necrosed, and sticking out beyond the living tissues; the condition of the soft

parts in the neighbourhood, and for some distance round, was also remarkable. In the immediate vicinity of the dead bone the skin was red, and had a tense and shiny aspect. Further away, the cellular tissue was infiltrated to a certain extent, and pressure along the course of the tendons, more especially on the plantar and palmar aspect of the foot or hand, gave great pain. In the greater number of cases, the feet only were affected. Many of these patients also suffered constantly from pain and a sensation of burning, more especially at night, when it was frequently so severe as to require powerful narcotics to procure sleep.

With such a state of things, the idea naturally suggested itself to the surgeon—Are there any modes of artificial interference, by means of which I can hasten the cure of my patient? Seeing a quantity of necrosed bones sticking out from a man's foot in that way, would give the surgeon a desire to remove them. The form of operation to be performed of course greatly depends upon the greater or less extent of bone that is diseased. If only a superficial seale or small sequestrum exists, it will be sufficient, after it has become partially separated, to detach it entirely, and the wound will heal over it and form a tolerably good stump. But the class of cases to which I wish particularly to draw your attention, are those where we have one or more entire phalanges diseased, or where, when a number of portions of phalanges have come away, and we know that perhaps a useless or at best a painful and inconvenient stump will be found, the question arises ought we in these cases to do an operation at once, and thus hope to save the patient a great deal of time and suffering? I have seen a great many operations done in this way, but I confess that the results have been far from such as I could wish to have seen. The opportunities that I had of seeing the particular affection in all its stages and complications on board the *Dreadnought* were very great, for during the winter months every year many scores of cases were admitted of sailors suffering in this way. The class of people amongst whom the affection was most common were Lascars. These unfortunate men were generally shipped in the East Indies, or China, as ordinary seamen, were most barbarously treated on board ship, being kicked and knocked about by every white man; their diet was very poor, consisting of nothing but rice and biscuit. All these circumstances, combined with a naturally very feeble circulation in the best health, predisposed them very much to be quickly affected by the influence of cold at a temperature that a European would consider agreeable. Thus, in consequence of being rendered useless in a foreign climate, they were compelled to sleep in the open air on deck, and become the subjects of frostbite. I have seen them brought from their vessels in every stage and every form of the disease, from the simple frostbitten toe where only the soft parts were superficially involved, to the more formidable state in which one or sometimes both lower extremities, as high as the knee, were in a complete state of slough from the skin to the bone. Unfortunately I have not the notes of any of these cases out here, but can record many in which operations were performed as soon as the line of demarcation was sufficiently distinct to enable me to discern between the dead and living tissues. I remember two cases that were admitted at the same time in December last year. In one case, the leg, as high as the knee, was one complete slough; in the other, the mischief had extended above the knee. Both the patients were very much reduced in health and strength: they were put upon good diet and allowed a considerable quantity of stimulus in the shape of wine and brandy. Neither of them, however, showed any signs of improvement; in fact, they appeared to be getting lower, and as they were suffering a great deal of constitutional disturbance, and the line of demarcation was distinctly marked out, it was thought proper to give them the chances of amputation. They were both operated upon on the same day; some improvement in their general condition took place a day or two after the operation, but acute sloughing subsequently ensued, and death followed in one instance on the 7th, in the other on the 10th day after amputation. Mr. Solly had under his care at the time I was his dresser a case in which both the man's legs were frostbitten as high as the knees. The history was this: a relation of his died and left him £100, which he spent in drink in a week; at the end

of that time he had nothing upon which to exist, and lay about the streets. He was extremely emaciated and in very bad condition. Amputation of both limbs was performed, the stumps sloughed, but the man ultimately recovered. Another case I remember in which Chopart's operation was performed in consequence of the metatarsal bones being extensively affected; this man had sloughing of the stump, and died from purulent deposits in the lungs. In the great majority of cases in which only a phalanx, or sometimes even part of a phalanx, was removed early, where it was necessary to cut through soft parts, sloughing occurred, and in some cases such severe constitutional disturbance, as to endanger considerably the patient's life, and to retard his cure. The deduction that I have drawn from observing the results of these operations is, that although in some cases, operating early may be successful, yet in the greater number we shall find that surgical interference is attended with unsatisfactory results. The reason of it appears to be this: after a part has been frostbitten, if not killed, it takes a considerable time to recover its original state of vitality, so that a slight injury, or the interference of the surgeon with the knife, is frequently sufficient to induce the original morbid action that existed in the part. It is surprising how long after the first exposure to cold that this tendency exists to take on the original action. Cases exemplifying this in a remarkable manner have occurred here, showing that the removal even of part of a phalanx was sufficient to produce sloughing of the soft parts. The general rule that I would lay down with regard to operation in these cases is simply this, to remove necrosed portions of bone, by means of forceps, as they become loose, and allowing the stumps to heal of themselves, taking care not to meddle with the soft parts, and not to perform any operation for the making of better stumps until some time after the parts have quite recovered. Conical and painful stumps are very frequently the result of such a plan of treatment, but these should be left for a time until all redness, pain, and swelling have entirely subsided. On the other hand, very often by drawing out a necrosed phalanx, or detaching a sequestrum after it has become loosened, we get not only a very useful, but also a very respectable looking stump. The author concluded by some remarks on the treatment of the earlier stages of frostbite, strongly recommending, as a local application, cotton wool used with gutta percha or oil silk placed over it. The wool requires to be soaked for some hours in water, in order to become thoroughly saturated, and it should then be wrung out and placed aside for use. When applied, it should be dipped in hot water, wrung out, and closely adapted to the part, and, with oil-silk placed over, it retains the warmth and moisture much longer than any poultice, in addition to soaking up the discharges from the wound, and preventing the matter from being in close apposition with the sore.

Mr. Spencer Wells said, that the inferences drawn from isolated instances in which bad symptoms had followed surgical interference in cases of frostbite, must be received with considerable reservation. In one case he removed a phalanx of the great toe, which was partly necrosed, and severe rigors with low fever set in, followed by pulmonary congestion, and a large purulent deposit in the thigh. He was at first disposed to attribute these phenomena to the little operation, but just at the time another man in the same ward, who had also a frostbitten foot which had not been interfered with, was seized in a precisely similar manner with these evidences of pyæmia. He thought, therefore, that some general condition of the atmosphere of the hospital, or of the ward, had more to do with the constitutional disturbance in the first case than the slight local irritation caused by the removal of the diseased bone, and that, in other instances, although such slight operations were sometimes followed by severe general derangement, yet that this relation was accidental. With regard to the treatment of cases in which the phalanges of the fingers or toes, or portions of them, had been destroyed by cold, he exhibited two specimens which illustrated the results of two opposite methods of treatment, both of which had been extensively followed in the hospital. The one was about half of the ungual phalanx of the great toe, the greater part being necrosed. Mr. Wells said he had removed it by the cutting forceps in March, and the man had been discharged to his duty in a fortnight, and went to the Crimea by the

first vessel. The other specimen was also a portion of the ungual phalanx of the great toe, but here the necrosed part had separated spontaneously, or rather had become so loose that it was easily lifted off with the forceps. Both men had been admitted in February. The one operated on had returned to his duty in March. The one left to nature was still in the hospital, the bone only being separated at the end of June, and the cicatrization of the soft parts was not yet complete. Mr. Wells contended, therefore, that as men, when in hospital, crowded it and became exposed to fever, gangrene, or erysipelas—as the constant dressing was painful to themselves, and troublesome to the patients, and as their services were required at the seat of action—it was good practice to remove diseased bones, or diseased portions of bone, as soon as the general health of the patient was in a tolerably satisfactory state, and that in this way a better covering was obtained for the remaining bone, and consequently a more useful stump. He said that in several men lately sent to England as invalids, when the dead bone had been allowed to separate spontaneously, the covering was little more than a red, thin, tense epidermis, which was very apt to give way on the least pressure of the shoe.—*Med. Times and Gaz.*, Aug. 11, 1855.

33. *Treatment of Laryngitis by the Insufflation of Nitrate of Silver.*—M. EBERT observes that every surgeon who has attempted the application of solutions of nitrate of silver to the larynx must be convinced of its difficulty, if not impossibility. Indeed, he doubts whether the fluid ever reaches that organ at all; and in two cases of croup, in which the attempt was made by a most expert surgeon, he found after death that not a trace had penetrated into the larynx itself. In repeated attempts in the dead body he has constantly failed to introduce a sponge mounted upon a whalebone, and only after manœuvring could he get a silver catheter to enter. How much more difficult must it be, then, during life, when the very attempt stimulates all the neighbouring muscles to resistance.

On the other hand, nitrate of silver, in powder, may be introduced by the simplest agency. We all know how easily pulverulent substances, drawn in unawares by a deep inspiration, enter the larynx, and give rise to irritating cough. Trousseau was the first to apply this fact to the treatment of laryngeal affections by nitrate of silver; and Professor Burow, of Königsberg,¹ speaks very highly of this mode. He mixed three grains of arg. nitr. with one drachm of saccharum lactis, and caused the patient to insufflate daily as much as would lie in the barrel of a steel pen. Chronic laryngitis, which had gone on to the production of complete aphonia, was thus cured in a few weeks. Since he saw this account, M. Ebert has used the same mixture with surprising success in several cases of laryngitis. So minutely does the sugar of milk divide the nitrate, that if even a small portion of the powder only reach the larynx, it will still contain its proportion of the nitrate. He administers it in the following way. A steel pen, charged with as much powder as it will hold, is attached to one end of the barrel of a quill, which is also open at the other end. This is introduced far enough into the mouth to bring the steel pen opposite the root of the tongue. The lips are now closed around the quill, and the nostrils compressed, while the patient is desired to draw in air rapidly and forcibly through the quill-barrel. Almost every one fails at first, but all succeed on the second or third attempt—the cough and irritation of the larynx announcing the penetration of the powder there. Even delicate females and children easily practise the insufflation, and will repeat it for days or weeks together. Young children may have it administered by an apparatus contrived by Professor Burow. M. Ebert has as yet only employed the remedy in laryngitis; and he briefly relates six cases of its successful application.—*B. and F. Med.-Chirur. Rev.*, July, 1855, from *Annalen des Charité Krankenhauses*, Bd. v.

34. *Treatment of Chancre.*—From observations conducted on a large scale at the Vienna Hospital, Dr. SIGMUND concludes—1. Chancre can only be treated locally

¹ Deutsche Klinik, No. 21, 1853.

during the first four days, and the further we recede from this, the greater the urgency of the general treatment. 2. The local treatment consists in cauterization, which effectually destroys all the chancreous exudation to the sound tissue. 3. The observation of more than a thousand cases during eleven years, assures Dr. Sigmund that secondary symptoms never occur when the chancre has been completely destroyed within the first four days. He is only aware of two doubtful cases in which cauterization on the fifth day even has not prevented accidents. The best caustic is the Vienna, composed of quicklime and two or three parts of caustic potass. Cauterization should also be practised even after the fifth day, for although the chances of preservation from secondary syphilis are diminished, they are not totally abolished; and we prevent the chancre being communicated to other parts of the same patient, or to other individuals. 4. The general treatment consists in the methodical employment of mercury, no other means curing so quickly and so surely. 5. In the exceptional cases in which secondary symptoms occur in spite of general treatment, they are not found in an aggravated form. 6. According to circumstances, the general treatment should be continued for six or twelve weeks. The levity with which the public and the profession at the present time regard venereal symptoms, should be met by the strongest opposition. 7. Clinical observation shows that every chancre, well diagnosed, and not carefully destroyed, leads to secondary symptoms, if general treatment has not been instituted. This will be admitted by all who establish a rigorous diagnosis, and look for secondary symptoms soon enough where they are first to be found, viz: in the lymphatic glands. 8. Positive diagnosis is alone attainable by inoculation or the production of secondary symptoms. 9. Secondary symptoms are usually observed about the sixth week after infection, and very rarely later than the twelfth; and we must not always depend upon the patient's assertion, but make ourselves a rigorous search for their early manifestation. If between the sixth and end of the twelfth week no secondary symptom has shown itself, and the local manifestation has disappeared, the patient may be pronounced cured—the few exceptions that occur notwithstanding. 10. The amount of mercury administered varies according to the indications offered by different patients. The dietetic and hygienic management, both during and after taking the mercury, is too much neglected.—*L'Union Médicale*, 1855, No. 75.

35. *Remarkable Case of Subclavian Aneurism; New Method of Treatment; Recovery.*—Mr. FERGUSSON presented to his class on the 4th of August, a most interesting case—one of a series, as we subsequently learned—where a very remarkable cure has been effected in well-marked subclavian aneurism, by a new and specific method of manipulation which he has adopted. We may state here that we saw the case about a year and a half ago also, when the man was previously under treatment. Some short period before that time Mr. Fergusson conceived the plan of stopping the circulation in the aneurism by pressing the sides of the aneurismal sac together, with their intervening fibrinous deposit; and in this case, from the phenomena attending the manipulation, there appeared to us very little doubt that the object held in view by Mr. Fergusson had been attained—viz: the clots of fibrin in layers in the aneurismal sac had been displaced, and, spreading from the subclavian into the axillary and brachial, a new sort of Brasdor's operation, at the distal side of the subclavian had been the result. In other words, we believe Mr. Fergusson here, without ligature, had attained all the advantages of the last-named operative proceeding; for not only had a blocking-up of the axillary and brachial been followed by a partial stoppage of the current through the enlarged aneurism of the subclavian, but even with very marked, but not so satisfactory, results as regarded the pulse in the radial at the wrist, which became completely stopped for a time, with symptoms of paralysis in the arm, all resulting from the displacement of the fibrinous clots.

The aneurism in the present case was situated in the subclavian, in the usual site of subclavian aneurism—namely, between the scaleni muscles, and to us seemed almost to invite some modification of the Dublin surgeons' plan by compression on the first rib. The plan by compression, we need hardly ob-

serve, is in general applied to the artery above the aneurism, between the latter and the heart. Crampton, however, in 1816, showed that the obliteration of an artery can be effected without rupture or ligature of its coats, as generally conceived, simply by this blocking-up process. The early volumes of *The Lancet* contain cases also cured by Brasdor's operation; it seems, however, more applicable to carotid than subclavian aneurism.

Mr. Fergusson related to his class on the 11th, at some length, the details of a previous case of subclavian aneurism, of the same character as the present, in which his ideas on this subject were first matured. In both cases the method of cure by deligation at the tracheal side of the scaleni, as well as Brasdor's operation at the distal end of the aneurism, were inadmissible; yet it was gratifying to find the present plan, by firm pressure of the thumb on the aneurism, so as to displace some of the fibrinous clots, followed up by local pressure, succeeded in obtaining most striking and in many respects curious but satisfactory results. Intimately associated as the subclavian is at the right side with the vertebrals and carotid, the method of displacing fibrinous coagula is not without danger. A patient under such circumstances will fall down perhaps in a fit from want of circulation in one side of the "circle of Willis," formed by these arteries; yet, as the cause is so apparent, the danger may not be very alarming. Some instances of cure of aneurism of even the innominata have been given by American surgeons, in which recourse was had to ligature on Brasdor's plan of the subclavian; the result here ought to be equally dangerous. Hodgson gives us cases also in which a plug of effused lymph had nearly obliterated the subclavian; while Gendrin has imitated all the phenomena of arteritis and blocking up of aneurisms by injecting irritant substances into a portion of artery contained between two ligatures. In Mr. Fergusson's new mode of operation, we believe an entirely novel idea is acted on—namely, the displacement of the lamellated fibrin of the aneurism, on which no operation has been performed, and so directing the clots of fibrin that they shall block up the distal end of the artery so diseased. As Mr. Fergusson has expressed an intention of bringing the entire subject under the notice of the Medico-Chirurgical Society, we purposely abstain from giving the cases in detail. The method of treating aneurism by compression, originating with Desault and Hunter, and recently revived with such excellent results by the Dublin surgeons, will gain an immense accession of interest, if it should prove that the fibrinous deposit of the sac of the aneurism may be thus as it were utilized in bringing about the results hitherto gained in a different mode by Brasdor's operation at the distal end of the aneurism. Considerable caution will be at first necessary, as observed by Mr. Fergusson, in selecting cases which are fitted for the present method, as premature or ill-judged experiments in the shape of direct pressure or manipulation on the sac of aneurism not requiring it, one of which we mentioned recently as brought into Guy's, where direct and prolonged pressure had been made in the popliteal space before the patient came into hospital would be certain to be followed by severe inflammation of the sac and other dangerous results. The spontaneous cure of aneurism is not unknown in practice; it may take place, it must not be forgotten, by a coagulation of the contents or increase of the quantity of lamellated blood in the sac, the cavity becoming filled, and the circulation conveyed to the parts beyond the disease by the collateral vessels; or, again, in some rare cases the aneurismal tumour may be doubled up and press upon the portion of artery leading directly to the aneurism; or in a third fashion, as in a remarkable case given by Mr. Liston, where the patient had well-marked subclavian aneurism, which subsided and disappeared—an aneurism of the innominata pressing on and obliterating the aneurism of the subclavian!

Whatever may prove to be the correct pathological explanation of the phenomena in Mr. Fergusson's present cases, we deem it our duty to state here briefly that the cure seems complete and unequivocal without any ligature of vessels, nor is there any reason to believe the case was one of spontaneous cure of subclavian aneurism, as in the case given by Mr. Liston. It is now two years since the man came first under observation; he has been, on and off,

under treatment all that time in King's College Hospital and at home in the country; but happening to be in town within the last fortnight, Mr. Fergusson took advantage of the opportunity to exhibit the case to his class.—*Lancet*, Sept. 1, 1855.

36. *Cutaneous Nævi cured by Application of Iodine Paint.*—S. EDWARDS, M. D., relates two examples of this. In the first case, the nævus was unfortunately situated on the side of the neck of a female infant. At birth it appeared simply as a small, red shining spot, which in three months increased to the size of a fourpenny piece. The mother of the child at this time positively refusing to have any escharotics employed, fearing that it might give rise to a permanent and greater deformity, I recommended astringent and cold applications to be applied constantly, and this was kept up for some time, but with no good result. The nævus at the end of ten months had acquired additional size, and was observed to become redder and a little more elevated, whenever the circulation was increased by crying, etc. The parents still refusing any of my former suggested remedies, or even of vaccination, "until it got worse," I recommended the use of iodine paint, which was regularly employed by gently painting over the surface with a camel's hair pencil every alternate day, occasionally leaving it off for three or four days when the skin was very irritable and rough. Under this treatment I was pleased to find that the growth of the nævus was arrested, became smaller and mottled, and finally disappeared; a speck or two being alone visible to mark its former site.

The second case was very similar; occurred in a little boy nearly two years of age. The nævus was about the size of a shilling, but slightly elevated, and situated on the abdomen, and had gradually, but very slowly, increased since birth. No treatment had been employed, the physician who attended the mother of the child, having advised nothing to be done unless it increased. The tincture was commenced in September, 1854, and was continued more or less up to last month, when the disease had disappeared, leaving scarce a trace of the mischief.—*Med. Times and Gaz.*, June 2, 1855.

37. *Aneurism of the Superior Palatine Artery.* By M. TEIRLINCK.—This surgical curiosity was met with in the case of a man, æt. 74. The tumour occupied the roof of the palate, which bled so frequently that the patient was much exhausted. The tumour was soft, elastic, and pulsated synchronously with the heart, alternately expanding and diminishing. Its cause was unknown, and it had lasted for three weeks. The actual cautery was employed, the slough separated in eight days, the hemorrhage did not recur, and a perfect cure resulted.—*Dublin Hospital Gazette*, August 1, 1855, from *Gazette Médicale*.

38. *Excisions of Joints.*—On a recent occasion, Mr. FERGUSSON submitted to the inspection of the pupils of King's College Hospital, a number of patients on whom excision of the various joints had been performed from time to time, and took the opportunity of making some valuable remarks upon the improvements which had been made in this department of surgery, and upon the individual cases now submitted to notice. He first made some observations upon the introduction of excision of the elbow-joint, in the place of amputation through the arm. Passing a eulogium upon Professor Syme for the part he had taken in introducing and recommending this operation, he observed that it was not followed out even yet, as it ought to be, notwithstanding its acknowledged superiority over all other modes of treating incurable disease of that joint. As illustrative of these remarks, two patients were introduced in whom excision of the elbow-joint had been performed, in one of which it had not been done more than ten weeks, and yet the patient (a woman) had already got an excellent arm.

Mr. Fergusson stated that, in reference to this proceeding (in the elbow), there was now no question in the mind of any surgeon; but in the instance of some other joints, there had been much controversy, and he would now draw their attention to a case where he had performed the operation of excision of the head of the thigh bone several years ago with the most complete success.

[The individual on whom this operation had been done was brought into the theatre; he was a fine healthy young man, and walked with the greatest facility, and stated that he sometimes walked sixteen miles in the day.] He was particularly glad at being able to show them this patient, inasmuch as some very erroneous remarks had been made in reference to the propriety of this operation. Mr. Syme had, in his lectures in the *Lancet*, recently published, stated that, if the head of the thigh bone had been taken away with success, the limb could not be of any use for the support of the body; but those who now had the case before their eyes could see for themselves how gratuitous such an assertion was.

Three patients were next brought forward, upon whom excision of the knee-joint had been successfully performed. The first was a fine healthy woman, who had undergone the operation about two years since; a perfect ankylosis had taken place, and by the help of a high boot, well fitted to the limb, she walked with the utmost facility. The second was a little boy who had undergone the operation twelve months since, and could also walk, even without artificial assistance, with great facility; and the third case was also a lad upon whom his friend, Mr. Henry Smith, had operated nine months since, under the greatest disadvantages; but here also, as the pupils could see, the patient had made an excellent recovery, and could walk about with the same facility.

Mr. Fergusson made some lengthened remarks upon the operation in question, and stated that although, to his regret, most unjustifiable and unhandsome remarks had been directed by Mr. Syme against the promoters and pursuers of this mode of treatment, the proceeding had been followed out by several surgeons, and the success was so admirable that, notwithstanding the veto of the Edinburgh Professor, the operation was now fairly established; taking the opportunity of eulogizing Mr. Jones, of Jersey, and the late Dr. Mackenzie, for their efforts in this direction, he concluded his remarks by stating that he hoped his pupils would endeavour, in fitting instances, to follow out the same line of practice, for which they had ample authority in the cases he had just had the great pleasure of bringing before them that day.—*Med. Times and Gaz.*, Aug. 18, 1855.

39. *Extirpation of the Submaxillary Gland.*—Dr. Wm. H. HINGSTON relates (*Medical Chronicle or Montreal Monthly Journal of Medicine and Surgery*, September, 1855) the following example of this:—

W. S., æt. 20, placed himself under my care some time in December, 1853, when he related the following: One day in autumn, while working in the field, he was struck lightly by a farm servant with a potato upon the jaw. Very little inconvenience was felt at the time; but a few weeks after the part beneath the jaw appeared red, tense and swollen. In the region corresponding to the posterior angle of the submaxillary triangle of the neck, a tumour is visible, causing slight fulness of that part of the cheek. The tumour is painful on pressure. Its upper border is covered in by the body of the lower jaw.

During a period of four or five weeks, every attempt at discussing it was made, but attended with no beneficial results. At the end of that time, I proposed its removal by the knife, to which Stewart readily consented. The sight of the instruments, however, weakened his resolution, and he left, promising to return on the following day. I saw nothing of him until the 5th of March. During the interim, the tumor had increased to twice its size, was more painful, and what was to him a source of much grief apparently, he thought it looked "unco bad." He was now eager for its removal. With the assistance of my friend, Dr. Wright, I commenced the operation by carrying an incision of about one and three-quarter inches in length along the base of the left side of the lower jaw, commencing at the angle.¹ The platysma, superficial and deep fasciæ were divided, and the tumor exposed. It was found to be bound down on all sides, by condensed areolar tissue, which yielded only to the edge of the knife.

¹ Velpeau recommended a second incision from the posterior of the first to the os hyoides. But finding that the tumour could be exposed by a single incision, at the suggestion of Dr. Wright I was induced to adopt this plan.

This made the dissection more hazardous. The facial artery was tied and cut; the facial vein also was divided. Hemorrhage from the latter and from some other small venous branches was profuse, and greatly impeded our dissection, but by firm traction, downwards and forwards, it was isolated by the knife from its deep attachment. The edges of the wound were then brought carefully together, and union by second intention took place in seven days.

It is now nearly eighteen months since the operation was performed, and the patient tells me he experiences no inconvenience whatever. A white seam alone indicates the former situation of the wound.

The tumour (which was about the size of a walnut), was of a whitish colour, and very hard, creaking under the knife like cartilage. A section of it showed it to be made up of concentric layers, having in their centre a nucleus of pus.

40. *Lithotrity Successfully Performed on a Man eighty-six years old.*—Mr. F. WILKINSON records (*Lancet*, July 28, 1855) a case of stone in the bladder complicated with stricture of the urethra, which is worthy of note in consequence of the advanced age of the patient (86 years), and the success which attended the operation of lithotrity, which was performed by Mr. Coulson.

41. *Report on the Employment of Mathysen's Gypsum Bandage.*—By Drs. GRIMM and JUNGKEN.—Dr. Mathysen's bandage was employed in thirty-six cases of fractures of various kinds early in 1854. The bandage is prepared by stretching it upon a table, and well rubbing powdered gypsum into it on each side. It is then rolled up or otherwise arranged, according to the use to be made of it. Immediately before applying it, it is dipped in water or other fluid, the limb being protected by a flannel or other bandage prior to its application. Any portion of the bandage that is found not to have become wetted is moistened by a wet sponge. Flannel will take up twice as much gypsum during the rubbing as linen; but it is more clumsy, and not so easily applied. If a very firm, immovable bandage is required, some of the gypsum, in the form of a thin paste, should be applied during the last turns of the bandage. Its appearance is much improved by passing a damp sponge several times along it while still wet, and at a later period it may be smoothly polished by means of glass. To remove the bandage, it only requires to be again well wetted.

• The Reporters pronounce this bandage to be the best of all hitherto invented, including those that most resemble it, as the starch-bandage, upon the following grounds: 1. The rapidity with which it hardens. 2. Its simplicity and easy application. 3. Its small cost. 4. The ease with which it may be removed—the linen composing it being available, after twenty-four hours' soaking, for new bandages. 5. Its firmness and immovability render it suitable for the most oblique and difficult fractures. 6. From its rapid hardening and its firmness, it is well adapted for those cases which require extension and counter-extension to produce coaptation of the fractured parts. The position obtained remaining unchanged, apparatus of extension, so uncertain in operation, and so annoying to the patient, is not required. 7. The ease with which it is borne. 8. Its porosity. Cutaneous transpiration is not quite suppressed, and if the fracture be complicated by wounds, ulcers, &c., these are indicated by the discharges making their way through the bandage. 9. The gypsum bandage is a good conductor of heat, and a bladder of ice placed over some oil-skin, around the fractured part, takes effect in five minutes. 10. When the bandage is properly applied, the form of the limb is so well displayed, that any irregularity of the fractured part may be judged of externally. 11. Its handsome appearance and regularity distinguish it from all analogous bandages. 12. Fractures seem to unite sooner under its employment.—*B. and F. Med.-Chirurg. Rev.*, July, 1855, from *Annal. des Charité-Krankenhäuses*, Bd. v.

42. *Transfixion of the Chest, from Side to Side, by a Sharp Stake.* *Recovery.*—This remarkable case is communicated to the *Journal de Médecine* of Bordeaux, by Dr. LASSAIGNE, Surgeon of the 11th Regiment of the line. When we con-

sider the extreme danger of penetrating wounds of the thorax, the recovery of the patient is certainly surprising.

CASE.—A soldier was employed in gathering cherries, when the branch of the tree on which he rested his weight broke, and he fell a height of several feet; his trunk coming horizontally on the stakes which protected some vines placed beneath, he was literally spitted.

Dr. Bax, one hour and a half afterwards, found the wounded man lying on the ground, with the chest pierced from side to side by the stake; the point had entered the intercostal space between the seventh and eighth ribs on the left side, four or five fingers' breadth below the nipple, and rather behind it, and had come out between the fourth and fifth ribs on the right side, behind the angle of the axilla, the point projecting from the chest to the extent of fifty centimetres. The wounded man retained all his senses; his countenance did not indicate any great suffering; he only complained of the weight of his head when it was not supported, and of a difficulty in moving his arms; voice natural; no cough; apparently no disturbance of either respiration or circulation; no indication of hemorrhage either externally or internally.

Dreading hemorrhage, when no assistance was to be had, M. Bax was unwilling to remove the foreign body on the moment, and accordingly the patient was removed to an hospital in the neighbourhood. When placed in bed and undressed, a portion of the shirt was found to have been carried into the thorax, forming a kind of sheath around the stake, for a considerable distance. The stake was extracted with ease, the sheath formed by the fold of the shirt seeming to facilitate the operation. This was on the 3d July.

Nothing unusual was observed subsequently, except the occurrence of emphysema, which extended upwards to the neck; there was no cough or evidence of internal hemorrhage.

Intense inflammation of the side, and of the chest generally, supervened, but yielded to active treatment. On the 6th July an improvement had already taken place, and the wound on the left side had united by the first intention. The improvement steadily continued, the wound on the right side subsequently closed, and about the 25th July convalescence was established.

The patient was subsequently presented to the Society of Medicine of Bordeaux; after examining the wounds, the members agreed in thinking that the stake had passed through and through the lung.—*Dublin Hospital Gazette*, August 1, 1855, from *Journal de Médecine de Bordeaux*.

43. *Complete Dislocation of the Lower Jaw reduced by a new method.* By W. COLLES.—Miss —, æt. 25, whilst indulging in a protracted yawn, felt a jerk in the jaw, and found she could not close her mouth. She immediately applied to a medical man in her neighbourhood, who recognized a dislocation, but failed in his efforts at reduction. She then applied to Mr. Colles. "The mouth was open, the symphysis of the jaws slightly projecting. On applying the fingers to the angle of the jaw, and tracing the ramus upwards, it led in a direction much anterior to the position of the articulating surface. The posterior edge of the bone could be most distinctly felt, and a broad furrow or hollow existed between the bone and the ear. The heads of the bone were felt and perceived prominent in front of their natural position, so that the face appeared broader at this place than natural.

"Before attempting reduction, I wished to ascertain the position in which I would have most command of the force to be used. Standing before her, I passed both thumbs into the mouth, but felt I would not have a position the most favourable for applying all my force, if necessary.

"I then stood behind her, and it at once struck me this was the position which afforded most advantages.

"Placing her head against my chest, I passed each thumb as far back on the corresponding side of the jaw as possible. By making a rotatory motion from the wrist, I found the bone to yield; by now adding a motion of drawing the hand in towards the chest, the left side first, then the right, slipped into their positions, and the patient closed the mouth, the rows of teeth falling into their relative positions, and she now could speak plainly.

"I think there are many advantages to be derived from attempting reduction in this posture, viz: the surgeon standing behind the patient, the head applied to his breast, and the thumbs turned inwards on the corresponding angles of the jaw, the fingers under the bone in front.

"In the first place, the head is much more secure than in the original process, where it is applied against a wall, because in the latter the surgeon may press down the bone, and the patient generally will draw the head in the same direction by moving the body forward in the chair.

"By standing behind the patient, while depressing and pushing back the thumbs, he is pressing forwards with the chest, and thus fixes the head more steadily, and assists his manipulations; and even if the patient do move on the chair, a slight motion of his body will suffice to counteract this movement, and retain the head steadily fixed.

"Another advantage is, that he can use much more force, because when standing in front he can only use the muscles that depress the hands; whereas standing behind the patient he has the power of those muscles, and is assisted by the powerful class of muscles that rotate the thumbs inwardly; and, besides, in the former case his pressure is away from his body, whereas in the new position the pressure is more directly downwards and towards himself. The only disadvantage in this proceeding, if it can be considered one, is, that the mouth is stretched more than in the original plan."—*Dublin Hospital Gazette*, July 15, 1855.

44. *Wounds inflicted in the recent Bombardments of Sebastopol.*—D. J. DUIGAN, Surgeon, R. N., gives the following notice of some remarkable gunshot wounds seen by him:—

"Most of the wounded were interesting in a surgical point of view, but it would be both useless and tiresome to attempt even a list of them; yet some appeared to be so very singular in their nature, that I will mention those only, to the exclusion of all others.

"1. *Shell wounds.*—A seaman, knocked down by a fragment of mortar shell, was picked up dead. The head was apparently swept from his shoulders, but there was no trace of hemorrhage. On disentangling his clothes, which were tightly jammed around the injured part, the head was found driven downwards into the chest, carrying with it a great portion of blue shirt and red comforter. A small tuft of hair alone was visible at the bottom of a deep cavity. It was a regular intussusception.

"An officer of engineers had just entered the battery, when a 13-inch mortar shell fell close by him, exploding as it struck the ground. One thigh was blown into the air; the other, with its bones shattered throughout, but retaining its continuity by means of the integuments, was thrown around the back of his neck, and hung pliantly over the opposite shoulder, just as the arm of a child might lie in contact with its mother's neck. He lived for a few minutes.

"A shell from the Malakoff burst through the embrasure of the right Lancaster gun, disabling four men. One received a compound comminuted fracture of the left thigh, and a similar injury of the arm; the second, a compound comminuted fracture of the left thigh; the third, a comminution of the right knee-joint and ankle, both which joints were widely open. About a pound weight of the iron shell was immovably impacted in the inner condyle of the femur. The fourth was an artilleryman at the next gun, whose tarsus was injured. There was extensive laceration in the first three cases, and the shock was extreme; there was an oozing or welling of blood in two of them. The medical officers attended to those cases where the men fell, in rear of the embrasure, at which the enemy still continued to pour their shot and shell, but, fortunately for the medical officers, with less precision than before.

"A shell was fired at a group, principally composed of sappers and miners. One was killed, his face having been shot away. Another was carried up to the first parallel, badly wounded. On examination, it was found that half of the inferior maxilla of the dead man was driven into the roof of the second man's mouth.

"A night or two after the capture of the quarries, a man was killed in them

by a shell from the Redan. An officer of the 97th, who received several severe superficial wounds from splinters imbedded in the middle third of the left thigh, which was the finger. It was a large portion of one of the cylinders who had been killed; it was as sharp as a chisel.

"Two artillerymen stationed in the 8-gun battery, in the right attack, were sitting or lying down, engaged in conversation, when exploded as it approached their position. The head of one was taken off by an axe, above the neckcloth, the tie of which was undisturbed. The forearm of the other man must have lain in juxtaposition with his thigh, for both limbs were lopped off by the same blow, in a line corresponding with Poupart's ligament. This man lived for about half an hour, urgently requesting all around him to keep sprinkling his face with water. The wounds in both limbs were jagged. The muscles of the thigh were drawn out in long bands. There was no hemorrhage.

"Another artilleryman, somewhere about the same part of the works, had his left knee-joint laid open and comminuted by a fragment of shell; no shock; a slight hemorrhage oozing. As he approached the medical officers on a stretcher, he facetiously asked if it was not a wooden leg for him; and as he was being carried to camp, he asked us to make the leg for him.

"A 13-inch mortar shell dropped so close to a seaman that it burned his perineum, testicles, and clothes as it burst. One of the ankle-joints was laid open; but the wonder is, how he escaped being blown to fragments.

"About the central point of Gordon's battery, a shell burst among a gun's crew. One poor fellow was struck over the angle of the ribs. He uttered a cry for the doctor, and rushed about twenty yards, when he staggered, fell, and instantly expired. His heart and great vessels were ruptured. A second man's face, right shoulder, and arm, with the trapezius and latissimus dorsi, were torn away from the body. A third received a compound fracture of the ankle-joint, and a similar one of the ulna. The fourth escaped with a severe laceration of the calf of the leg.

"An artilleryman, sitting near one of the magazines, had part of his thorax and shoulder cut away, in a line from the sterno-clavicular articulation to the hypochondrium, by the half of a 13-inch shell falling upon him.

"As the military relief entered the right attack early one evening, the enemy opened a fire of shell upon them from the Garden batteries. One burst over the head of the column, by which two men were killed and about a dozen seriously wounded, the legs and arms of some being carried away. One man, whose forearm was destroyed, had all the comminuted bones driven into the thigh.

"2. *Wounds from round shot.*—These wounds are easily recognized at the first glance, as there is but little variety in the appearance they present. Most of the men killed by shot had their heads knocked away, either completely or in part. However, some cases occurred where those large projectiles went through the body, and even through the upper part of the thigh, making orifices of entrance and exit.

"A bombardier, at one of the mortar batteries, while in the act of laying the mortar, was struck over the ribs by a spent shot, which had barely sufficient force to ricochet over the parapet and drop into the covered way. As soon as the man was struck, he uttered a loud scream, and, as he fell, made a convulsive death-grasp, and seized the cap of the officer who was standing beside him. Death was instantaneous, although there was no mark nor breach of surface to show the site of the injury. Nothing could persuade his companions against the idea of his having been killed by a 'wind contusion.'

"During the past winter, a shot ricocheted with great force over one of the parapets, carrying away the cap from a seaman's head. The man was a little stunned, but no further mischief ensued. When his cap was picked up, it contained a handful of hair, which had been shaved from the scalp by the shot. This would have been a 'poser' for the old wind contusionists!

"3. *Bullet wounds.*—Our advanced trenches being, in many places, within forty yards of the enemy's rifle-pits, wounds of great severity were inflicted on both sides, as the force of the bullets was undiminished by distance. The ori-

"The exit caused by the conical balls more resemble shell wounds, in some instances, than a bullet aperture. In wounds about the head, especially, I have seen nearly the whole of the parietal bone carried away.

"Notwithstanding those jagged wounds from Minié balls, I have seen a soldier of the 41st hit by one on the nose, which caused as clean a wound as if done by a sharp knife. The nose was divided at the junction of the cartilages with the bones, the lower portion dropping down, but adhering by a good pedicle. It was brought together as in harelip.

"Most of the wounds caused by the new conical bullets are, however, remarkable for the manner in which they plough up the soft parts.

"A soldier of the 33d was struck by a ball, which made six openings. It passed through the right thigh, through the scrotum, and through the left thigh, where it escaped.

"The Russians use several kinds of bullets—one a solid conical ball, which belongs to the Liège rifle; another, of a larger size and conical form, hollow at the base, with a small pillar, or nipple, standing in the cavity. It is surrounded by three lines. At the base, to guide the ball in its flight, there are two other smaller ones, modifications of this principle. The old round ball is also still employed. In some cases, two of those round bullets have been found connected by a transverse wire, like bar-shot.

"4. *Grape-shot wounds* have been severe and numerous. The following was an interesting case:—

"A soldier of the 49th was struck on the temple by a grape-shot, which destroyed the squamous portion of the temporal bone. The brain was flowing through the wound, the man breathing stertorously. The grape-shot was supposed to be within the skull. It was subsequently found in his mouth, at the base of the tongue, pressing against the epiglottis.

"Since the commencement of siege operations before Sebastopol, one medical officer has been killed, and two or three have been wounded. The first, Mr. O'Leary, assistant surgeon of the 68th regiment of light infantry, was actually cut in two by a cannon ball while in the act of assisting a wounded seaman. It is only to be wondered at that more casualties have not occurred among the medical officers, for during the heat of the fire they are constantly called from place to place, running along the batteries, through the line of fire, in quest of the wounded. During the second bombardment, this peripatetic system was very trying and fatiguing, for the soil was heavy and tenacious from the torrents of rain that then deluged the trenches, and instances occurred where officers' boots drew off while running along to assist the wounded.

"The French arrangements are excellent. They have established a surgery, with all the necessary appliances, in one of the ravines, where a staff of medical officers are stationed for daily duty. The medical officers in the trenches apply tourniquets, or do whatever is most urgently called for, on the spot, and then send the wounded on to the hospital staff, by whom they are next examined. If the cases demand immediate amputation, the operations are performed; if not so urgent, the cases are sent on to the field hospitals.

"The duties of the siege fall heavily on the medical officers of the naval brigade, five of whom do that duty, and, out of the five, two go daily on trench duty. It comes to the turn of an army medical officer to go less frequently into the trenches. The time of duty varies in the different divisions, according to the strength of the medical officers in them. In some, the medical officer goes once in a fortnight, while in others the time extends even to once in five or six weeks. The royal artillery send no medical officer now, unless a general bombardment be going on. On the whole, this trench duty is very trying and hazardous; and, in performing it, the medical men run the same dangers, if not more, certainly not less, than the executive officers, who are generally stationary in a battery, while the medical officer, as ubiquitous as possible, is rushing in all directions to succour the wounded."—*Med. Times and Gaz.*, Sept. 8, 1855.

45. *Lubrication of Catheters.*—The practical surgeon can afford to regard nothing as insignificant which can exercise the slightest influence on the result of his treatment. Although not a matter which generally claims much atten-

tion, we observe that several of our hospital surgeons, and those, too, among our best, are very particular as to the kind of grease which they employ for smearing catheters, etc. A moment's consideration will, indeed, convince any one that the question of preference is one of some importance. To say nothing of pain, etc., to the patient, the difference between a well-lubricated urethra and one which is not so may not unfrequently decide the success or failure of the attempt at catheterism. The objects to be gained by lubrication are several: 1st. Mechanical friction is diminished; 2dly, the mucous membrane is shielded and rendered much less sensitive; 3dly, the mouths of follicles, crypts, etc., are filled; 4thly, the prevention of irritation to the mucous membrane prevents also spasm, a circumstance which, as the muscularity of the urethra is now generally admitted, is of the greatest consequence; 5thly, the mucous membrane is rendered (mechanically) supple, and the chance of its laceration or abrasion is very much diminished. Now, in order that these intentions be well fulfilled, it is necessary that the grease used be of a kind likely to be carried with the instrument down the whole tract of the urethra. It must, therefore, possess a certain amount of cohesion, and not be easily rubbed off. The ointment used for this purpose at St. Bartholomew's and several other hospitals consists of equal parts of olive-oil and fresh lard; at others, castor oil is employed. We are inclined, on the whole, to give preference to the latter. When cold, it is very viscid, and adheres well to the catheter; and it loses its viscosity just at the proper time, as it becomes warmed by the instrument and the urethra. It keeps much better than anything containing lard. Olive oil is much too liquid, and should never be used. If the urethra be known to be very irritable; it may be well to adopt a plan to which we observe Mr. Wormald often resorts, of employing two instruments, the first of which is withdrawn just before touching the stricture, being used merely for the purpose of lubricating the canal; and the second, again, well greased, is carried onwards.—*Med. Times and Gaz.*, July 21, 1855.

[We have for some time employed the castor oil for lubricating catheters and bougies, and can testify to its superiority over olive oil for that purpose.—EDITOR.]

OPHTHALMOLOGY.

46. *Opacity of the Cornea treated by Operation.*—Dr. M. DAVIS reports (*Med. Times and Gaz.* Aug. 18th last) the following cases of opacity of the cornea treated by operation under the care of Mr. HAYNES WALTON and Dr. TAYLOR. A man, about 50 years of age, a patient of Dr. Taylor's, had a quantity of lime thrown into his eye four years ago. The eye was immediately washed out, and it was supposed that all the foreign matter had been removed, but a dense white opacity remained, covering nearly two-thirds of the cornea, and completely concealing the pupil when in a state of medium contraction. Many ineffectual attempts had been made to remove or diminish the impediment to vision, by means of lotions and other local applications. On examining the eye minutely, it was seen that the opacity was smooth and uniformly covered by the epithelium; its upper edge, where it did not extend to the margin of the cornea, was shaded off gradually, and the surface generally appeared slightly more elevated than that of the clear part of the cornea. This elevation, taken in connection with the history of the case, led Dr. Taylor to suspect that the apparent cicatrix was formed by a portion of the lime which had not been removed at the time of the accident, and had become incorporated with the corneal tissues. He therefore, with a fine iris knife, carefully raised the epithelium in front of the pupil, and found that, by careful manipulation, the opacity could be chipped off in small flakes, and that in no part, towards the centre of the cornea, did it appear to have penetrated the anterior elastic lamina. After clearing the pupil, the operation was suspended for the time, partly on account of the severe pain which it occasioned, and partly to avoid the risk of inflammation. On a

subsequent occasion, the remainder was removed, with the exception of a few small spots towards the margin of the cornea, which appeared to be due to interstitial inflammatory deposit.

The slight haziness which remained after the operation was speedily dissipated, and the man was dismissed with almost perfect vision. Chemical examination showed the opaque matter to consist of carbonate of lime.

In another case, also under the care of Dr. Taylor, the opacity was removed, partly by operation, and partly by the process of absorption, excited by mechanical irritation.

The patient, a female, 24 years of age, had been subject, till within the last six years, to attacks of ulceration of the corneæ. She now applied on account of a central milky opacity of the right cornea, shading the pupil and destroying useful vision in the eye. It had remained undiminished in size for six years, notwithstanding a great variety of local applications. Near the centre of the opacity were two small, dark-brown spots, situated, apparently, in the substance of the cornea. These were, probably, the effects of a former long-continued use of nitrate of silver solution, while the cornea was ulcerated. The surface of the opacity was readily peeled off in small flakes, by a cautious use of the iris knife, but the brown spots were found to be so deep-seated, that Dr. Taylor did not consider it prudent to interfere with them, especially as they would not impede vision. The result of this little operation, which has since been repeated, has been the rapid diminution of the opacity, and corresponding improvement in vision; and as absorption is still going on steadily, there is every prospect that the sight of the eye will be completely restored.

It might be objected, that the following case ought not, in strictness, to be placed under the heading of this report, but it is given, not only on account of its great peculiarity, but because it is somewhat allied to the above.

T. P., aged 38, a meteorological instrument-maker, discovered, about four years ago, that the left eye was misty. The mistiness increased slowly, and attributing the failing of sight to the injurious effects of his trade, he disregarded professional advice, till the right eye had given evidence of the same kind of obscurity that had attacked its fellow, and now he applied to Mr. Walton. The eye first diseased, the left, is virtually blind, for nothing can be seen with it, as in the centre of the cornea there is a brown oval opacity, placed transversely, large enough to cover the pupil, and dense enough to intercept light. It is of a sepia colour, and shaded towards the extremities, not raised, and possessed of the same lustre as other parts of the surface of the cornea.

The right eye is effected in a similar manner, but in a less degree, and enough of the pupil is yet uncovered, that with a magnifying glass the coarser works of his trade can be executed. There have not been any subjective symptoms, and he himself is quite unaware that there are brown spots on his eyes.

Mr. Walton directed atropine to be used to the left eye, the effect of which was to dilate the pupil beyond the opacity, and thus to enable objects to be seen with that eye nearly as well as with the other.

The right eye was then treated in the same manner, and the vision was improved. The patient now expressed himself quite satisfied with the benefit received, and desired to cease attendance, but yielded to the request of Mr. Walton to attend another day, that he might ascertain how far the opacities were capable of being removed by operation. An attempt was made to scrape a portion of one away; but a clear surface beneath could not be obtained, as the disease had extended into the true texture of the cornea, and perhaps completely pervaded it.

Dr. Taylor, who had taken his microscope to the hospital, to examine, in a fresh state, whatever might have been removed, found that the portion separated consisted of epithelium, some of which contained pigment granules.

I must beg to observe that, so far as my personal experience goes, opacities resulting from loss of substance of the cornea; in fact, cicatrices and interstitial deposits from inflammation are not capable of being pared away, but depositions, for the most part consisting of earthy materials on the surface of the cornea, and the accidental impingement of a foreign substance, as in the first case, may be so removed.

47. *Protuberance of Eyeball with Enlarged Thyroid Gland, Increased Action of Heart, &c.*—Dr. J. T. BANKS records (*Dublin Hospital Gazette*, June 1, 1855) the following example of this, which is particularly interesting from the opportunity having been afforded of a *post-mortem* examination:—

The subject of this case, a woman, æt. 30, was admitted into Whitworth Hospital, January 25, 1855. Her health had been good up to the age of fifteen, and no hereditary taint was discoverable. About the age of puberty she suffered much mental disquietude, from which she has never since been altogether exempt. She does not remember the exact period when the catamenial function was established; it had always been irregular, and for the last year and half had ceased altogether, having been suddenly arrested in the midst of a period. She has always been nervous and subject to palpitations of the heart. Every winter for the last few years has had bronchitis; some time since she suffered from a fit of violent vomiting and straining, and after this she perceived that her neck was swelled, and she felt a sensation of throbbing in it. Of late she has been much distressed by palpitations and pulsation in the neck, and a feeling as if she were being choked by something drawn tightly round her throat; for the last ten nights she has been almost sleepless, and utterly unable to lie down.

On admission, she presented the following appearance: Wild agitated expression of countenance; dusky hue of skin; eyes unnaturally prominent, staring, and brilliant; evident enlargement of the thyroid gland, more particularly of the right lobe; violent throbbing of the vessels of the neck, which were considerably augmented in size; one large superficial vein crossed the trachea.

The thyroid, permanently enlarged, becomes much more turgid on the occurrence of palpitation of the heart, or paroxysms of coughing; a purring thrill, a loud continuous venous murmur, and an interrupted arterial sound are present.

The area of precordial dulness is increased; the heart's action is tremulous and irregular in the extreme; a few unequal beats of extraordinary rapidity, and then a brief pause. No murmur was distinctly audible.

The pulse is small, feeble, and unequal, and so rapid as to render it almost impossible to calculate its frequency; the countenance indicates great suffering. She says her chief distress arises from inability to lie down, or to sleep quietly, from a "feel as if her heart was in her throat." She also complains of headache and frequent cough; during the fits of coughing, her urine passes away involuntarily. With the exception of bronchitic rales, nothing abnormal was found by auscultation of the lungs. The heart's sounds were more extensively audible than is usual in health.

From the date of admission, January 25 to February 6, no marked change in condition of patient; at the latter date, oedema of the lower extremities and a slight puffiness about the eyes, were observed. Greater respiratory distress; restlessness and mental disquietude. The size of the thyroid is rather greater than when first seen; position has a remarkable effect on the pulsation; on assuming the recumbent posture, which always causes dyspnoea, the throbbing of the vessels visibly diminishes, and the murmurs become almost inaudible. The urine is of a dark smoky colour, albuminous, and depositing a sediment which, on examination, was found to consist of broken down blood globules, sp. gr. 1.017. The heart's sounds more regular.

From this time the patient rapidly declined in strength; the oedema extended almost over the whole body; the cellular tissue of the back was the seat of extensive effusion; inability to sit up from weakness, and she says from the weight of her head, and giddiness. Still the pulse remains more regular (96). The character of the urine unchanged.

For the last week of life intense bronchitic rales were heard, and there was an abundant expectoration of a bloody fluid, not viscid.

It should have been remarked that the vision was perfect up to the close of life. There never was the slightest inflammatory affection of the eyes. Death, which occurred on the 7th March, was rather sudden. She had been speaking a few minutes before, and so free from struggle were her last moments, that those near her did not know exactly the moment of her death.

The morbid appearances presented on examination were in many respects interesting.

The thyroid gland, enlarged to four or five times its natural size, was found to cover, to a considerable extent, the front of the trachea. The right lobe was larger than the left: the thyroid glands were remarkably dilated.

The gland was dense, very solid to the feel, and lobulated. A section of different parts disclosed the existence of numerous cysts, containing a yellow fluid like honey. The contents of some of the cysts were dark coloured, and resembled coagulated blood. The microscopic appearances were similar to those observed and figured by Rokitanski in ordinary enlargement of the thyroid. The jugular veins were enlarged; the bronchial glands were found of an unusually large size; the heart was enlarged generally and the cavities dilated, but not to any considerable extent.

The heart was as large as that of an ordinary man, the subject being a woman under the middle size; the valves were free from disease, with the exception of slight thickening of the anterior edge of the mitral valve.

The lungs were highly congested, and the bronchial membrane bore the marks of intense inflammation.

The liver appeared as if in the earliest stage of cirrhosis; the spleen large and congested.

The brain softer than natural; the lining membrane of the ventricles much thicker than usual.

The kidneys had undergone the changes usually observed in the early stage of Bright's disease.

The case reported presents to our notice an example of a disease which is not very frequently met with, even by physicians enjoying extensive opportunities for observation.

Comparatively rare as it is to encounter the disease, it is still more rare to have an opportunity of investigating its morbid anatomy.

Few are the dissections recorded to which we can refer in the hope of arriving at a satisfactory conclusion as to the pathology of this most obscure affection.

That the disease, with all its characteristic phenomena, may exist without organic lesion of the heart or other morbid condition, tending of necessity to shorten life, is confirmed by the observations which have from time to time been published.

In the woman who was the subject of the present notice, violent palpitations of the heart had existed for many years, with irregularities of the uterine functions, and she had, moreover, suffered from the effects of violent mental emotion. This is the history of some of the recorded cases of the disease. Functional derangement of the heart then seems to be the *point de depart* in the greater number of instances of this disease. Nervous and hysterical females, however, are not the only persons who are found to labour under this disease. Men, though by no means so frequently, and females of advanced age, have also been found amongst the victims of the malady. We cannot, therefore, assign to abnormal states of the uterine function, an important part in the production of the disease. It has been noted in some cases that the catamenia were *perfectly regular*.

It may be observed, regarding one of the most remarkable phenomena of the disease, namely, the prominence of the eyes, that it has been observed to occur suddenly. In the present case, the woman was unable to inform us as to the time when her eyes were first noticed to have acquired this remarkable character; probably at the period when the thyroid gland became enlarged. To the learned President of the Pathological Society, Mr. Adams, we are indebted for the observation of the fact of the eyes having become prominent *suddenly* after a violent fit of coughing. The scanty materials from which to establish the pathology of this disease have been already alluded to. It is of value then to place on record every necroscopic examination, even though little, if anything, be contributed in the way of foundation, upon which to erect a theory as to the efficient cause of this most obscure disease.

MIDWIFERY.

43. *Cases of Labour complicated by locking of Heads of Twins in the Pelvis.*—*Case 1.* Dr. SIDNEY communicated to the Edinburgh Obstetrical Society the following particulars of a case of twins locked during labour, in which both children were born alive, and referred to various cases of the same kind which had been recorded.

Mrs. B., her fifth confinement and her second of twins; saw her during the day, when the pains were slow and weak, the presentation being high, and of the breech. I left word that when the pains became stronger I was to be sent for. In the evening they became so, and when I arrived the breech was just expelled; having relieved the cord from pressure on the head turning into the hollow of the sacrum, a hard tumour came pressing down under the arch of the pubis during a severe pain, preventing the presenting head being extracted. Upon examining carefully, I found this tumour nothing else than the head of a second child, jamming itself firmly on the first, and had some trouble in relieving the now locked heads; taking advantage of the interval of pains, and keeping up the head of the second child during the pain, I succeeded in extracting the head of the first child, and the second followed. The twins were of the usual size; mother and twins did well.

Dr. SIMPSON made some remarks on the treatment of such cases, and referred specially to the case brought before the society some years ago by Dr. Lewins, of Leith.

Case 2. Dr. MATTHEWS DUNCAN stated that he had been requested by Dr. Geo. Rosa to assist him in effecting delivery in a case of the above description. Dr. J. Sidney was also present during the delivery.

Mrs. F., æt. 21, was taken in acute labour of her second child at noon, after being for some hours affected with lingering pains. The body of a child was born at 2 P.M. Matters continued in this state, under very strong pains, till nearly 5 P.M., when Dr. Rosa first saw the woman.

Dr. Duncan saw the case soon after, and found that while the body of the first child was born, its head was prevented from entering the pelvis by the head of the second child, which was lodged in the hollow of the sacrum, having the neck of the first lying between it and the region of the thyroid foramen of the left side of the mother. The occiput of the second child was directed to the right side of the mother; that of the first child was in nearly an opposite direction. During a pain, both heads descended, but especially the second, which then pressed strongly into the outlet of the bony pelvis. It appeared to Dr. D. that both children might have been delivered entire. But a different plan was followed.

The child half born was certainly dead; and the second child was so also (as far as could be made out). The vagina was much stretched, and as if dragged upwards, and the uterine tumour tender to the touch. The woman had been long in this distressing state, and was urgent for relief. Dr. D., therefore, resolved to deliver quickly, and as easily and safely as possible for the mother.

With a bistoury he decapitated the first child, already certainly long dead. After this, a few pains were observed. The heads continued in the same relative position, and both were propelled downwards during the contractions, but without any decided progress. The forceps were then applied to the head of the second child, which was thus delivered almost without any traction. The head of the first child was then easily removed by seizing the stump of the cervical spine with a Lyon's forceps, and extracting it. The placenta was soon afterwards delivered (6 h. 15 min. P.M.). The woman made a very good recovery. The twins were large, well-formed children.—*Edinburgh Medical Journal*, Aug. 1855.

49. *New Reason for inducing Premature Labour.*—Prof. SIMPSON stated to the Edinburgh Obstetrical Society that he had been induced to bring on abortion in a case of pregnancy, on account of the presence of extreme dropsy and

dyspnœa from albuminuria. The patient, who was apparently dying at the time, recovered speedily and entirely after the abortion.

Dr. G. WEIR mentioned the particulars of a somewhat analogous case.—*Edinburgh Medical Journal*, Aug. 1855.

50. *Experiments on the Pelvic Articulations, to determine the Expediency of Symphyseotomy.*—Dr. KEILLER recently reported to the Obstetrical Society of Edinburgh the results of a series of experiments which he had performed with the view of ascertaining the nature and extent of motion in the sacro-iliac and pubic articulations, and of determining the expediency of the operation of Symphyseotomy. The experiments were performed on female subjects, several of whom had borne children, and in three of the cases an advanced state of pregnancy existed.

Dr. K. admitted that a certain degree of separation and motion occasionally took place in the pelvic joints, and that a small amount of space might be obtained by forcible separation of the pubic articulation, either by nature or operative procedure, but denied that the space so gained was in the direction where it was usually required in deformed pelves, viz: in the antero-posterior diameter of the brim.

Dr. K. demonstrated that, on dividing the inter-pubic fibro-cartilage, great retraction of its fibres took place in front, where its structure was comparatively thick, thus giving rise to the idea that considerable separation of the bones had taken place, which was not actually the case, a point easily ascertained by examining the posterior aspect of the joint, where the bones, after the operation, would be still found almost in close apposition. This apparently great separation of the articulation in front had no doubt frequently misled those who believed that by division of the symphysis real space was gained.

Dr. Keiller stated that he could not in any other way account for the conclusions arrived at by Dr. Duncan, who, in his recent communication to the society on this subject, had affirmed that, in experimenting on the pelvis of a subject, he had succeeded in readily procuring about two inches of space between the pubic bones, by simply dividing the symphysis, and without applying any force to increase the amount of separation.

Dr. Keiller had now performed the experiment of dividing the symphysis pubis on a great number of subjects, and had carefully noted the comparative measurements, and had arrived at conclusions widely different from those come to by Dr. Duncan.

Dr. SIMPSON exhibited three recent pelves illustrative of this subject, and mentioned the results of his experiments with them. He agreed with Dr. Keiller that the extent of separation and motion was by no means so great as had been stated by Dr. Duncan. Dr. S. considered the Sigaultian operation inexpedient and dangerous, and thought that it would not again be revived from its present obsolete position.

Dr. DUNCAN made a few remarks on the preparations, and adhered to the opinions he had formerly expressed as to the mobility of the articulations in pregnancy, and as to the propriety of the question of adopting the Sigaultian operation in particular cases being reconsidered.—*Edinburgh Medical Journal*, Aug. 1855.

51. *Perineal Fistula left by the Transit of the Infant through the Perineum.*—Prof. SIMPSON records (*Edinburgh Journ. Med. Sci.*, July, 1855) the following example of this rare lesion:—

The patient was attended during her first labour by a practitioner in the west of Scotland. The labour—as he subsequently informed me—was tedious, particularly during the advance of the hand through the lower part of the pelvis. After the perineum had become much stretched and distended by the child's head, and when the artificial support of it by the hand happened to be for a short time withdrawn, a very strong expulsive effort supervened, and the practitioner was recalled to the patient in consequence of her loud cries. To his surprise he found the head passing, or in fact, already nearly entirely passed through a rent in the perineum; and the next pain expelled the body

of the child through the same opening. Through this same perineal perforation the cord and placenta were delivered. The sphincter ani and the anterior edge, or fourchette, of the perineum remained untouched. The sides of the laceration did not entirely unite. A year subsequently to her delivery I saw her, along with the late Dr. Dawson, of Bathgate, under whose care she had come with symptoms of phthisis. The perineal fistula still remained, and, as we found on a *post-mortem* examination, some months subsequently, it was about the size of the barrel of a goose-quill. The opening was situated about half an inch behind the posterior commissure of the vulva. The perineum was very thin at the site of the fistula, and converging lines of old cicatrices were still visible on its mucous surface. But its anterior edge, or fourchette, was strong and dense, and placed unusually far forwards over the vaginal orifice.

Prof. S. has been able to find on record only two other cases, in any way analogous, of perineal fistula originating in perineal perforation during labour. They are described by Marter of Königsberg,¹ and Halmagrand,² of Paris.

In a primiparous woman, to whose assistance Marter was summoned by a midwife, he found the head of the child already passing through a central laceration in the perineum. The child was speedily pushed, by the strong pains that were present, through the abnormal aperture, and the placenta afterwards followed through the same crucial-shaped laceration. Inflammation of the lips of the wound subsequently occurred, and, despite of the use of ligatures, a perineal fistula remained, by which the menses escaped during the two subsequent years. She then again became pregnant; and this second child was born naturally by the vulva.

In 1838, a patient applied to Halmagrand, sometime after delivery, with a perineal perforation not yet cicatrized, and forming a communication with the vagina. He cut the anterior bridle of the perineum, which was slender, made raw the edges of the fistula, and brought them together by the apposition of the thighs alone. Reunion and cicatrization took place in a few days. The patient was subsequently confined without any renewal of the perineal lesion.

52. *The strictest Caution requisite in Chloroformization in Midwifery Practice.*—Dr. M'CLINTOCK briefly related (June 6, 1855) to the College of Physicians in Ireland, the history of a case of chloroformization in midwifery practice, in order to show the great necessity that existed for always using the utmost caution and circumspection in the employment of chloroform inhalation in labour, as well as the great importance of intrusting its exhibition to none but a medical man. He did so because the writings and practice of some of the foremost advocates for anæsthesia had led many persons to suppose that the administration of chloroform to parturient cases was almost, if not entirely, free from danger—a most dangerous fallacy, and one that would inevitably lead to disastrous consequences if generally acted on.

The leading circumstances of the case he brought forward were these: A healthy woman, aged 28, was admitted into the Lying-in Hospital, in labour of her first child, some days before. The first stage was very tedious, in consequence of an unyielding condition of the os uteri, and much general irritability. With a view to relieve this state, and to give her some rest, as she was much harassed by frequent short pains, it was thought advisable to put her under the influence of chloroform. For about an hour she got it in small quantities—in fact, merely *chloroform à la reine*—but without experiencing any benefit, and without its producing any anæsthetic or soporific effect. The quantity put on the sponge, a large cup-shaped one, was now increased, but still could hardly have exceeded one drachm, and was not more than is habitually given in the hospital to patients undergoing obstetric operations, to which cases its use is chiefly limited; and it was administered by the senior assistant, who has had ample experience of its use, having given it before in hundreds of instances. After the sponge was reapplied to her mouth, and she had taken three or four inspirations, a change came over her countenance, the eyeballs turned up, the

¹ Siebold's Journal fur Geburtskunde, vol. ix. p. 726.

² Demonstrations des Accouchemens, p. 577.

pulse left the wrist, respiration was suspended for a space of time that would have occupied about three or four inspirations, and some froth collected at the angles of the mouth. On the first appearance of these alarming symptoms, the sponge was instantly withdrawn, the free circulation of fresh air was promoted, the face and chest were aspersed with cold water, and ammonia was applied to the nostrils. It should, perhaps, be mentioned that during the entire of the above period she was in bed, and lying down. Under the influence of these restoratives, animation gradually returned. It was evident, however, to all around—and many of the pupils were present—that she was all but gone, and that her life was preserved by the early recognition of the poisonous effects of the medicine, and the prompt and judicious employment of appropriate restoratives. Hardly any one will venture to deny that had this woman been in non-professional hands, her life would have been lost. That she got a dose of chloroform which, *to her*, was an overdose, is sufficiently obvious; and yet the quantity given, the quality of the chloroform used, the mode of exhibition, and even the administrator, were all the same as on hundreds of previous occasions, when everything went on favourably. Hence, then, the absolute necessity for *invariably and uniformly observing the strictest caution, prudence, and circumspection in the employment of this powerful agent, and never intrusting its exhibition to a non-medical person.* A case that strikingly illustrates these remarks is recorded in the *Medical Times and Gazette* for April 14, 1855, where a lady died in the course of a natural labour, from the effects of chloroform administered to her by the nurse, on a handkerchief, without the sanction or knowledge of the doctor, who was in the house at the time. The quantity used in this case, with fatal effect, could not have exceeded five fluidrachms.—*Dublin Quarterly Journal of Medical Science*, Aug. 1855.

53. *Extra Uterine Fœtation.*—Dr. W. MARSDEN relates (*Medical Chronicle*, May, 1855) the following case of this:—

Madame B—, a handsome, intelligent, and lively brunette, now aged thirty-four years, of the Parish of Beauport, in the District of Quebec, is the mother of two children, and was attacked with morning sickness in January, 1848, having ceased to menstruate between five and six weeks previously. On a Saturday afternoon, about the seventh week of pregnancy, she fell through a trap door, by which one leg was arrested in the fall, and extended at right angles from her body, causing considerable pain and uneasiness, which, however, soon passed off, and she continued to attend to her domestic affairs, and went to church on the following day, Sunday. On Monday, in the course of the morning, severe intermitting, cutting and lancinating pains in the abdomen came on, and she was obliged to go to bed, to which she was confined for four or five days, and during that period warm applications, poultices, &c. were used, and resulted in a mere show, for a moment. From this period, until the expulsion of the fœtus, which occurred in the thirteenth month of pregnancy, she was troubled with constipation, which became more obstinate and intractable as time advanced. She also suffered extreme pain in the lumbar region. She expected to be delivered about the beginning of September, at which time, true to her reckoning, labour pains came on, and the midwife who had attended her on two previous occasions, was summoned to her aid. Three days of active labour followed, accompanied by regular expulsive pains, and she felt the movements of the fœtus distinctly all the time, when all action and movement suddenly ceased, and she felt a dead weight (in her own words) rather behind and to the right side. Vomiting now set in and continued daily, of fetid green or bluish clayey looking matter, which was sometimes also gray and ashy looking, and sometimes black: but, always very offensive to the smell. From the last period referred to, until the middle of December, she was under the care of several medical practitioners, whose treatment, besides general treatment, consisted in a great measure of hot poultices, sinapisms and blisters. From September, when the labour pains ceased, until the termination of the case, the alvine evacuations were of the most offensive character.

The treatment of Dr. Blais, up to the 21st December, offers nothing new or worthy of remark, and had resulted in no clear diagnosis, and it is therefore

needless to detail it. At this time, however, Madame B—, who seemed to be sinking, insisted, as she had done before, that "the child was in the back passage." A careful examination by external manipulation, and per vaginam took place, which is thus described by Dr. S.

The abdomen was large and distended, and had a hard lumpy feel low down on the right side. On the left it was soft, and felt empty. On examining per vaginam, found the os uteri low down, and the cervix uteri long and soft. On passing the fingers up behind the cervix uteri, two oblong hard bodies could be felt, lying in a transverse direction, high up in the rectum.

Dec. 22. High fever, vomiting, skin hot and dry, pulse hard, wiry and frequent. Ordered copious enemata, which were administered. On the 23d, the right foot of a child came away with one of them. Pains in the loins intense. On examination per vaginam, found that the hard bodies before alluded to had disappeared, and found in the situation they had occupied, "*une bosse molle*," a soft bump or swelling.

On the 24th, finding the patient in an apparently sinking state, eyes protruding, fixed and glassy; insensible and immovable, we resolved to explore the rectum. Having dilated it carefully, Dr. B. introduced the right hand, and passing it far up the rectum, while I assisted in dilating and protecting the parts, he felt and secured the foot and legs of a foetus, and by careful traction removed a female child eighteen inches long, perfectly formed, and only slightly decomposed about the head, right leg and left arm. Ordered emollient injections of mallows, &c. On the 25th and 26th insensibility continued. On the 27th, however, consciousness returned, patient complained of a tearing and cutting pain on the left side of the anus. On introducing a finger into the vagina, found a large body which felt like an abscess. Introduced the catheter, and to our great surprise, drew off upwards of four quarts of healthy pus. On the 28th and 29th, emollient poultices to the abdomen and enemata continued. On the 30th, finding that the tearing pains continued, introduced the hand into the rectum, and extracted with some difficulty the two parietal bones, which were large and hard, and not at all injured, but dark coloured.

From this time, until the 15th of January, 1849, she continued to mend, having no unfavourable symptom, with the exception of retention of urine on two occasions, when upwards of three quarts were drawn off by the catheter. Her convalescence was regular and progressive, the only remarkable feature being extreme weakness of the legs, almost amounting to paralysis. She is now in perfect health, active and cheerful. Catamenia regular and abundant, and enjoys sexual intercourse. Feels as well as she did before her illness.

54. *Epidemic of Puerperal Fever in Brakel, District of Minden.*—Dr. Disse has given an interesting account of this disease, which occurred in a small community, and under circumstances which permitted the narrator to take a comprehensive view of the sanitary condition of the whole population, and the puerperal history of every woman delivered during the period of interest. He was thus enabled to give an unbroken history of a particular epidemic.

Brakel is a town containing 3000 inhabitants; it is one of the healthiest in Westphalia; and, hitherto, epidemics have been rare. It is surrounded on two sides by the streams Nethe and Brucht, which unite below the town to pour their waters into the Weser. The soil is clayey.

The epidemic appeared on the 15th September, 1852, and lasted throughout October, November, December, and ended about the 11th January, 1853. At the time of the outbreak, bilious diarrhoea prevailed in Brakel and the neighbourhood. The lying-in women were free from this. From the 15th September to the 11th January, 28 women were delivered in Brakel, of which 13 were attacked with the fever: 12 died; the remaining 15 remained healthy. All the women attacked had, with one exception, boys, who, two only excepted, died soon after birth, with convulsions.

The epidemic ran the following course: The first case occurred on the 15th of September in a healthy multipara, aged 36; she was taken on the third day after delivery, and died on the fifth. 2. 20th of September, a healthy multipara: seized the fourth day, died the seventh. 3. 7th of October, a multipara:

seized on the third day, died on the fourth. 4. 8th of October, a strong, healthy multipara: seized on fourth day, died on the eighth. 5. 10th of October, multipara: seized second day, died fourth. 6. 13th of October, multipara: seized second day, died fourth. 7. 15th of October, multipara: seized third day, died on the eighth. 8. 18th of October, primipara: seized third day, died seventh. 9. 27th of October, primipara: seized fourth day. This was the only woman who recovered. Herself and child did well. 10. 30th of October, multipara: seized second day, died seventh. 11. 2d of November, multipara: seized second day, died sixth. 12. 9th December, multipara: seized second day, died seventh. 13. and last, 11th of January, a primipara: seized second day, died third.

The 15 other women delivered within the same period, and who escaped the epidemic, were delivered on the 19th and 23d of September; 16th, 17th, 25th, 27th, and 28th of October; 11th, 16th, 18th, 21st, and 25th of November; 8th, 17th, and 30th of December. Of these, 9 bore girls and 6 boys.

All the 28 labours were natural.

Symptoms.—On the second or third day a strong shivering-fit appeared, followed by great heat and unquenchable thirst. Soon after this, burning and cutting pains came on in the region of the uterus, increased by pressure, spreading towards stomach; great tympanitis. Tongue in the middle moist, sides red and dry. In most cases, nausea and even vomiting; in many cases, constipation; in some, diarrhoea of mucous and offensively-smelling masses. Pulse from 120 to 130: feeble, irregular, compressible. Respiration oppressed. Heart-beat tumultuous. Urine in some retained; when passed, red, turning turbid. Skin dry (calor mordax). Perspiration, milk, and lochia arrested. All evinced anxiety of countenance, a yellowish-green complexion, features distorted. All, at beginning, had unclouded consciousness, but with feeling of a heavy sickness. Towards the end consciousness was disturbed, meteorism reached its greatest point, and the pulse attained its greatest frequency.

Autopsy of case 11, thirty hours after death.—Abdomen gave vent to an immense volume of stinking gas, and a thin, brown, offensive pus flowed from the peritoneal sac. Stomach distended with gas; on the mucous membrane a gray, slimy, stinking mass; the duodenum and small and large intestines showed strong vascular injection, of a dark-brown or blackish colour, and distended with air; between the convolutions of the small intestine a great quantity of a bloody serous exudation, but without any fibrinous coagulation. The mesentery was deeply injected, and of blue-black colour. Peritoneum in similar condition. Liver, large, deep-black, full of blood; parenchyma, soft, lacerable. Kidneys, same character. Uterus, large; peritoneum, very vascular, dark-black; broad ligaments, same; ovaries, same; also Fallopian tubes. Inner surface of uterus covered with a chocolate-coloured, slimy, stinking exudation; os internum deep-black; nowhere coagulation; muscular substance soft, lacerable. Its vessels contained no pus, as in metro-phlebitis. Vagina in same condition as uterus. In the iliac veins was found slimy, stinking, chocolate-coloured exudation.

Etiology.—Dr. Disse thinks it connected with the epidemic of bilious diarrhoea, which caused a similar blood-poisoning in the puerperal women. It could not be caused by any dissection poison, as observed by Semmelweis in Vienna. The first patient was examined by no one who could from any source be so infected.

Treatment.—The septic character of the disease precluded antiphlogistics. Turpentine, camphor, and ipecacuanha were used. The patient who recovered was treated with turpentine, internally and externally.—*B. and F. Med.-Chirurg. Rev.*, July, 1855, from *Monatschrift für Geburtsche*, Feb. 1855.

55. *Contagiousness of Puerperal Fever.*—M. DEPAUL believes that puerperal fever, especially when epidemic, is contagious. He relates the following examples. During an epidemic of puerperal fever at the Maternité, a midwife was entrusted with the case of a woman recently delivered, affected with a most severe metro-peritonitis. One morning this midwife, in giving the attention to the patient which her situation required, was powerfully impressed,

and as if suffocated, by the emanations which escaped on raising the bed-clothes. The same evening a strong shivering fit occurred, her abdomen became very painful, pulse small and frequent, greenish vomiting, diarrhoea; at last all the symptoms most characteristic of puerperal fever. She died in forty-eight hours. At the autopsy the changes usually observed in cases of this nature were found, the tissues of the uterus being unaltered. M. Depaul was enabled, moreover, to establish that this young woman was not only not in any form of puerperal state, but that she presented all the signs of virginity.

A physician was engaged in making the post-mortem examination of a woman who had died of puerperal fever, when he was summoned to attend a labour. Precautions of every kind, change of clothes, washing, could not rid him of the smell that autopsies of this kind commonly leave on the hands. The labour took place in the usual way, but in the evening the patient was seized with a most severe puerperal fever, and died the next day. M. Depaul relates also another similar case in which the woman died in a few hours.—*L'Union Médicale*, March 3, 1855.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

56. *On the Effects of the Death of the Fœtus on the Duration of Pregnancy.*—Dr. ROBERT JOHNS, formerly Assistant Master to the Lying-in Hospital, Dublin, makes (*Dublin Quarterly Journal of Medical Science*, Aug. 1855) some interesting observations on this subject, of which we shall present an abstract.

Mrs. M., aged 40, mother of four living and two premature still-born children, when pregnant for the seventh time, engaged Dr. J. to attend her in her then approaching confinement, which she stated would occur on the 11th of April, 1853, as she had menstruated on the 11th of July, 1852.

On the 12th of February this lady sent for him in consequence of uterine hemorrhage, which had set in on the previous evening, and was then continuing, but unaccompanied by pain.

"I made," says Dr. J., "a vaginal examination, and satisfied myself that she was not in labour. I also examined the abdomen most carefully, and then mentioned to her my conviction that she was not so long pregnant as she believed herself to be, for that the womb had only attained the size and position in the abdomen usual at six months of pregnancy; to which she replied that she had quickened on the 10th of November, when four months pregnant, and had continued to feel her child up to the end of the sixth month, but that since that period she was not sensible of its vitality. At this visit, the pulsations of the fœtal heart were not discoverable, but the placental soufflet was indistinctly audible, abrupt, and weak. The hemorrhage ceased, rest being the only means employed.

"On the 11th of the following month (March) I was again summoned to Mrs. M., the hemorrhage having returned on that morning, but without pain, as on the former occasion. She being very hysterical, I ordered an anodyne draught, which, together with rest in the recumbent posture, had the desired effect of calming her, and arresting the discharge. I embraced the opportunity of this visit to test the accuracy of my former prognosis, when I was much surprised at discovering that the uterus had not increased in size since my visit of the preceding month, its fundus then having reached as far as the umbilicus, which was protruded. My patient, however, insisted that she had become much larger, which fact, doubtless, was attributable to flatulency, which obtained to a great extent, and from which she suffered much. I then made a very careful stethoscopic examination of the whole abdomen, but I could not discover either the fœtal pulsation or the bruit placentaire. Borborygmi were, however, very audible, but not in positions likely to mask the other sounds.

"The occasion of my next visit to this lady was when labour had set in, which

occurred at 9 o'clock P. M. of the 11th of April, 1853. The uterus then held the same position in the abdomen as on my two former visits. Once more a stethoscopic investigation was instituted, but not with happier results than before. Labour progressed slowly till half-past four o'clock A. M. of the 12th of April, when, with one violent pain, she brought forth the placenta with the membranes unruptured, containing about a pint of whitish fluid of the consistence and appearance of skimmed milk, in which was floating a dead, dried-up, and withered foetus, apparently of about six months, presenting very much the aspect of having been for some time macerated in spirits of wine. The placenta and membranes were healthy, and neither they, the child, nor the liquor amnii exhaled the least unpleasant odour. There was not any hemorrhage or other bad symptom after delivery, and she recovered very quickly. Mrs. M. mentioned to me, in conversation, that she had enjoyed much better health than usual whilst carrying this child, and that she had gained flesh.

"The points of interest in this case, to which I wish to direct attention, are—

"1. The death of the child occurring at so late a period of uterine life, and the arrest of development in the uterus consequent thereon, not having terminated in labour before the full period of gestation.

"2. The absence of putrefaction in the foetus and the secundines.

"3. Nature's initiatory efforts at the seventh and eighth months to throw off the foreign body, and her success by inducing labour at the ninth month, on a day corresponding to her abortive attempts, and to the last appearance of menstruation.

"1. It is a well-known fact to the practical accoucheur, that when a foetus is blighted during the early months of utero-gestation, it, as a general rule, is expelled on its death, and that when the child dies after the expiration of the fifth month, labour sets in at a period of about ten or fifteen days from the occurrence, except in the case of twin pregnancy, where one child dies; then nature, in consideration of the living one, does not arrest gestation, or, in rare cases, she throws off the dead child, but permits its uterine companion to be fully developed.

"It is not very unusual for ova, when blighted early in pregnancy, to be carried in utero for months, or even years, but in most of, if not in all, such instances they are found converted into hydatids or moles; however, in the hydatid pregnancy, the uterus, instead of being arrested in development by the cessation of life in the ovum, is always more enlarged than in natural pregnancy.

"Not very long since I met with an ovum mostly converted into hydatids, which a lady had retained in utero for upwards of twelve months, and, as she had increased to a great size, much family disappointment was experienced at the result.

"Guillemot says: 'It often happens that labour does not set in immediately after the cause which has provoked it. That when the foetus alone has experienced the effects, it declines and soon perishes; the disturbance which this mode of dying produces, by insensible degrees, in the placental and uterine circulation, gradually solicits uterine contraction, and prepares the womb for the expulsion of the foetus. It is from this period that the cervix uteri begins to thin, and its os to open; at length the contractions become more energetic, and the dilatation consequent thereon allows the product of conception to escape; the time at which this expulsion occurs is generally about nine or ten days from the experienced accident; sometimes it occurs at a very distant period from the death of the foetus. I remember having attended, in 1837, a young woman who, when five months pregnant, fell on the buttocks; after the shock which she experienced, she ceased to feel the movement of her child, which she had felt up to that time; notwithstanding two bleedings which I adopted, and all the precautions taken, pregnancy remained as if stationary, and it was not till the nine months had passed over that labour set in, when, after ten hours, the ovum was expelled entire. The foetus was arrested at the state of development which it had at five months; the membranes were dense, and more solid than they ought to be if the child had been born alive; the placenta was withered.'

"A case very similar to Guillemot's occurred in my practice very lately, but the development was arrested at four and a half months. M. Martin relates six cases in which the child had died during the latter months, in four of which the child was thrown off within fifteen days from its death, in one within three weeks, and in one within four weeks from the same occurrence.

"In the summer of 1843 I was called to see, in consultation, a lady at the eighth month of pregnancy in the most violent puerperal convulsions, but which were got under by the ordinary means—venesection, cathartics, tartar emetic, &c. During my attendance, I was asked one day, by her husband, what was likely to be the issue of the case, and if she should have a return of the convulsions; to which I replied that, when labour set in, it might be attended with the fits, but that, as very active measures had been employed, I was in hopes she was pretty safe from them; but that his lady was then carrying a dead child, and that she should be confined of it within fifteen days. This latter part seemed to puzzle him very much, for he thought, as did also the nurse-tender, that, as labour had not then begun, she should go on to the full time. Suffice it, however, to say, that on the twelfth day from this conversation I was summoned in haste to Mrs. B., who was almost immediately on my arrival delivered of a dead and putrid child of about eight months. She had not any return of the eclampsia, and finally recovered.

"In speaking of twin conceptions, Guillemot remarks: 'If the womb cannot accommodate itself to the distension necessary for the growth, it happens sometimes that, instead of abortion taking place, one of the twins dies, and the other continues to grow beside its uterine brother, which, by the pressure it has undergone, becomes flattened.'

"I find the following in the work of Boivin and Duges: 'Sometimes, in twin pregnancy, one of the ova is affected, the other is developed as usual; in this case, the mole is expelled with the secundines of the healthy foetus, or some days afterwards; at other times this complication has induced premature labour; but it is of much rarer occurrence to witness the premature expulsion of the mole at the seventh month, and the subsequent natural progress of gestation. It is remarkable that miscarriage is not the most usual event in such pregnancies, and that the changes induced in one of the ova are not communicated to the other.'

"M. Martin says: 'In the case of twin conception, there may be common or separate membranes; one of the foetuses may die, at a more or less advanced period of pregnancy, without the life of the other being compromised, or even its health injured, or labour being induced.' He then relates three cases illustrative of this fact.

"M. Perret records one such case. M. Bugnard also mentions another. M. Sarrajeane states that he met with two similar cases.

"While I was Assistant Master to the Dublin Lying-in Hospital, a female in the institution brought forth a full-grown and healthy child, at the full time, in whose membranes was contained a blighted foetus of about two and a half months, but not putrid. May not such cases as this explain the so-called instances of superfetation?

"M. Biliotel narrates the case of a lady who, at the end of the seventh month of pregnancy, brought forth a still-born child, and who, in two months after, was safely delivered of a healthy, full-grown female infant. It is more than probable that the premature labour here alluded to was induced by a fall on the ice, which the lady is stated to have experienced about fifteen days previously.

"I have at present under my care a lady who, when three months pregnant, expelled a foetus, but not any placenta, although all attempts, *per fas et nefas*, were resorted to—hot baths, &c. In about two months after the abortion, being greatly alarmed at the increase in her size, and the retention of the after-birth, she came to Dublin to consult me, when, to her great surprise, I told her that she was still pregnant, and carrying a living child, at about the fifth month of utero-gestation. She is now enjoying excellent health, and is progressing favourably to term.

"2. I find cases recorded by Martin, Perret, and Raillard, in which the child had been carried for a period of time varying from two to six months, all pre-

senting, on expulsion, appearances very similar to the one now under consideration, but in each the death of the foetus took place before the fifth month. I am not, however, aware of any having been otherwise than decomposed, who had perished at so late a period as six months, if carried for any length of time beyond its demise: the longest period of which detention had been one month (except in the case of twins, as before mentioned), and the most usual period about a fortnight.

"M. Martin, of Lyons, says: 'When the foetus dies from the second to the fifth month, it fades, wrinkles, and dries up; it then resembles a small yellow mummy, or very much a foetus for a long time macerated in alcohol; the placenta often participates in this state of withering, and the liquor amnii is wanting, or is replaced by a thick humour like earth, which incrusts the foetus;' but, says the same author, 'when the child dies from the fifth month till the ordinary period of gestation, the little corpse mortifies, increases in size, and exhales a horrible fetidity which characterizes putrid fermentation.'

"3. The attempts of nature alluded to under this head are, I conceive, negative proofs of the rarity of the case.

"It may be asked, what grounds have I for stating that the child in Mrs. M.'s case ceased to live at the time specified? To which I would advance the following:—

"1. The mother having ceased to feel the foetal movements after the sixth month, she having done so up to that period.

"2. The size of the uterus having corresponded to the same period of pregnancy.

"3. The pulsations of the foetal heart not having been audible at the seventh month, and, at the same time, the placental soufflet having been very indistinct, and its character, as before observed, having been much altered: a change mentioned by Kennedy as frequently obtaining in this sound for some short period after the death of the child, and previous to its final cessation—of the accuracy of which I can confidently speak from long and repeated experience.

"4. Not any sound having been discoverable in the abdomen after the seventh month.

"5. The size of the child and placenta, when expelled, having corresponded to that of one of six months' pregnancy.

"The popular belief of the size of the child having an effect upon the corpulency of the mother, seems to gain some support from this case.

"The following aphorism of Mauriceau is, I think, very pertinent here: 'The size,' says he, 'of the dead aborted foetus does not always correspond to the pregnancy, for it ordinarily has, when expelled from the womb, only the size to which it had attained when its vitality was destroyed.'

"I conceive that in the contemplation of this case is involved a very serious and important question—what effect has the death of the child on the duration of pregnancy? This, at a superficial view, may appear of little moment; but, on deeper reflection, it shall become self-evident that a false prognosis in such a case as the one before us may induce very fearful consequences to the physician, or may embitter the happiness of families by causing wounds that never may be healed, separations never to be reunited. In illustration, the following case is by no means improbable:—

"Captain B., R. N., marries, and leaves his youthful bride, to join his ship, three weeks after his marriage, she then being pregnant, but without his or her own knowledge. Gestation progresses favourably for six months, at the end of which period the child ceases to live, but is retained within the womb till the full period of natural pregnancy; this viscus not having enlarged after the child's death, the mother never felt her child, nor was she at all conscious of her condition, being necessarily inexperienced in such matters, besides being, as she supposed, unwell each month (which discharge may have depended upon ulceration of the os uteri upon nature's attempts to throw off the dead foetus, which had become a foreign body, or upon many other causes too numerous here to relate). However, at the end of the ninth month her husband returns, expecting to find a young and spotless wife, to be alike participator of his joys and griefs, when, alas! to his horror and dismay, he finds that she has just given

birth to a six months' child, but dead, and, in other respects, like Mrs. M.'s baby. Is it not natural that he at once accuses his lady of infidelity? and what protestations of hers as to her innocence, be they never so solemn, shall convince him that she still is not guilty? This is the juncture at which the physician may be the balm or the wormwood; therefore, upon him rests the responsibility of deciding the question. But if he be ignorant of the possibility of a dead child being carried in utero for such a period, he condemns the lady, and, I need hardly say, the consequence is too apparent. If, however, subsequently, the ill-judged, ill-treated, and unfortunate lady's innocence be proved, what shall become of the reputation of that physician who unhesitatingly pronounced her guilty? But if no such happy result should ensue to the lady, what shall and ought to be the feelings of that man, when, in after years, he shall discover his ignorance, and think upon the mischief it had entailed upon society?"

57. *On Death by Stifling*.—Of 116 new-born infants, the cause of whose death M. TARDIEU was called upon to investigate, he found that 58 had died stifled. The signs of this mode of death not appearing to him sufficiently set forth, he has performed a great number of experiments, varying as much as possible the mode of producing the suffocation—such as occlusion, compression of the parietes of chest and abdomen, burying alive, or confinement in a limited space. He has also compared the signs of death from suffocation with those of other kinds of asphyxia.

The change to which most importance should be attached is the dissemination of sanguineous extravasations under the pleura and hairy scalp. In whatever degree, and in however small numbers these may be present, they suffice to positively determine that suffocation has been really the cause of death. To these lesions are often added, but in a less constant manner, ecchymosed spots beneath the pericardium, the rupture of some of the superficial pulmonary vesicles, and the presence of a fine white or slightly rose-coloured foam in the air-passages. The same observation applies to the external signs of violence, as flattening of the nose and lips, excoriation of the integuments, etc. The multiplicity and extent of such lesions will indicate, if not the duration, at least the energy, of the resistance offered by those who died stifled. These signs permitting us to distinguish with certainty death from suffocation, from death from hanging or drowning, and may often be of great service in preventing our confounding homicide with suicide.—*Med. Times and Gaz.*, Sept. 1, from *Bulletin de l'Académie*, tom. xx. p. 97.

58. *Poisoning by Colchicum*.—By Dr. CASPER and JULES ROUX.—Cases of poisoning by colchicum are rare, and we are desirous of bringing under our readers' notice the accounts of no less than nine autopsies, made under the most favourable circumstances for examination and comparison. Four of these cases came under Dr. Casper's notice last year at Berlin; but as he was only concerned juridically in the examination of their bodies, he does not furnish much account of the symptoms exhibited during life. Four hearty young men having stolen some tincture of colchicum seeds, they each drank a glass of it, believing it to be bitter "schnaps." Vomiting, purging, and great prostration ensued, the intellect remaining quite clear. They took the poison on the 20th February, and by the 22d they were all dead. Dr. Casper describes each examination in detail, and then sums up the appearances under two categories:—

1. Appearances which were observed in all the bodies, and therefore may be received as signs of colchicum poisoning. (a) Putrefaction did not occur earlier than usual. (b) The fluids contained in the stomach, and the urine exhibited strong acid reaction. (c) The blood was of a thick consistence, and of a dark cherry-red colour. Dr. Casper has met with this appearance of the blood in sulphuric acid and other poisoning. (d) Immense distension of the vena cava inferior. (e) The kidneys were loaded with blood. (f) The bladder contained more or less urine. (g) There was no hyperæmia of the liver. (h) The right side of the heart contained much blood, but the lungs were in their normal state. (i) There was great congestion of the brain. 2. Among the organs, the appearances of which did not correspond after death, foremost was

the stomach. In one case there were the signs of a true gastritis and commencing enteritis; in another, fulness of vessels and ecchymosis, due evidently to stasis; and in the two others the organ was quite normal in appearance. Thus these four cases should teach us caution, by showing how much individual circumstances may influence the appearances caused by poison in the organ which first receives it. The varying appearances of the contents of the stomach and of the gall-bladder must also be regarded as accidental.

The cases related by M. Jules Roux occurred some years since, in the persons of five convicts at Toulon, to each of whom 60 grammes of tincture of colchicum were given in error for cinchona. In about two hours they were seized with colic, purging, and vomiting. Excessive prostration ensued, and they complained of burning heat along the œsophagus and in the abdomen, together with insufferable thirst. The intellect, sensation, and movement remained unimpaired to the last. Three died during the night of the day on the morning of which the poison was swallowed, and the other two expired the next morning. At the *post-mortem* examinations, made 36 hours after the death of the last patient, the vermilion redness of the muscles of the splanchnic cavities, and the remarkable state of preservation of the bodies, struck all present. The mucous membrane of the stomach and intestinal canal was found much softened, and at intervals reddened, but nowhere ulcerated. The spleen, liver, and kidneys were much gorged with blood, and the bladder, containing a little urine, exhibited a few red patches. The heart was flaccid, and contained a little dark blood and a few coagula. The vena portæ, and vena cava inferior were distended with blood of the consistency and colour of red currant jelly. The lungs were in a normal condition. The cerebro-spinal axis exhibited great injection, and general, well-marked ramollissement. From the progress and results of these cases, M. Roux believes with Giacomini and the Italian school, that the colchicum operates by its hypæsthenizing effects upon the nervous system rather than by inducing inflammatory action of the digestive passages.

Dr. Casper enters at some length into the question of the detection of *colchicin* by Stas's process in the bodies of persons poisoned by colchicum, which he declares is quite possible. We must refer our readers to the paper for the steps of the analysis pursued by M. Schacht. We may, however, mention that this chemist calculates that the fatal doses of colchicum in these fatal cases did not exceed from two-fifths to half of a grain.—*Med. Times and Gaz.*, June 16, 1855, from *Casper's Wochenschrift*, Jan. 1855, and *L'Union Médicale*, 1855. No. 36.

59. *On Antidotes to Poisoning by Copper*.—Dr. SCHRADER, of Gottingen, having undertaken an experimental investigation into this subject, arrives at the following conclusions:—

1. Hydrated magnesia is just as little of an antidote as the alkaline carbonates, the hydrated oxide of copper that is formed being gradually dissolved by the stomachal and intestinal acids. Magnesia may, however, retard the effects of the poison, although it cannot entirely counteract them.

2. The hydrated sulphuret of iron decomposes the salts of copper immediately, and the sulphuret of copper is well-nigh insoluble in the juices of the alimentary canal. How far the sulphuret of iron may act disadvantageously, by liberating sulphuretted hydrogen gas, further experience is required to show.

3. The reductive power of sugar takes place, at the temperature of our bodies, far too slowly to be available in acute copper poisoning. It may be useful, when taken abundantly in water, to excite or favour vomiting.

4. Of all pharmaceutical substances ferrocyanide of potass is the best. Large quantities may be taken without material disadvantage to the economy; the ferrocyanide of copper, which is immediately produced, being very insoluble.

5. Milk and white of egg neutralize poisonous salts of copper, and have the advantage of being easily obtained. Care should be taken to evacuate as rapidly as possible the albuminates and caseates of copper thus produced.—*Med. Times and Gaz.*, May 5, 1855, from *Buchner's Repertorium*, 1855, No. 2.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Antidote to Strychnia. (Extract of a letter from WM. NICK PINDELL, M. D., of St. Michael's, Talbot County, Maryland, to the Editor.—The object of this letter is merely to state facts, and through the Journal to have the facts made known, at the same time to obtain through your influence further experiments, either to satisfy or dispel from the minds of some that there is always at hand a "safe" and "sufficient" antidote to the poisonous "strychnia." Will you have the "antidote" fully tested?

I will now state the experiments of mine own, and the occasion of their having been made.

There were some three or four dogs, nightly frequenting my premises, committing devastations upon anything left exposed. They had continued to worry me until forbearance ceased to be a virtue, and sentence was pronounced: "They shall die." A piece of meat containing "1 grain of strychnia," was placed close beside a jar containing refuse "lard." I sat, watched a dog take, eat the meat, and commence upon the lard. My watch was beside me, and I expected the dog to die; five, ten, fifteen, twenty, thirty minutes passed; still, he did not die. That night, the "lard" having all been eaten, three pieces of meat, upon which the poison had been placed, were dropped in separate places. Next morning, I found three dogs dead. I, that day, tried dog No. 1 with two more grains, without the lard, and, in ten minutes, he was dead. There have been nine instances in which the poison was given, and antidote used; in neither one did the dog die. In eleven, without the lard, they all died. The half grain was sufficient to kill. Three grains failed when the antidote was used.

The test has been also used upon cats with the same result. A difference in time of death is made by simply putting it upon *fat* instead of *lean* meat; the latter being over some three minutes sooner than the former.

It is proper to state, that the lard was given in not less than "half pints" up to the one and a half.

What is the action of the lard?

Will you, doctor, have this matter tested fully, and then report through the Journal? The article of strychnia, which I have used, was obtained from my chemists in Baltimore, Coleman & Rogers. I do not know, or am unable to account for the action of the lard. I may now state that I have used the camphor, and "failed."

If it be necessary, it will afford me pleasure to report at length of the trials, and then have the public to know of the agent, if it should meet your wishes.

ST. MICHAEL'S, TALBOT CO., MARYLAND, July 12, 1855.

[We trust our correspondent will pursue his experiments, and communicate to us the results.]

A Case of Rupture of the Uterus, and Recovery. By WILLIAM W. DUVALL, M. D., of Prince George's County, Maryland.—June 8, 1854. I was

called to see —, who had been in labour twelve hours; but, for four hours previous to my visit, there had been an entire suspension of uterine effort. Upon examination, the shoulder was found presenting. Turning the child, delivery by the feet was resorted to and effected with but slight difficulty and delay—the child being dead. The uterus being passive the placenta was retained, and as there was considerable hemorrhage, its extraction was deemed necessary, which was done—it being detached from the uterus, and lying near its mouth. The hemorrhage not ceasing, or abating, so far as to render the patient's condition one of safety, it was thought advisable to introduce the hand to provoke contraction, and upon so doing, I perceived a transverse rent in the walls of the uterus, about three inches above the cervix, anteriorly, through which I could easily pass my index, middle, and ring-finger. The patient being much exhausted, a neat and efficient bandage was applied around the abdomen. She was enjoined to lie upon her back, and opiates and cordials were administered. The lochial flux was excessive for several days, followed by sero-sanguineous, and then purulent discharge, which continued for several weeks, accompanied by irritative fever and diarrhœa. The patient had borne three children previously, and the presumption is that the laceration occurred by the violent and unavailing efforts of the organ under the malpresentation, as there was but slight effort required in turning the child. Since her recovery, she has enjoyed good health, menstruating regularly—having lived *absque marito*.

Description of a New Pessary. By GEO. J. ZIEGLER, M. D.—Most pessaries are so defective as to be very inefficient; some are even positively injurious, and the best are still too imperfect to satisfactorily answer the desired indications.

The chief difficulties met with in their construction and employment are connected with their bulk, weight, form, component material, expense, introduction, adaptation, and removal, mechanical interference with the functions of the pelvic viscera, and their general tendency to the production of both functional and organic derangement.

With a view to obviate some of the principal of these, and to obtain more favourable results from their use, I have contrived the instrument, of which a description is here presented.

It is composed of two substances, viz: a metallic base or frame, and a shield or envelop of organic matter. The first consists of steel wire, though that of common iron, or other suitable metal, will probably answer sufficiently well. This is covered with gutta percha, the quantity of which, however, should be as small as is consistent with efficiency, so as not to unduly increase the bulk or weight of the instrument. In general form it resembles the so-called horse-shoe pessary of Dr. Hodge, though materially differing therefrom in some particular features. It is oblong, and so curved longitudinally as to be best adapted to the curve of the vaginal canal, the axes of the inferior and superior straits and the arc of the pelvic circle. In breadth it gradually diminishes from the base or pubic upwards to the uterine extremity, so as to lessen the lateral distension at the upper part of the vagina. The uterine extremity is slightly rounded. The pubic end is considerably curved inwards, downwards, and upwards on the same plane as the outer limbs, so as to form, when *in situ*, a bridge or opening for the urethra and neck of the bladder.

The size, curves, and thickness of component materials may be varied according to circumstances.

The whole is so simple in shape, material, and manufacture, as to be readily constructed by any one of ordinary ingenuity.

The theoretic value of the instrument, thus presented, rests upon the simplicity and economy of its component materials, facility of construction, general flexibility, small bulk, light weight, cheapness, peculiar shape, the ease with which it may be introduced and removed, its ready adaptation to the form of the parts it is designed to occupy and the mechanical changes incidental to the functional operations of the pelvic viscera, as well as those induced by the general movements of the body. Its practical value, however, must be determined by experience.

PHILADELPHIA, Feb. 23, 1855.

A Word in Defence of an American Surgeon. By HORATIO R. STORER, M. D., one of the Physicians to the Boston Lying-in Hospital.—The following correspondence may not be considered out of place. In the fact that the erroneous and offensive conclusions objected to, were based upon a page of this Journal, I trust I have reason for here discussing them:—

(Extracted from the *London Medical Times and Gazette* of May 19, 1855.)

To the Editor of the *Medical Times and Gazette*:—

SIR: I beg you will allow the insertion of the accompanying letter in the next number of your Journal. It will explain itself.

I select your Journal, from believing it the first in the profession; and am the more emboldened to ask the favour, because a similar injustice is so often done your countrymen by my own—a system of things I shall always condemn and strive to remedy.

I am, &c.

HORATIO R. STORER, M. D.

EDINBURGH, May 11, 1855.

In the *Edinburgh Medical and Surgical Journal* for January, 1855, in connection with a case therein quoted, certain expressions are used by one of its editors that may have struck others beside myself, as alike unnecessary, uncourteous, and unjustifiable: I trust you will allow me a few lines for answering them.

As a contrast to two instances of successful excision of joints, by Mr. Jones, of Jersey, and Dr. Keith, of Aberdeen, there is given the history of a case of extensive caries of the elbow-joint, in which amputation had been performed by Dr. J. Mason Warren, of Boston.

The patient had previously been under treatment for phthisis; all operation had at first been considered in consultation, unjustifiable, "on account of the affection of the lungs," and it was only from the reasons, that her general health was becoming completely undermined, and that "the patient herself strongly urged it," that amputation was finally performed—two months after the consultation alluded to—and with much relief to the more immediately distressing symptoms, for which alone it was undertaken.

To the history of this case, as copied from the *Edinburgh Journal*, was added the following comment, to every word of which, I do most strongly object: "We have given this case entire, not from any peculiarly interesting features it contains, but for the purpose of showing that a society, instituted for medical improvement, as it did not challenge the recorded malpractice, appears to be unacquainted with one of the most successful modern improvements in surgery, viz: "Resection of the elbow-joint, evidently the proper procedure in the case in question."

First, as regards the alleged "malpractice." Dr. Warren, it may be premised, is well known in this country, and is here considered, as he is at home, one of the first of American surgeons; he is certainly second to none. In Lon-

¹ Records of Boston Society for Medical Improvement, *American Journal of the Medical Sciences*, October, 1854.

don, he has many friends, all of whom will bear me out in this assertion, and in Edinburgh he is not wholly unknown. He was an early pupil of Mr. Syme, as also of Roux, in Paris, and might, therefore, *à priori*, be supposed not wholly "unacquainted" with the fact that joints can be excised. I may also mention, that such things have been done in Boston, and by Dr. Warren himself. As an old pupil of his, I happen to have seen him successfully perform resection more than once, and have seen several of his colleagues, who saw this case with him, and who coincided in his opinion, perform it also. To this last fact I shall again allude. Such being the case, we should be justified, I think, in supposing there was some good reason for the course pursued, even had there been stated nothing more than that amputation was performed.

But when the history of the case is given us in full, and with it, not merely by implication, but directly, the reasons for what was done, I think it is hardly justice to stigmatize that course, as "malpractice."

Would, however, any other course have been justifiable in this case? To answer this, it is not at all necessary to go into any long detail of the respective merits of amputation and resection. In many cases there can be no doubt of the infinite advantage of the latter; but, in this case, from what I know of its history, it would simply have been ridiculous. Here was a poor girl, far gone in consumption, with suppurating tubercles under both clavicles, totally unfit for any operation, considered as a curative. Even amputation was, at first, thought unjustifiable, and was only at last adopted at the patient's own urgent request, and merely to ease her of her irritating burthen, and permit her to pass in comparative comfort the few days left her on earth.

She had previously been seen, in consultation with Dr. Warren, by his colleagues, the other surgeons of the Massachusetts General Hospital, which, in America, is generally supposed to be one of the best institution of the kind in that country, officered by men competent to give an opinion. They are men, at any rate, to whom resection is no novelty, and when, with the patient before their eyes, these gentlemen had, at first, decided it best, in no way to interfere, for reasons already given, is it really fair for a critic, thousands of miles away, peremptorily to convict them all of gross ignorance of their profession, and to assert that "resection of the elbow-joint was evidently the proper procedure in the case in question?"

One word more and I have done. The Medical Society, to which the case was reported, and to which such unkind allusion has been made in the *Edinburgh Journal*, is composed only of the best men in Boston. It is a society in high standing, not in Massachusetts, or in America alone, but in this country, and on the continent. For many years, its records have been honoured by frequent reference in most foreign journals, and by many writers, and there are few things done here, that are not soon known, and discussed there. Were I a member of it, which I am not, I should write, with even more warmth; but I believe that, from what I have stated, you, Mr. Editor, and with you, the profession in this country, will bear me out in repelling this attack upon my teacher, his city, and his friends.

HORATIO R. STORER, of Boston.

46 CHARLOTTE SQUARE, EDINBURGH, May 11, 1855.

(Extracted from the *London Medical Times and Gazette* of June 2, 1855.)

To the Editor of the *Medical Times and Gazette*:—

SIR: In your last number, there is a very indignant letter from Dr. H. R. Storer, regarding a short criticism appended to a case of Scrofulous Disease of the Elbow-Joint, quoted in the *Edinburgh Medical and Surgical Journal* for January, 1855.

As I happen to be the author of the few lines so warmly animadverted on, and do not choose to shelter myself under the mantle of "one of the editors," to which I have no claim, I trust you will allow me a few explanatory remarks.

In Dr. Storer's letter to yourself, it would be seen that he considers the offending criticism especially directed against Dr. Warren, and the Boston Medi-

cal Society as Americans. Now, this I explicitly disclaim, and am quite ready to admit all that Dr. Storer has said in their praise; but that is not the question at issue.

When a case is published in a medical journal, it becomes, as it were, public property, and any person is, I conceive, entitled to point out what he may consider an error in practice. This, I have ventured to do in the present instance, and I am told my remarks are "*alike unnecessary, uncourteous, and unjustifiable.*"

1. First then, as regards their being "unnecessary."

When isolated cases are brought under the notice of a medical society, or published in a medical journal, they are expected to contain something of especial interest, either in their symptoms or treatment; otherwise the journal's would be overflowing, even more than they are at present, with details of no value whatever to any but the parties desirous of appearing in print. After perusing carefully Dr. Warren's cases, the only novelty I could find was his having recourse to amputation for scrofulous disease of the elbow: but resection of that joint for the disease in question, is now so firmly established in practice, that it constitutes the rule, amputation of the arm, the exception. If this be admitted (as by Dr. Storer's own showing we may expect Dr. Mason Warren and the Boston Medical Society to do), the reader of the case cannot but be struck by the total absence of any allusion whatever to the departure from the ordinary operation; and I appeal with confidence to every practical surgeon conversant with resection of the elbow-joint, to say whether there are any details in the case, *as recorded in the Journal*, sufficient to contraindicate that operation.

The majority of medical men have not the advantages possessed by metropolitans, and are, therefore, apt to be guided in unusual cases by what they glean from their more fortunate brethren. I would, therefore, humbly suggest that the fact of so eminent a surgeon as Dr. Mason Warren, recording a case of amputation of the arm for scrofulous elbow-joint, without one word of explanation as to his departure from the usual course, is apt to mislead, and likely to induce others to reject the conservative method on too trivial grounds. My remarks, therefore, were not "*unnecessary.*"

2. They are styled "*uncourteous.*"

Having already disclaimed any intention of being disrespectful to our transatlantic brethren, I trust I may be acquitted of discourtesy; but I may remark that Dr. Storer has applied a stronger interpretation to the term "*malpractice*" than I intended it to convey, and I hasten to disabuse his mind, and that of any other who may have given it the same meaning, and, by doing so, making the only reparation in my power. I did not calculate on its being interpreted as a charge of very culpable and gross misconduct on the part of Dr. Warren, but merely on its representing what I conceived to be bad practice, an error in practice, or, in other words, injudicious. Still less did I mean absolutely to assert that the Boston Medical Society ("*composed only of the best men in Boston*") are unacquainted with resection of the elbow-joint, for the sense of the passage must be much distorted ere it can admit of such an interpretation.

3. My criticism is said to be "*unjustifiable.*"

Having during the last fourteen years witnessed, with but one or two exceptions, every case of resection of the elbow-joint occurring at the Royal Infirmary, Edinburgh, where it is so frequently practised, myself a former pupil and house surgeon of Mr. Syme, one of its warmest and most able advocates, and now one of his colleagues at the Surgical Hospital, I should at all events know something of the operation; and no one will, I think, question the accuracy of the statement, when I assert that a large majority of the cases requiring resection are of a scrofulous habit, and not a few of them have suspicious indications of pulmonary disease of a phthisical character. When this disease is in the quiescent state, but the patient likely to succumb to irritative fever, it is considered the proper course to combat the more immediate danger by resection of the joint, notwithstanding the threatened phthisical tendency; and many of these cases subsequently do well.

Dr. Storer says that in the details of the case, as given in the *Boston Journal*,

there is sufficient evidence against the propriety of resection. This I leave for competent judges to decide, merely, for my own part, adopting the language of Shylock:—

“I cannot find it: ’tis not in the bond.”

Had I possessed the private means of information enjoyed by Dr. Storer, I would *most certainly* not have suggested *resection*, though I would as decidedly have been disposed to challenge the propriety of *amputation*; but I was unacquainted with the fact that “*a difficulty in the lungs*” was synonym for “*suppurating tubercles under both clavicles*.”

But I am afraid Dr. Mason Warren will be disposed to cry, “*save me from my friends!*”—for Dr. Storer, while endeavouring to show the impropriety and “ridiculous” proposal of resection, has surely proved too much. He tells us the patient had only a few days to live on earth, owing to these suppurating tubercles under both clavicles, and that the amputation was undertaken at the patient’s urgent request, in order that she might pass these few days in comparative comfort.

I suspect few of your readers are prepared to admit, when a patient is evidently dying of acute phthisis, and has only a few days to live, that any capital operation whatever is justifiable, even though it should be at the urgent request of the patient. In the same number of your *Journal* we are told that Luigi Buranelli tried to persuade a friend to shoot him; but the friend did not feel himself called on to *shorten his days* by complying with his request.

I fear I have trespassed too much on your valuable columns, but I could not answer in shorter space the various charges brought against me.

I am, &c.,

JAMES D. GILLESPIE, M. D., F. R. C. S. E.,
Assistant Surgeon, Royal Infirmary, Edinburgh.

30 YORK PLACE, EDINBURGH.

Owing to long absence in the country, Dr. Gillespie’s answer to my letter of last May has but just reached me. In part it is satisfactory, in part not. It removes unpleasant doubts, and takes back offensive language—but it still defends so peculiar a form of criticism, and opens up such important questions in surgery, from this letter seemingly answered so differently on the two sides of the Atlantic, that I feel compelled, though against my will, to rejoin.

On these points, my friend has taken very strong ground. I as decidedly differ from him—and, with him, I most earnestly appeal to the profession to decide between us. In practical surgery there can be but one character to a man’s course—it must be either right or wrong—and, implicated as I now am in this matter, I naturally am very anxious to learn which of these opinions is correct.

But to return for a moment to part of my former letter, and to Dr. Gillespie’s answer.

I could, indeed, hardly believe that the severity of his criticism became greater because upon Americans, my own experience, of his uniform courtesy and manliness were so at variance with this. It being, however, no unusual occurrence for such attacks to be made in every country upon foreigners, and those from whom distance, as here, or difference in language, renders an answer somewhat improbable, Dr. Gillespie must pardon my having made any such allusion. I am glad that, in this respect, he differs from so many critics.

In recanting the charge against Dr. Warren, some surprise is expressed at my having found fault with the word *malpractice*. Dr. Gillespie “did not calculate on its being interpreted as a charge of very culpable and gross misconduct on the part of Dr. Warren;” but such meaning is generally borne by the word in this country, and, unless I greatly err, in Great Britain also.

And so with the charge against the Medical Improvement Society: Dr. Gillespie at first asserts that he quoted Dr. Warren's case expressly "for the purpose of showing that this society * * * appears to be unacquainted with * * * resection of the elbow-joint"—but now, mark the change! thinks that "the sense of the passage must be much distorted, ere it can admit of such an interpretation." I am afraid we can only apply these, his own words, to his effort at explanation.

It is now asserted that this case of Dr. Warren's should never have been reported at all; on the one hand, as containing no unusual or important feature, and, on the other, as being an instance of decided error in treatment; and, in addition to this, we are told that, as reported, it was calculated to mislead—on the one hand, a country surgeon to equally "bad practice," and, on the other, a reviewer to severer criticism. But Dr. Gillespie has evidently forgotten two rather important features of the case—that the trouble in the lungs¹ was distinctly stated to be "tuberculous," and that the case was not reported in this journal, from which Dr. G. quoted it, or in any other, by Dr. Warren. It was merely related to a medical Society—was there discussed at length; but, as is usual at such times, its mere outlines were recorded by the secretary. In this state, and, as an "extract from those records," it was printed, and under this very title it was quoted in Edinburgh.

Dr. Warren had, however, what seemed sufficient grounds for relating the case. Upon this point, I shall merely give his own words:¹—

"The case recorded was of considerable local interest. It had been under the care of one or two of the physicians and three of the surgeons of the hospital, and many members of the society, now present, were interested in it. In reporting the case, it was, therefore, thought unnecessary to go into any great detail before the society, as the particulars were so well known to many of them. It might be added that, even if the patient had been perfectly healthy in other respects, excision in this case would have been entirely out of the question, for the following reasons: As stated in the report, the arm hung by the side of the body perfectly useless; having, in a single night, fallen from a flexed position almost paralyzed. The limb, just above the elbow, was extremely attenuated, being not much larger than a common broom-handle; below, the elbow expanded into a large tumour, covered by very delicate and diseased integument. The muscular texture above and below the joint, as was obvious in the dissection, had degenerated."

Dr. Gillespie, after being informed of the true circumstances of the case, does not hesitate to challenge the propriety of the amputation; conceiving that, at no time, in a truly desperate case, and as a mere palliative of distressing and plainly harmful symptoms, "is any capital operation whatever justifiable;" and, to prove his point, gives an instance in no way pertinent.

The two questions here open are well worthy the attention of the profession, and I ask its decision upon them. My own opinion has already been strongly expressed; and I know that, in this country, at least, there are many who think with me. No man has a right to add to a patient's risk merely at that patient's request, if that happen to be a foolish one; but, where to this are joined reasons such as Dr. Warren has given, justification becomes a different matter. Several years since, the proposal of inducing anæsthesia upon certain death-beds was bitterly attacked; now, few can be found to question its propriety.

¹ *Boston Medical and Surgical Journal*, July, 1855, p. 461. For other remarks upon the case, the reader is referred to the same pages.

To this, I need merely add, that previously to writing one word upon this subject, I showed the article in the *Edinburgh Journal* to my friend Mr. Syme, in whose opinion, with Dr. Gillespie, we all find *the law*. He read the case and the comments of his colleague, of whose identity with the critic we neither of us had, at that time, any suspicion. To my question as to what course he himself would have pursued, this reply was given: "I should never, in this case, have performed resection; amputation was the thing."

7 CHESTER STREET, BOSTON, Sept. 13, 1855.

DOMESTIC SUMMARY.

Fracture of the Neck of the Os Brachii, complicated with Luxation of the Head of the Bone in the Axilla, treated successfully by Immediate Extension and Direct Manipulation over the Luxated Fragment.—Dr. JOHN WATSON, Surgeon to the New York Hospital, relates (*New York Medical Times*, July, 1854) two cases of this rare accident which have occurred in his practice, with the object of demonstrating the advantages of attempting the immediate reduction of the dislocation by extension of the arm and direct pressure upon the displaced head of the os brachii, over the plan of treatment laid down by Chelius and others; and to show that the let-alone system spoken of by Sir Astley Cooper and Mr. Key, is not always the best.

CASE I. December 29, 1851.—Henry Ringe, a coal-carman, fell from his loaded cart, and suffered a severe injury by the passage of the wheel over his left shoulder. I saw him at the office of Dr. Vanderpool soon after the accident, and detected the head of the os brachii in the axilla, and at the same time a fracture of this bone just below the tubercles, or midway between its upper extremity and the point of insertion of the deltoid muscle. After a careful examination, and before the patient had fairly rallied from the shock of the accident, I placed him flat upon his back on a sofa, with his injured arm drawn over a depression in the back of the sofa, which I had previously padded with a pillow to serve as a point of resistance and support for the axilla during the process of extending the arm. With the patient thus placed, I had firm and steady traction made by the hands of two assistants drawing upon the arm in a line of about 45° with that of the patient's body. The tension thus exerted upon the limb had the effect of putting the muscles about the shoulder in their normal relation with the shaft of the bone; and the broad fulcrum offered by the padded notch in the back of the sofa, served in some measure, during the process of extension, to push the head of the bone inwards from the axilla towards its natural position. In this way, with very little trouble, and with no severe amount of traction, the deformity at the joint was righted. The fracture was immediately afterwards reduced, and the limb adjusted with a pad in the axilla, and with splints and bandages, as in ordinary simple fracture of the os humeri. The case gave rise to no further trouble. My last minute upon it was on the 1st of March, 1852, at which date the fracture had united, and the joint was in its natural condition. The next case is one of recent occurrence.

CASE II.—Allen B. Nelson, shoemaker, a native of New York, aged 51, received a severe blow upon his right shoulder from a locomotive engine while in motion, and was brought to the New York Hospital soon after the accident, still in a state of prostration from the shock, Saturday, April 21, 1855. On the day of admission, owing to the patient's acute suffering, it was not deemed advisable to attempt any minute examination of the shoulder, which was already much swollen, and only such treatment was instituted as to favour re-
action and allay pain.

On Monday morning, April 23, after etherizing the patient, I made a thorough exploration of the injury. There was a small laceration of the integu-

ment in front of the shoulder, some two inches or more below the top of the acromion. The whole shoulder was greatly tumefied, the arm was quite movable, and could be drawn close down against the trunk. By tracing its long axis upwards, the shaft of the os humeri appeared to range an inch and a half, or more than this, within its proper line of relation to the glenoid cavity. The deltoid muscle was bellied out above, and wrinkled and contracted near its point of insertion; the lower edge of the pectoralis major near its insertion into the anterior lip of the bicipital groove, was in like manner contracted and corrugated. The tumefaction about the joint assumed a globular shape, leaving no sharp prominence or depression near the acromion. But the finger could be readily insinuated deeply beneath this process, showing that the head of the bone had escaped from its socket; and, on searching, it was found lying in the axilla. Rotation and traction upon the arm produced an occasional sound of crepitation. But the movements of the arm were not communicated to the head of the bone. As far as could be determined, the seat of fracture was as high up as the insertion of the teres major muscle, or near the head of the tubercles.

Slipping a strong roller towel under the axilla, and securing the upper extremity of this over the bed-head for counter-extension, I had firm traction exerted on the arm by the hands of assistants, and varied the mode of extension by sometimes trusting to simple traction, and sometimes placing a pad in the axilla, and pressing upon this with the heel of an assistant; in the mean while insinuating my fingers around the dislocated head of the bone, I attempted by direct manipulation, to force this outwards and upwards towards the glenoid cavity. So long as the traction was made at only a small angle from the line of the body, these efforts were unavailing. But by drawing the arm outwards nearly into a right angle with the trunk, and then repeating these manœuvres with the fingers over the head of the bone, this latter slipped readily into its proper place. After thus reducing the head of the bone, the broken fragments were brought into proper coaptation, and then secured with splints and bandages, with a firm broad pad in the axilla to prevent the bone from again forsaking the glenoid cavity, and with the elbow bent and the arm fixed to the side as in ordinary fracture. The splints were also so arranged as to allow the small lacerated wound in front of the shoulder to be dressed without disturbing them. The dressings were removed, and the shoulder carefully examined, for the first time after their permanent adjustment, on the 31st of April, at which time the tumefaction had greatly subsided, and the parts were in their natural relations. The bandages were again removed on the 5th of May for a second examination. No deformity was discernible, and the small sore was in process of cicatrization. May 15, the union of the fracture was firm, and the small ulcer nearly closed. May 31, the patient was discharged cured.

The successful issue of this latter case may, I think, be fairly attributed to the direct manipulation with the hand over the broken and luxated head of the os brachii. In a former communication, I had occasion to speak of the case with which dislocation of the head of the os brachii may be occasionally reduced where direct pressure can be exerted upon it. The instance referred to was one in which, through a wound in the integuments of the axilla, I succeeded in replacing the head of the bone by simply seizing it and forcing it inward towards its socket with my thumb and finger.

Dislocation of the Femur into the Ischiatic Notch—Reduction by Manipulation. By FRANK H. HAMILTON, M. D.—March 23, 1855. Charles McCormick, aged 21 years, at work for the "State Line R. R. Co.," was caught between two freight cars, with his back resting against one and his right knee against the other, his thigh being raised to a right angle with his body. As the cars came together, he felt a "cracking" at his hip-joint, and was immediately unable to walk or stand.

Two hours after, I saw McCormick, and, assisted by my son Theodore, and Austin Flint, Jr., I examined the limb, and made arrangements for its reduction. The patient was lying upon his back and left side. His right thigh was

flexed upon his body to nearly a right angle, and adducted, the knee being carried across the opposite thigh. It was also rotated inward, but not forcibly.

Turning the lad upon his back, and raising the left leg to a position corresponding to the right, both legs were carefully measured with a tape line from the anterior superior spinous process to the patella, and the right leg was found to be shortened one and a half inch. Measuring again from the anterior superior spinous process to the most prominent point of the trochanter major, the distance on the dislocated limb was six inches, and on the sound limb five inches. The head of the bone could not be felt, but no doubt remained as to its position. The limb was nearly immovable, except in one direction. It could neither be abducted, nor rotated outward, nor carried downward.

Procedure.—The patient lying upon his back, I seized the right leg and thigh with my hands, the leg being moderately flexed upon the thigh, and carried the knee slowly upwards towards the belly, until it had approached within twelve or fifteen inches, when, noticing a slight resistance to further progress in this direction, I carried the knee across the body outward until I again encountered a slight resistance, and immediately I began to allow the limb to descend. At this moment a sudden slip or snap occurred near the joint, and I supposed reduction was accomplished; but, on bringing the limb down completely, I found it was in the same position as before. I think the head had slipped off from the lower lip of the acetabulum, after having been gradually lifted upon it.

Without waiting, I commenced to repeat the manipulation, and in precisely the same manner. Again, at the same point, when the limb was just beginning to descend, a much more distinct sensation of slipping was felt, and, on dropping the limb, it was found to be in place and in form, with all its mobility completely restored.

No anæsthetic was employed, and no person supported the body or interfered in any way to assist in the reduction. No outcry was made by the patient, yet he informs me that moving of the limb hurt him considerably. The amount of force employed by myself was just sufficient to lift the limb, and the time occupied in the whole procedure was only a few seconds.

After the reduction, he remained upon his back, in bed, eleven days, in pursuance of my instructions. At the end of this time he began to walk about, but was unable to resume work until after eight weeks or more. It is probable that he could have walked immediately after the reduction, without much, if any, inconvenience, so slight was the inflammation which resulted from the accident. He never complained of pain, but, upon interrogation, he replied that there was a slight soreness back of the trochanter, near the head of the bone. This soreness continued several weeks, and was especially present when he bent forward. Even at the present time, four months after the accident, he occasionally feels a pain at this point when he is stooping. The motions of the joint are, however, free, and he walks nimbly and without any halt.

In short, if I may judge correctly from a single example, nothing could be more complete than the triumph of this process over a dislocation hitherto so formidable. Nothing could be more simple and easy of execution, and nothing more gratifying both to the surgeon and to his patient. Unless, therefore, experience shall demonstrate in its practical working defects or dangers which I cannot now anticipate, I shall regard it hereafter as one of the most valuable contributions to our art, and its inventor as a true public benefactor.—*Buffalo Medical Journal*, Sept. 1855.

Accidental Impalement.—Dr. B. F. CHAPMAN relates (*Southern Medical and Surgical Journal*, Sept. 1855) an interesting example of this. The subject of it was a boy, nineteen years of age, and weighing 160 pounds, who, in descending from a tree, fell on a hoe-handle about an inch and a quarter in diameter, which entered the anus, and penetrated for at least twelve inches, without any visible lesion except the laceration of a small portion of the integuments. He suffered greatly from pain in the epigastrium, with incessant vomitings. A copious bleeding relieved these. High fever supervened; but, under an antiphlogistic regimen and general treatment, he recovered so as to be able to resume labour in two weeks.

Liquid Caoutchouc as a covering to the Skin in certain Lesions.—Dr. J. B. D. STILLMAN recommends (*New York Journ. Med.*, Sept. 1855) the liquid caoutchouc as preferable to collodion as an impermeable covering to the skin. About a year since, he says, he was presented with a bottle of the milk of the Hevea, in its liquid form as it exudes from the tree. "It is preserved in that state by the addition of a small proportion of free ammonia, and is now introduced as an article of commerce for manufacturing purposes, and, from my experience with it, I am impelled to call the attention of the profession to it, as a most useful contribution to our materia medica. It is of the colour and consistency of pure milk (if my recollections do not deceive me), but becomes transparent as soon as dry. Owing to its great elasticity, it does not contract so violently as the collodion, it adheres closely to the skin, and allows entire freedom of motion and application to any extent. In burns it has an advantage over anything I have ever used, as also in erysipelas. An acquaintance with it by surgeons will lead, I do not doubt, to many valuable improvements in surgical appliances."

OBITUARY RECORD.—Died, at the City of Washington, D. C., on Sunday, August 12, 1855, HENRY LEE HEISKELL, M. D., Surgeon U. S. Army.

Dr. Heiskell was born in Winchester, Va., March 16, 1803; graduated in medicine at the University of Pennsylvania, in 1828; was appointed an assistant surgeon in the army on the 13th of July, 1832, and on the 7th of July, 1838, promoted to the rank of surgeon. After serving for several years at southern stations, and sharing in the dangers and hardships of the Florida war, he was ordered to Madison Barracks, Sacketts Harbor, N. Y., from whence, in September, 1840, he was called to the City of Washington, and assigned to duty in the Medical Bureau, as assistant to the Surgeon-General.

In this position he remained until his death, a period of fifteen years, having in that time repeatedly received the appointment of Acting Surgeon-General, and discharged all the duties of that office during the absence of the Surgeon-General on official business, and particularly while that officer was at the seat of war in Mexico.

To the performance of the highly responsible, and oftentimes delicate, duties devolving upon him, Surgeon Heiskell brought a clear, comprehensive, and sound judgment, and an elevated sense of honour and of justice, which won and secured for him the esteem and confidence of the Surgeon-General.

As the confidential assistant of the Surgeon-General, his recommendations, in all matters relating to the movements of the medical officers, were governed by what he deemed the true interests of the service, and the relative rights of the officers concerned. In business pertaining to the monetary transactions of the Medical and Hospital Department, his decisions were strictly in accordance with the laws and regulations; but in cases where these conflicted with the equitable claims of individuals, he was ever ready to present the facts to those having authority to extend relief.

Governed by fixed principles, and guided by laws and regulations, his recommendations and decisions at times necessarily conflicted with the wishes and interests of individuals; but the reasonableness of the former, and the impartial justice of the latter, as well as the ability with which, when necessary, they were argued and maintained, gave him not only great influence with his associates in the army, but also created for him an enviable reputation in all the administrative departments of the government with which he had official business.

The elevated principles which characterized him in his public career marked his conduct in all the relations of private life. Naturally of a quick and ardent temperament, he was prompt to resent an injury or an insult, while, at the same time, his nice sense of honour and gentlemanly feeling served as a sufficient barrier to prevent acts of aggression on his part. His habits were eminently social, and many who read these lines will recall his liberal hospitality.

In June, 1842, he married Elizabeth K. Gouverneur, the daughter of Samuel L. Gouverneur, and granddaughter of President Monroe, who, with four children, survives him.

During the last three years of his life, Dr. Heiskell's failing health served to

withdraw him, to a certain extent, from the busy cares of office. Urged by the promptings and example of his wife, and guided by a higher power, he added to his many virtues those brighter excellencies which adorn the character of the Christian. Having publicly dedicated himself to Christ, he knew no looking back, but, with that decision and directness of purpose which marked his conduct in worldly affairs, he "pressed toward the mark for the prize of the high calling of God in Christ Jesus." Under the mild but powerful influences of religion, the asperities which served in some degree to mar the symmetry and beauty of his moral character were softened or removed. He bore the sufferings and trials of a long-protracted and painful illness with patience and resignation, and, with firm reliance upon the promises of the Saviour, he awaited his final orders as became a true soldier of the cross.

WORKS PREPARING FOR PUBLICATION.

Messrs. Blanchard & Lea have in press and will shortly publish the following valuable works:—

Physiological Chemistry. By Prof. C. G. LEHMANN. Translated by G. E. DAX, M. D., and edited by R. E. ROGERS, M. D., Professor of Chemistry in the University of Pennsylvania. Complete in two large and handsome octavo volumes, with numerous illustrations. (*Early in October.*)

The Practical Anatomist; or, the Student's Guide in the Dissecting-Room. By J. M. ALLEN, M. D., Professor of Anatomy in the Pennsylvania Medical College. In one very handsome royal 12mo. volume of 500 pages, with over 200 illustrations. (*Early in October.*)

Introduction to Practical Pharmacy; intended as a Text-Book for the Student, and Guide to the Physician and Pharmacist. With numerous formulæ, and over two hundred illustrations. By EDWARD PARRISH, Lecturer on Practical Pharmacy and Materia Medica in the Philadelphia Academy of Medicine. In one handsome octavo volume of about 400 pages. (*Early in October.*)

The Principles and Practice of Physical Exploration applied to the Diagnosis of Diseases affecting the Organs of Respiration. By AUSTIN FLINT, M. D., Professor of the Theory and Practice of Medicine in the University of Louisville. In one handsome octavo volume. (*In November.*)

A Manual of Chemical Physiology. From the German of C. G. LEHMANN. Translated, with copious Notes and Additions, by J. CHESTON MORRIS, M. D. With an Introduction by SAMUEL JACKSON, M. D., Professor of Institutes of Medicine in the University of Pennsylvania. In one handsome octavo volume. (*In November.*)

Atlas of Diseases of the Skin. By J. MOORE NELIGAN, M. D. In one very handsome quarto volume, with numerous elaborately coloured plates. (*In November.*)

The Microscope and its Revelations. By WILLIAM B. CARPENTER, M. D. In one octavo volume, with several hundred beautiful illustrations. (*In November.*)

A Manual of the Principles and Practice of Medicine. By GEORGE H. BARLOW, M. D., Physician to Guy's Hospital, London. With Additions by the American Editor. In one neat octavo volume. (*In November.*)

Synopsis of the Course of Lectures on Materia Medica and Pharmacy, delivered in the University of Pennsylvania. By Prof. J. CARSON, M. D. Second edition, revised. In one neat octavo volume. (*In October.*)

Medical Anatomy. By FRANCIS SIBSON, M. D., Physician to St. Mary's Hospital. In handsome folio, with splendid coloured plates.

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PENNSYLVANIA COLLEGE—MEDICAL DEPARTMENT.

SESSION OF 1855-'56.

The regular Course of Lectures will commence on Monday, October 8, and will be continued until the first of March.

FACULTY.

DAVID GILBERT, M. D.,	Professor of Obstetrics, and Diseases of Women and Children.
ALFRED STILLÉ, M. D.,	Professor of Theory and Practice of Medicine.
JOHN NEILL, M. D.,	Professor of Surgery.
J. M. ALLEN, M. D.,	Professor of Anatomy.
JOHN J. REESE, M. D.,	Professor of Medical Chemistry.
JOHN B. BIDDLE, M. D.,	Professor of Therapeutics and Materia Medica.
FRANCIS G. SMITH, M.D.,	Professor of Institutes of Medicine.
JOSEPH SHIPPEN, M. D.,	Demonstrator of Anatomy.

Clinical Instruction will be given by Professors BIDDLE and NEILL at the *Philadelphia Hospital, Blockley*, during the entire term of the session, in conjunction with other members of the Medical Board of the Hospital. The Students of Pennsylvania College—*both first course and second course*—will be furnished *gratuitously* with the ticket to the Philadelphia Hospital. Second-course students have the option of receiving *gratuitously* the ticket to the Pennsylvania Hospital. A Clinic will also be held at the College every Wednesday and Saturday morning throughout the session.

F E E S.

For the entire Course of Lectures	\$105 00
Matriculation (paid once only)	5 00
Graduation	30 00

The Dissecting-Rooms will be opened in September, under the direction of the Professor of Anatomy and the Demonstrator.

Preliminary Lectures will be delivered during the fortnight preceding the opening of the Session.

JOHN J. REESE, M. D., *Registrar*,
No. 122 South Ninth Street, Philadelphia.

MEDICAL COLLEGE OF THE STATE OF SOUTH CAROLINA.

The annual Course of Lectures in this Institution will commence on the first Monday in November, on the following branches:—

Anatomy by	J. E. HOLBROOK, M. D.
Surgery by	E. GEDDINGS, M. D.
Institutes and Practice of Medicine by	S. HENRY DICKSON, M. D., LL. D.
Physiology by	JAMES MOULTRIE, M. D.
Materia Medica by	HENRY R. FROST, M. D.
Obstetrics by	THOS. G. PRIDLEAU, M. D.
Chemistry by	C. U. SHEPARD, M. D.
Demonstrator of Anatomy	F. T. MILES, M. D.
Prosector of the Professor of Surgery	T. F. M. GEDDINGS, M. D.

CLINICAL INSTRUCTION.

D. T. Cain, M. D., Physician to the Marine Hospital and Clinical Instructor, lectures twice a week on the diseases of that Institution.

H. W. Desaussure, M. D., Physician to the Hospital of the Almshouse, at which Lectures are delivered twice a week on Diseases, the diagnosis discriminated, and the student indoctrinated in their treatment.

The anatomical rooms are opened the latter part of October, and the dissections conducted daily under the direction of the Demonstrator. Much attention is directed to this department; the material being abundant, and illustrations of various character being afforded for acquiring a competent knowledge of this all-important branch of study.

HENRY R. FROST, M. D., *Dean*.

June, 1855.

PHILADELPHIA COLLEGE OF MEDICINE.

FIFTH STREET, BELOW WALNUT.

The WINTER SESSION will commence early in October, 1855, and continue until March, 1856.

The SPRING SESSION will commence in March, and terminate in July.

DEGREES are conferred in MARCH and JULY.

FACULTY.

GEORGE HEWSTON, M. D.,	Professor of Anatomy.
B. HOWARD RAND, M. D.,	“ Medical Chemistry.
HENRY HARTSHORNE, M. D.,	“ Institutes of Medicine.
JAMES L. TYSON, M. D.,	“ Materia Medica.
ISAAC A. PENNYPACKER, M. D.,	“ Theory and Practice of Medicine.
JAMES BRYAN, M. D.,	“ Surgery.
LEWIS D. HARLOW, M. D.,	“ Midwifery and Diseases of Women and Children.

JOSEPH PARRISH, M. D., Emeritus Professor of Midwifery and Diseases of Women and Children.

FEES: For one full Course, \$84; Perpetual ticket, \$150; Matriculation, \$5; Practical Anatomy, \$10; Graduation, 30.

Examinations are given daily by the Professors upon each branch. Second-course students are furnished gratuitously with the ticket of the Philadelphia Hospital for one year; and such as are sufficiently advanced have the opportunity of attending out-door patients in connection with the clinique under care of the Professors.

For further information, apply to

B. HOWARD RAND, M. D., DEAN,
At the College, Fifth Street below Walnut.

UNIVERSITY OF NASHVILLE.

MEDICAL DEPARTMENT.

The Fifth Annual Course of Lectures in this Institution will commence on Monday, the 29th of October next, and continue till the first of the ensuing March.

PAUL F. EVE, M. D.,	Principles and Practice of Surgery.
JOHN M. WATSON, M. D.,	Obstetrics, and the Diseases of Women and Children.
W. K. BOWLING, M. D.,	Theory and Practice of Medicine.
A. H. BUCHANAN, M. D.,	Surgical and Pathological Anatomy.
C. K. WINSTON, M. D.,	Materia Medica and Medical Jurisprudence.
ROBERT M. PORTER, M. D.,	General and Special Anatomy.
J. BERRIEN LINDSLEY, M. D.,	Chemistry and Pharmacy.
THOMAS R. JENNINGS, M. D.,	Institutes of Medicine and Clinical Medicine.

WILLIAM T. BRIGGS, M. D., Demonstrator of Anatomy.

The Anatomical rooms will be opened for Students, on the first Monday of October. A full *Preliminary Course* of Lectures, free to all Students, will be given by the Professors, commencing also on the first Monday of October.

A Clinique has been established, in connection with the University, at which operations are performed and cases prescribed for and lectured upon in presence of the Class.

Amount of fees for lectures in the University is . . .	\$105
Matriculating fee (paid once only),	5
Practical Anatomy,	10
Graduating fee,	25

Good boarding can be obtained at about \$3 00 per week. Further information can be obtained by addressing

NASHVILLE, TENN., July, 1855.

J. BERRIEN LINDSLEY,
Chancellor of the University.

SUMMER AND WINTER COURSE OF LECTURES
AT THE
NEW YORK MEDICAL COLLEGE.
(*East Thirteenth Street, between Third and Fourth Avenues.*)

- HORACE GREEN, M.D., LL.D., Emeritus Professor of Theory and Practice of Medicine, and Professor of Diseases of the Respiratory Organs.
E. H. DAVIS, M.D., Professor of Materia Medica and Therapeutics.
B. FORDYCE BARKER, M.D., Professor of Midwifery and Diseases of Women and Children.
R. OGDEN DOREMUS, M.D., Professor of Chemistry and Medical Jurisprudence.
J. M. CARNOCHAN, M.D., Professor of the Principles and Operations of Surgery, with Surgical Pathology.
EDMUND R. PEASLEE, M.D., Professor of Physiology and Pathology.
EDWARD H. PARKER, M.D., Professor of General, Descriptive, and Microscopic Anatomy.
HENRY G. COX, M.D., Professor of Theory and Practice of Medicine, and of Clinical Medicine.
D. S. CONANT, M.D., Demonstrator of Anatomy.

The Summer Course of Lectures will commence on June 12, 1855, and will continue until October 12, when the Winter Course will be resumed. The Summer Course will be entirely Practical and Clinical in its character. Lectures will be delivered daily at the College or at the Hospital, by the following gentlemen:—

- On Therapeutics, by Prof. DAVIS.
On Puerperal Diseases, by Prof. BARKER.
On Practical Chemistry, by Prof. DOREMUS.
On the Surgical Operations of the Eye, by Prof. CARNOCHAN.
On the Microscope as applied to the Diagnosis of Disease, by Prof. PARKER.
On Diseases of Children, by Prof. COX.
On Auscultation and Percussion, by J. HANCOCK DOUGLASS, M. D.
On Medical Jurisprudence, by DAVID UHL, M. D.

Prof. CARNOCHAN, Surgeon-in-Chief, and Prof. COX, Physician-in-Chief to the Emigrants' Hospital, which contains about 1,500 beds, will visit the Hospital with the Students, and Clinical Lectures will be delivered there by them. Students of this College can also attend at the Bellevue Hospital, Blackwell's Island Hospitals, and the New York Hospital. At the College three Cliniques are held weekly—Surgical Clinique by Prof. CARNOCHAN, Medical Clinique by Profs. DAVIS and PARKER, and Clinique for Diseases of Women by Prof. BARKER. At the latter, every variety of Uterine Disease is exhibited to the class. Midwifery cases will be furnished to the Students to attend at the houses of the patients.

The Class will be divided into sections to attend at the office of Prof. GREEN, where they will have an unequalled opportunity to witness the treatment of the Diseases of the Respiratory Organs.

It will thus be seen that unsurpassed clinical opportunities are afforded for Students of this College.

Physical Diagnosis as applied to both Medical and Surgical Diseases, will receive especial attention during the Summer Course.

Students are especially urged to avail themselves of the gratuitous instruction in Chemical Analysis, which will commence three weeks in advance of the Winter Course; opportunities for this practical knowledge, it is believed, are not afforded in any other Institution.

Dissections will be commenced as early as practicable, and an abundant supply of material will be furnished at the lowest rates.

T E R M S .

For Summer and Winter Course, inclusive	-	-	-	-	\$105 00
For Winter Course alone	-	-	-	-	105 00
For Summer Course alone	-	-	-	-	25 00

If Students attending the Summer Course afterward decide to attend the Winter Course, the \$25 00 paid will be placed to their credit on taking out the tickets for the Winter Course. Matriculation Fee, \$5 00.

Letters may be addressed to any member of the Faculty, or to
R. OGDEN DOREMUS, M. D.,
Dean of the Faculty.

NEW YORK MEDICAL COLLEGE, May 1, 1855.

MASSACHUSETTS MEDICAL SOCIETY.

PRIZE QUESTION.

Through the liberality of one of its Fellows, the Massachusetts Medical Society is authorized to offer the sum of *One Hundred Dollars* to the author of a Dissertation, which may be adjudged worthy of a prize by a Committee appointed by the Councilors of the Society, on the following subject, viz:—

"The History and Statistics of Ovariotomy, and under what circumstances the operation may be regarded safe and expedient."

Dissertations for the Prize must be sent, post-paid, to the Corresponding Secretary, Dr. C. E. WARE, 6 Temple Place, Boston, on or before the *First Wednesday* of April, 1856.

Each Dissertation must have written upon it some device or sentence, and be accompanied by a sealed packet on which is written the same device or sentence, and within which is inclosed the author's name and residence.

The packet accompanying the successful Dissertation will be broken, in open meeting, at the annual meeting of the Society, in May, 1856, and the author's name announced. Such portions of the Dissertation as the Society may desire, may then be read, and the whole may be printed in the Society's Communications.

All unsuccessful Dissertations will be deposited with the Corresponding Secretary, from whom they may be obtained, with the sealed packet unopened, if called for within one year after the result shall have been declared.

The Prize is open to the profession throughout the country.

By order of the Massachusetts Medical Society.

ROXBURY, MASS., July 31, 1855.

B. E. COTTING, *Rec. Sec.*

TRANSYLVANIA UNIVERSITY—MEDICAL DEPARTMENT.

THE 39th Session will commence on the 1st Monday in November next, and continue for four months under the following Faculty, viz:—

BENJAMIN W. DUDLEY, M. D., Emeritus Prof. of Surgery.

ROBERT PETER, M. D., Professor of Chemistry and Pharmacy, and Dean of Faculty.

JAMES M. BUSH, M. D., Professor of Anatomy.

SAMUEL M. LETCHER, M. D., Professor of Obstetrics and Diseases of Women and Children.

HENRY M. SKILLMAN, M. D., Professor of General and Pathological Anatomy and Physiology.

ETHELBERT L. DUDLEY, M. D., Professor of Surgery.

WILLIAM S. CHIPLEY, M. D., Professor of Theory and Practice of Medicine.

BENJAMIN P. DRAKE, M. D., Professor of Materia Medica and Therapeutics, and Medical Jurisprudence.

JAMES M. BRUCE, M. D., Demonstrator of Anatomy.

Cost for the full course, \$105; Matriculation and library fee, \$5; Graduation fee, \$25; Demonstrator's ticket, \$10.

Boarding, \$3 50 to \$4 00 per week.

ROBERT PETER, M. D.,

LEXINGTON, KY., July 16, 1855.

Dean.

UNIVERSITY OF LOUISIANA, MEDICAL DEPARTMENT.

The Annual Course of Lectures in this department will commence on Monday, November 19, 1855, and will terminate in the ensuing March.

JAMES JONES, M. D., Professor of Practice of Medicine.

J. L. RIDDELL, M. D., Professor of Chemistry.

WARREN STONE, M. D., Professor of Surgery.

A. H. CENAS, M. D., Professor of Obstetrics.

A. J. WEDDERBURN, M. D., Professor of Anatomy.

GUSTAVUS A. NOTT, M. D., Professor of Materia Medica.

THOMAS HUNT, M. D., Professor of Physiology and Pathology.

CORNELIUS BEARD, M. D., } Demonstrators of Anatomy.

SAMUEL CHOPPIN, M. D., }

The rooms for Dissecting will be open on the third Monday in October.

The Faculty are Visiting Physicians and Surgeons of the Charity Hospital, and attend this institution from November to April.

The Students accompany the Professors in their visits, and, free of expense, enjoy extraordinary practical advantages.

There are, during the Session, about eight hundred persons prescribed for daily.

In 1854, the number of patients was thirteen thousand one hundred and ninety-two.

THOMAS HUNT, M. D., *Dean.*

The Administrators of the Charity Hospital elect annually, in April, fourteen resident students, who are maintained by the institution.

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